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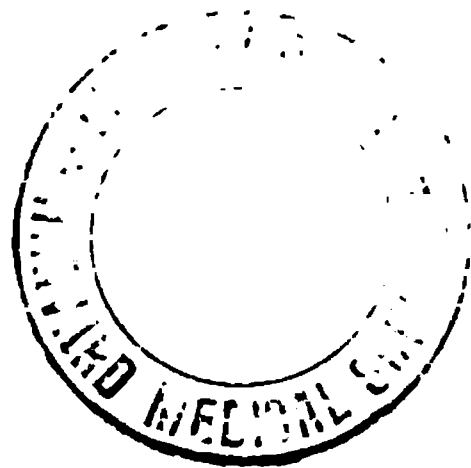




**THE**  
**BRITISH AND FOREIGN**  
**MEDICO-CHIRURGICAL**  
**REVIEW**

**OR**  
**QUARTERLY JOURNAL**  
**OF**  
**PRACTICAL MEDICINE AND SURGERY.**

**VOL. VII.**  
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# CONTENTS OF N° XIII

## OF THE

# BRITISH AND FOREIGN

# MEDICO-CHIRURGICAL REVIEW.

## JANUARY, 1851.

### Analytical and Critical Reviews.

	PAGE
<b>ART. I.—1. Report of the General Board of Health on the Epidemic Cholera of 1848 and 1849</b>	1
Appendix (A). Report of Dr. SUTHERLAND.	
Appendix (B). Report of Mr. GRAINGER.	
<b>2. Report of the Cholera in Boston (U.S.) in 1849. (Presented to the Health Commissioners in Boston)</b>	ib.
<b>3. Statistics of Cholera; with the Sanitary Measures adopted by the Board of Health, prior to and during the prevalence of the Epidemic in Philadelphia (U.S.), in the Summer of 1849. (Prepared by the Sanitary Committee, approved by the Board, and ordered for Publication)</b>	ib.
<b>4. Statistics of Cholera. By Assistant-Surgeon EDWARD BALFOUR, of the Madras Army. (Printed by Order of the Governor in Council)</b>	ib.
<b>5. Report on the Epidemic Cholera, as it has appeared in the Territories subject to the Presidency of Fort St. George. Drawn up, by the Order of Government, by WILLIAM SCOT. Abridged from the original Report, printed at Madras in 1824, with Introductory Remarks by the Author</b>	ib.
<b>6. Cholera and its Cures. By J. S. BUSHNAN, M.D.</b>	ib.
<b>7. The Phenomena of Pestilential Cholera, &amp;c. By G. M'CULLOCH, M.D., and A. C. MACLAREN</b>	ib.
<b>8. Reports of the Origin and Progress of Pestilential Cholera in the West Yorkshire Lunatic Asylum. By T. G. WRIGHT, M.D.</b>	
<b>9. Die Asiatische Cholera und die Gesundheitspflege. Von Dr. C. F. RIECKE</b>	2
<b>ART. II.—1. Traité Pratique de la Colique de Plomb. Par J. L. BRACHET</b>	41
<b>ART. III.—1. Alcoholismus Chronicus eller Chronisk Alkoholssjukdom ett bidrag, &amp;c. &amp;c.</b>	46
<b>2. Voorlezing over den Invloed van Sterken Drank op het Ligchaam. Voorgedragen in het Natuurkundig Gezelschap te Utrecht. Door J. L. C. SCHROEDER VAN DER KOLK</b>	ib.
<b>3. The Physiological Effects of Alcoholic Drinks, from the 'British and Foreign Medical Review;' with Documents and Records of the Massachusetts Temperance Society, illustrating the Origin of the Temperance Reformation, and its Progress in the State of Massachusetts</b>	ib.
<b>4. On the Use and Abuse of Alcoholic Liquors in Health and Disease. Prize Essay. By WILLIAM B. CARPENTER, M.D. F.R.S. F.G.S. &amp;c. &amp;c.</b>	ib.
<b>5. Temperance and Total Abstinence; or the Use and Abuse of Alcoholic Liquors in Health and Disease. By SPENCER THOMSON, M.D., &amp;c. &amp;c.</b>	ib.
<b>6. Report of the Committee appointed to inquire into the Expediency of diminishing the present Quantity of Spirits, served out daily to the Seamen of the Royal Navy. (Parliamentary Paper)</b>	ib.
<b>ART. IV.—The Principles of Surgery. By JAMES MILLER, F.R.S.E. F.R.C.S.E., Professor of Surgery in the University of Edinburgh, Senior Surgeon to the Royal Infirmary, &amp;c. &amp;c. Second Edition. Illustrated by 238 Engravings on Wood</b>	84
<b>ART. V.—Iodognosie, ou Monographie Chimique, Médicale et Pharmaceutique, des Iodiques en général, et en particulier de l'Iode et de l'Iodide de Potassium. Par F. DORVAULT</b>	91

	PAGE
ART. VI.—1. Delle Malattie e delle Operazioni della Ghiandola Tiroidea. Di LUIGI PORTA, Professore di Clinica Chirurgica in Pavia. Con quattro Tavole in Rame . . . . .	96
2. The Cyclopædia of Anatomy and Physiology. Edited by ROBERT B. TODD, M.D. F.R.S., &c. &c.—Art. Thyroid Gland. By C. HANDFIELD JONES, M.D. . . . .	ib.
ART. VII.—1. Grundlage der Literatur der Pädiatrik, enthaltend die Monographien über die Kinderkrankheiten. Von FRIEDRICH LUDWIG MEISSNER, Doctor der Medecin, &c. &c. . . . .	107
2. Die Krankheiten der Neugeborenen und Säuglinge vom clinischen und pathologisch-anatomischen Standpunkte bearbeitet. Von ALOIS BEDNAR, Doctor der Medecin prov. Primararzte des k. k. Findelhauses in Wien, &c. &c. Krankheiten des Ernährungs-Kanals bei Neugeborenen und Säuglinge. Erster Theil . . . . .	ib.
ART. VIII.—The Anatomy, Physiology, and Pathology of the Eye. By HENRY HOWARD, M.R.C.S.L., Surgeon to the Montreal Eye and Ear Institution . . . . .	115
ART. IX.—1. Lectures on Inflammation, delivered in the Theatre of the Royal College of Surgeons of England. By JAMES PAGET, Professor of Anatomy and Surgery to the College . . . . .	127
2. On the State of the Blood and the Blood-Vessels in Inflammation, ascertained by Experiments, Injections, and Observations by the Microscope; being the "Astley Cooper Prize Essay" for 1850. By T. WHARTON JONES, F.R.S. (From the 'Guy's Hospital Reports.') . . . . .	ib.
3. General Pathology, as conducive to the Establishment of Rational Principles for the Diagnosis and Treatment of Disease; a Course of Lectures, delivered at St. Thomas's Hospital during the Summer Session of 1850. By JOHN SIMON, F.R.S., one of the Surgical Staff of that Hospital, and Officer of Health to the City of London . . . . .	ib.
4. The Principles of Surgery. By JAMES MILLER, F.R.S.E. F.R.C.S.E., &c. &c. Second Edition . . . . .	ib.
ART. X.—1. The Nature and Cure of Consumption, Indigestion, Scrofula, and Nervous Affections. By G. CALVERT HOLLAND, M.D., lately Physician to the Sheffield General Infirmary, &c. . . . .	167
2. Cases illustrative of the Cure of Consumption and Indigestion. By G. C. HOLLAND, M.D., &c. . . . .	ib.
3. Practical Suggestions for the Prevention of Consumption. By G. C. HOLLAND, M.D., &c. . . . .	ib.
4. A Practical Synopsis of Diseases of the Chest and Air-Passages; with a Review of the several Climates recommended in those Affections. By JAMES BRIGHT, M.D. . . . .	ib.
5. An Inquiry, how far Consumption is Curable; with Observations on the Treatment, and on the Use of Cod-liver Oil and other Remedies: with Cases. By JAMES TURNBULL, M.D., Physician to the Liverpool Infirmary, formerly Physician to the Northern Hospital, and Lecturer on Clinical Medicine at the Infirmary . . . . .	ib.
ART. XI.—Pathological and Surgical Observations on the Diseases of the Joints. By Sir BENJAMIN C. BRODIE, Bart, V.P.R.S., &c. &c. Fifth Edition, with Alterations and Additions . . . . .	177
ART. XII.—1. Of the Causes, Nature, and Treatment of Palsy and Apoplexy; of the Forms, Seats, Complications, and Morbid Relations of Paralytic and Apoplectic Diseases. By JAMES COPLAND, M.D. F.R.S., &c. &c. . . . .	189
2. The Causes, Symptoms, and Treatment of Eccentric Nervous Affections. By WILLIAM JOHN ANDERSON, F.R.C.S. . . . .	ib.
3. Traité Pratique des Maladies Nerveuses. Par C. M. S. SANDRAS, Agrégé de la Faculté de Médecine de Paris, Médecin de l'Hôpital Beaujon, &c. . . . .	ib.
4. i. The Lumleian Lectures for 1850.—On the Pathology and Treatment of Delirium and Coma . . . . .	ib.
ii. Lectures on the Treatment of Delirium and Coma (in Sequel to the Lumleian Lectures for 1850). By ROBERT B. TODD, M.D. F.R.S., &c. ('Medical Gazette' and 'Medical Times,' 1850.) . . . . .	ib.



	PAGE
ART. XIII.—A Theoretical and Practical Treatise on Human Parturition. By H. MILLAR, M.D., Professor of Obstetrics and the Diseases of Women and Children in the Medical Department of the University of Louisville	211

## Bibliographical Notices.

ART. I.—1. The Profession of Medicine, its Study and Practice; its Duties and Rewards. An Address, delivered at St. Bartholomew's Hospital on the Opening of the Medical Session of 1850-51. By CHARLES WEST, M.D. F.R.C.P., &c. &c.	231
2. The Studies of the Medical Man; being the Introductory Address, delivered at the Theatre of Anatomy and Medicine adjoining St. George's Hospital, at the opening of the Session of 1850-51. By EDWIN LANKESTER, M.D. F.R.S. F.L.S., &c. &c.	ib.
3. An Introductory Address, delivered at the London Hospital Medical School at the Opening of the Session 1850-51. By NATHANIEL WARD, F.R.C.S., &c.	ib.
4. Address to the Medical Students of London, Session 1850-51. By J. STEVENSON BUSHNAN, M.D. M.R.C.S., &c.	ib.
5. Our Institution and its Studies. An Introductory Lecture, delivered at the Bristol Institution for the Advancement of Science, Literature, and the Arts, on Monday, Sept. 23, 1850. By JOHN ADDINGTON SYMONDS, M.D.	ib.
6. Happiness in its Relations to Work and Knowledge. An Introductory Lecture, delivered before the Members of the Chichester Literary Society and Mechanics' Institute, Oct. 25, 1850; and published at their request. By JOHN FORBES, M.D. F.R.S., &c.	ib.
ART. II.—Elements of Chemistry; including the Applications of the Science in the Arts. By THOMAS GRAHAM, F.R.S., Corresponding Member of the Institute of France, Professor of Chemistry in University College, London, &c. &c. Second Edition, entirely revised and greatly enlarged. Part IV.	240
ART. III.—A Manual of Dissection. By LUTHER HOLDEN. Part III.	241
ART. IV.—1. Pathology of the Human Eye. By JOHN DALRYMPLE, F.R.C.S. Part VI.	242
2. Surgical Anatomy. By JOSEPH MACLISE, F.R.C.S. Fasciculus VII.	ib.
ART. V.—De Colica Scortorum Disquisitio. Autore MARTINO HASSING, Dr. Med., Medico Secundario Nosocomii Communis et Legionis Civilis/Hauniensis, Regiæ Societatis Medicæ Hauniensis Locio	245
ART. VI.—Conspectus Medicinæ Theoreticæ, Capita Quadraginta tria universa complectens, ad Usus Academicum. Auctore JACOBO GREGORY, M.D. Editio Undecima, omni parte perfecta, prioribus emendatior	246
ART. VII.—1. Report by the General Board of Health on the Supply of Water to the Metropolis	247
2. Further Examination of the "Gathering Grounds" for the proposed Government Water Supply to the Metropolis, as contained in a Report to the General Board of Health, Gwydyr House, Whitehall. By the Hon. WILLIAM NAPIER	ib.
ART. VIII.—A Practical Treatise on the Therapeutic Uses of Terebinthinate Medicines; with Observations on Tubercular Consumption, Gout, Mineral Waters, &c. By THOMAS SMITH, M.D., late Physician to the Cheltenham General Hospital and Dispensary, &c. &c.	248
ART. IX.—Gout; its Causes, Cure, and Prevention, by an Original and most Successful Treatment, founded on the Organic Changes in the Human Solids, and on the Functions of the Skin; without the Use of Colchicum. By ABRAHAM TOULMIN, M.D. M.R.C.S.	249
ART. X.—1. The Physicians', Surgeons', and General Practitioners' Visiting List, Diary, Almanac, and Book of Engagements for 1851	ib.
2. A Medical Almanack and Companion to the Visiting List for 1851	ib.
ART. XI.—The Surgeon's Vade-Mecum. By ROBERT DRUITT, F.R.C.S. Fifth Edition, much improved. With 175 Wood-engravings	250

## Periscope.

	PAGE
On the Functions of the Liver. By M. BERNARD . . . . .	251
On the Shape of the Thyroid Foramen of the Os Innominatum in Male and Female. By Dr. NEILL . . . . .	254
Electro-Physiological Researches. Seventh Series. By Signor C. MATTEUCCI . . . . .	ib.
Observations on the Freezing of the Albumen of Eggs. By JAMES PAGET, Esq. . . . .	256
On the Structure and Use of the Ligamentum rotundum Uteri, with some observa- tions upon the change which takes place in the Structure of the Uterus during Utero-gestation. By G. RAINEY, Esq., M.R.C.S.E. . . . .	ib.
On Elastic Fibres found in the Sputa of Phthisis. By Professor SCHROEDER VAN DER KOLK . . . . .	257
On the Treatment of Itch. By MM. BAZIN and BOURGUIGNON . . . . .	258
Disease of the Heart and Chorea . . . . .	ib.
On Lupus. By M. CAZENAVE . . . . .	ib.
On a Species of Atrophy of the Spinal Marrow. By Dr. NAMIAS . . . . .	259
Case of Ossification and Obliteration of the Vena Cava Ascendens. By Dr. COLLINS . . . . .	260
On Cyanosis from Transposition of the Orifices of the Aorta and Pulmonary Artery. By Professor JOHNSON . . . . .	ib.
On Morbid Changes in the Papillæ of the Tongue. By Dr. DUBINI . . . . .	261
Results of Revaccination in the Prussian Army, during 1849 . . . . .	262
On Vaccination and Revaccination. By M. CRANINX . . . . .	263
Cases of the Termination of Acute Rheumatism in Suppuration. By MM. FLEURY, ANDRAL, and TROUSSEAU . . . . .	264
On the Treatment of Insanity. By M. MOREL . . . . .	265
On Hydrocele of the Neck. By Professor MUTTER . . . . .	266
Operation for Ingrowing Toe-nail. By M. BAUDENS . . . . .	267
Cholesterine in Sparkling Synchysis . . . . .	ib.
Case of Hernia of the Bladder. By Dr. MARKOE . . . . .	ib.
Spontaneous Cure of Spina Bifida by Rupture of the Sac. By Dr. NANCE . . . . .	268
On a Fatal Case of Ligature of the Internal Iliac. By Dr. KIMBALL . . . . .	ib.
Case of Fatal Peritonitis from a Perforating Bubo. By Dr. LENTE . . . . .	ib.
On a Case of Dislocation of the Fifth Cervical Vertebra, with Congenital Fusion of the Sixth and Seventh. By Dr. LENTE . . . . .	269
Case of a Wound of the Heart, with a commencement of Cicatrization. By Dr. TRUGIEN . . . . .	ib.
Case of Luxation of the Penis . . . . .	ib.
On Subungual Exostosis. By M. LEGOUPIL . . . . .	270
Extraordinary Case of the Entrance of Fragments of Glass into the Orbit. By M. COLLETTE . . . . .	271
On Fatty Degeneration of the Placenta. By Professor KILIAN . . . . .	272
Cases of Fatal Extra-Uterine Pregnancy. By Professor JOHNSON and Dr. DENNY . . . . .	273
On Lactation. By M. TROUSSEAU . . . . .	274
On the Diminution in Weight of New-Born Infants. By Dr. HOFFMAN . . . . .	276
On Auscultation and the Pneumonia of Infants. By MM. TROUSSEAU and LASÈGUE . . . . .	277
Additional Observations upon the Tartrate of Potass and Iron. By M. MIALHE . . . . .	278
On the Sulphuret of Soda as a Depilatory. By M. BOUDET . . . . .	279
Abortive Power of Collodion on Smallpox . . . . .	ib.
Medico-Legal Researches on Dried Cerebral Substance. By M. ORFILA . . . . .	280
On the Elimination of Arsenic in Arsenical Cachexia. By M. HANNON . . . . .	281
On Poisoning by Oxide of Zinc. By MM. FLANDIN and BOUVIER . . . . .	282
On the Health of Workmen employed on Copper. By MM. CHEVALIER and BOYS DE LOURY . . . . .	282
 BOOKS RECEIVED FOR REVIEW . . . . .	 283

**CONTENTS OF N° XIV**  
**OF THE**  
**BRITISH AND FOREIGN**  
**MEDICO-CHIRURGICAL REVIEW.**  
**APRIL, 1851.**

---

**Analytical and Critical Reviews.**

	PAGE
<b>ART. I.—1. Operative Surgery. By FREDERICK C. SKEY, F.R.S.</b>	285
2. Beiträge zur praktischen Chirurgie. Nebst einem Bericht über die chirurgisch-äugenärztliche Klinik der Königlichen Universität zu Halle. Herausgegeben von dem Direktor der genannten Anstalt, Dr. ERNST BLASIUS, Professor der Chirurgie	ib.
3. Contributions to Practical Surgery; with an Account of the Clinique of Ophthalmic Surgery at the Royal University of Halle. By Dr. E. BLASIUS, Director of the Clinique, and Professor of Surgery	ib.
<b>ART. II.—A Systematic Treatise, Historical, Etiological, and Practical, on the Principal Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian, and Esquimaux Varieties of its Population. By DANIEL DRAKE, M.D.</b>	302
<b>ART. III.—1. Compendium de Médecine Pratique, ou Exposé Analytique et Raisonné des travaux contenus dans les principaux Traités de Pathologie Interne. Par M. LOUIS DE LA BERGE, Docteur en Médecine, Agrégé à la Faculté de Médecine de Paris, Chef de Clinique Médicale à la même Faculté; M. ED. MONNERET, Agrégé à la Faculté de Médecine de Paris, Médecin du Bureau Central des Hôpitaux; et M. LOUIS FLEURY, Agrégé à la Faculté de Médecine de Paris, Membre Correspondant de l'Académie Royale de Médecine de Belgique. Ouvrage Autorisé par le Conseil Royal de l'Instruction Publique et par le Conseil de Santé des Armées de Terre</b>	333
2. Guide du Médecin Praticien, ou Résumé Général de Pathologie Interne et de Therapeutique Appliquées. Par F. L. I. VALLEIX, Médecin des Hôpitaux de Paris, Membre Titulaire de la Société Médicale d'Observation et de la Société Anatomique, Auteur de la 'Clinique des Maladies des Enfants Nouveau-nés,' du 'Traité des Névralgies,' &c.	ib.
3. Handbuch der medicinischen Klinik. Verfasst von Dr. CARL CANSTATT, königlich-bayerischem Gerichtsärzte und Mitgliede mehrerer gelehrten Gesellschaften. Zweite vermehrte Auflage	ib.
4. Handbuch der Pathologie und Therapie. Von Dr. C. A. WUNDERLICH, Professor der Medecin, Vorstand der medicinischen Klinik zu Tübingen. Dritter Band	ib.
<b>ART. IV.—A Treatise on the Etiology, Pathology, and Treatment of Congenital Dislocations of the Head of the Femur. Illustrated with Plates. By JOHN MURRAY CARNOCHAN, M.D., Lecturer on Operative Surgery, &amp;c. &amp;c.</b>	358

	PAGE
ART. V.—Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XXXIII (Second Series, Vol. XV)	362
ART. VI.—A Theoretical and Practical Treatise on Human Parturition. By H. MILLAR, M.D., Professor of Obstetrics and the Diseases of Women and Children in the Medical Department of the University of Louisville	393
ART. VII.—Du Mode d'Action des Eaux Minérales de Vichy, et de leurs Applications Thérapeutiques. Par CHARLES PETIT	415
On the Mode of Action and the Therapeutical Applications of the Vichy Waters. By CHARLES PETIT.	
ART. VIII.—Iagltagelser anstillede under Maeslinge-Epidemien paa Faeroerne i Aaret 1846. Af Cand. Med. & Chir. P. L. PANUM	419
Observations on the Epidemic of Measles in the Færoe Isles, in 1846. By P. L. PANUM, Candidate of Medicine and Surgery. (From the 'Bibliothek for Lægr.')	
ART. IX.—1. A Treatise on Baths; including Cold, Sea, Warm, Hot, Vapour, Gas, and Sand Baths: also, on the Watery Regimen, Hydropathy, and Pulmonary Inhalation; with a Description of Bathing in Ancient and Modern Times. By JOHN BELL, M.D., Member of the American Medical Association, and of the Pennsylvania Medical Society; Fellow of the College of Physicians of Philadelphia, &c. &c.	429
2. Researches into the Effects of Cold Water upon the Healthy Body, to Illustrate its Action in Disease; in a Series of Experiments, performed by the Author upon himself and others. By HOWARD F. JOHNSON, M.D., Physician of the "Ferns" Hydropathic Establishment, &c.	ib.
3. The Bath Waters: their Uses and Effects in the Cure and Relief of various Diseases. By JAMES TUNSTALL, M.D., Physician to the Eastern Dispensary of Bath, &c.	ib.
4. The Baths of Rhenish Germany; with Notices of the adjacent Towns. By EDWIN LEE, &c. &c. &c.	ib.
ART. X.—On the Therapeutical Application of Electro-Magnetism in the Treatment of Rheumatic and Paralytic Affections. By ROBERT FROBIEP, Doctor in Medicine and Surgery, Public Professor of General Surgery in the University of Berlin, &c. &c. Translated from the German by RICHARD MOORE LAWRENCE, M.D., Surgeon Extraordinary to H.R.H. the Duke of Saxe Coburg and Gotha	455
ART. XI.—1. On Animal Chemistry, in its Application to Stomach and Renal Diseases. By H. BENCE JONES, M.D. A.M. F.R.S. &c.	460
2. Elements of Urinary Analysis and Diagnosis, Chemical and Microscopical. By ROBERT VENABLES, A.M. M.B. &c.	ib.
ART. XII.—1. Lectures on Inflammation, delivered in the Theatre of the Royal College of Surgeons of England. By JAMES PAGET, Professor of Anatomy and Surgery to the College. (From the 'London Medical Gazette.')	472
2. General Pathology, as conducive to the Establishment of Rational Principles for the Diagnosis and Treatment of Disease; a Course of Lectures, delivered at St. Thomas's Hospital during the Summer Session of 1850. By JOHN SIMON, F.R.S., one of the Surgical Staff of that Hospital, and Officer of Health to the City of London	ib.
ART. XIII.—1. The Pathology of the Kidney in Scarlatina. Illustrated by Cases. By JAMES MILLAR, M.D., Physician to the Western General Dispensary	498
2. On Scarlatina. By J. W. TRIPE, M.D. (From the 'Medical Times,' 1848-9.)	ib.
ART. XIV.—On a New and Successful Treatment of Febrile and other Diseases, through the Medium of the Cutaneous Surface. Illustrated with Cases. By WILLIAM TAYLOR, Surgeon to the Clerkenwell Infirmary, and to the Police	513

## Bibliographical Notices.

	PAGE
<b>ART. I.</b> —On Excision of the Enlarged Tonsil, and its Consequences in Cases of Deafness. With Remarks on Diseases of the Throat. By <b>WILLIAM HARVEY</b> , Surgeon to the Royal Dispensary for Diseases of the Ear . . . . .	517
<b>ART. II.</b> —The Surgical Anatomy of the Principal Regions of the Body. By <b>THOMAS MORTON</b> , late F.R.C.S., and Assistant-Surgeon to University College Hospital; and <b>WILLIAM CADGE</b> , F.R.C.S. and Assistant-Surgeon to University College Hospital . . . . .	518
<b>ART. III.</b> —1. Physician and Patient; or a Practical View of the Mutual Duties, Relations, and Interests of the Medical Profession and the Community. By <b>WORTHINGTON HOOKER</b> , M.D. . . . .	ib.
2. Lessons from the History of Medical Delusions; being the Fiske Fund Prize Dissertation of the Rhode Island Medical Society. By <b>WORTHINGTON HOOKER</b> , M.D. . . . .	ib.
<b>ART. IV.</b> —Deafness practically Illustrated: being an Exposition of Original Views as to the Nature, Causes, and Treatment of Diseases of the Ear. By <b>JAMES YEARSLEY</b> , Surgeon to the Metropolitan Eye and Ear Infirmary . . . . .	520
<b>ART. V.</b> —Thirty-seventh Annual Report of the Dispensary for the Cure of Complaints of the Eyes, Frogmore Street, Bristol . . . . .	ib.
<b>ART. VI.</b> —Additional Observations on the Nitrate of Silver; with full Directions for its Use as a Therapeutic Agent. By <b>JOHN HIGGINBOTTOM</b> , F.R.C.S. . . . .	521
<b>ART. VII.</b> —The Scale of Medicines with which Merchant Vessels are to be furnished, by Command of the Privy Council for Trade; with Observations on the Means of Preserving the Health and Increasing the Comforts of Merchant Seamen; and Directions for the Use of the Medicines, and for the Treatment of various Accidents and Diseases. By <b>T. SPENCER WELLS</b> , F.R.C.S., Surgeon, R.N. . . . .	522
<b>ART. VIII.</b> —Surgical Anatomy. By <b>JOSEPH MACLISE</b> , F.R.C.S. Fasculus IX . . . . .	523
<b>ART. IX.</b> —Corpulence, or Excess of Fat in the Human Body; its Relations to Chemistry and Physiology, its Bearings on other Diseases and the Value of Human Life, and its Indications of Treatment. With an Appendix on Emaciation. By <b>THOMAS KING CHAMBERS</b> , D.M., Fellow of the Royal College of Physicians . . . . .	524
<b>ART. X.</b> —Annual Report of the Progress of Chemistry, and the Allied Sciences, Physics, Mineralogy, and Geology; including the Application of Chemistry to Pharmacy, Medicine, Agriculture, the Arts and Manufactures. By <b>JUSTUS LIEBIG</b> , M.D., Professor of Chemistry in the University of Giessen, and <b>H. KOPP</b> , Professor of Physics in the University of Giessen. Edited by <b>A. W. HOFFMAN</b> , PH.D. F.C.S., Professor in the Royal College of Chemistry, London; and <b>H. BENCKE JONES</b> , M.D. F.R.S., Physician to St. George's Hospital. Part I for 1849 . . . . .	525
<b>ART. XI.</b> —The Moral and Sanitary Aspects of the New Central Cattle Market, as proposed by the Corporation of the City of London. By <b>J. STEVENSON BUSHNAN</b> , M.D. . . . .	526

## Periscope.

	PAGE
On the Absorption of Sugar and Albumen by the Vena Portæ. By M. BERNARD .	527
Affection of the Olfactory Nerve, with total Loss of Smell and Taste : with Remarks upon the probable Identity of these two Senses. By Dr. KNEELAND .	528
On the Functions of the Chorda Tympani Nerve. By M. DUCHENNE .	ib.
On so-called Chylous Urine. By H. BENCKE JONES, M.D. A.M. F.R.S., &c. .	529
On the Iodine of Fresh-Water Plants and Animals. By M. CHATIN .	530
Observations on the Curare (Woorara) Poison. By MM. BERNARD and PELOUZE .	531
Absorption of Inorganic Poisons by Plants .	532
Remarks on the Cooking and Preserving of Meat. By PROFESSOR LIEBIG .	ib.
On Coup de Soleil. By Drs. PEPPER and CONDIE .	534
On the Diseases from Lead in Paris. By M. CHEVALLIER .	ib.
On the Animal Temperature in Intermittent Fever. By M. GIRBAL .	535
On the Treatment of Lead Colic by the Internal and Topical Employment of Chloroform. By M. ARAN .	ib.
On Nervous Affections. By M. CERISE .	ib.
Case of Gangrene of the Vermiform Appendix. By Dr. STORER .	537
Erysipelas after Vaccination .	ib.
On Progressive Muscular Atrophy. By M. ARAN .	538
Observations on Ozone. By MM. POLLI, HEIDENREICH, and FABER .	540
On the Treatment of Sciatica by Galvanism and the Cautery. By MM. DUCHENNE and VALLÉIX .	541
On the Treatment of Ophthalmia in General. By Professor LANGENBECK .	543
On Subconjunctival Dislocation of the Crystalline Lens. By M. BARRIER .	545
On Acupuncture in Varix. By Professor RIZZOLI .	ib.
On the Induction of Premature Labour. By Dr. LEHMANN, of Amsterdam .	546
On Oophoritis. By Dr. PISTOCCHI .	ib.
On the Application of Collodion to Inflamed Breasts. By Professor EVANS .	547
Case of Early Viability of an Infant .	ib.
Case of Quintuple Birth of Living Children. By Dr. SERLO .	ib.
Abnormal Relation of the Clitoris and Urethra .	548
Volatile Oils. Quantities yielded by Plants .	ib.
On the Destruction of Leeches by the Oniscus aquaticus. By M. LEON SOUBERAIN .	549
Iodine in Hospital Gangrene .	ib.
Professor Mettauer's Aperient Solution .	ib.
Substitutes for Quinine .	550
Fatal Case of Poisoning by Bromine. By Dr. SNELL .	551
On a Case of Fatal Hæmorrhage from the Female External Genital Organs. By Dr. EVARD .	552
On the Duration of Life among the Clergy. By Dr. SCHNEIDER .	ib.
Poisoning by Peach Blossoms .	553
On Poisoning by Decoction of Rose-bay (Nerium oleander). By Dr. LANGLOIS .	ib.
Cases of Poisoning by Corrosive Sublimate .	ib.
On the Solidification of Foot-prints. By M. HUGOULIN .	554

---

Intelligence :—Letter of Dr. Blair, on the last Yellow Fever Epidemic of British Guiana .	ib.
Hospital for Sick Children .	561
Medical and Chirurgical Society .	ib.

---

BOOKS RECEIVED FOR REVIEW .	ib.
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PART FIRST.  
Analytical and Critical Reviews.

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ART. I.

1. *Report of the General Board of Health on the Epidemic Cholera of 1848 and 1849.*—8vo, pp. 158.  
*Appendix (A).* Report by Dr. SUTHERLAND.—8vo, pp. 164.  
*Appendix (B).* Report by Mr. GRAINGER.—8vo, pp. 200.
2. *Report of the Cholera in Boston (U.S.) in 1849.* (Presented to the Health Commissioners in Boston.)—8vo, pp. 180.
3. *Statistics of Cholera; with the Sanitary Measures adopted by the Board of Health, prior to and during the prevalence of the Epidemic in Philadelphia (U.S.), in the Summer of 1849.* (Prepared by the Sanitary Committee, approved by the Board, and ordered for Publication, Oct., 1849.)—pp. 70.
4. *Statistics of Cholera.* By Assistant-Surgeon EDWARD BALFOUR, of the Madras Army. (Printed by Order of the Governor in Council.)—*Madras*, 1849. 8vo, pp. 70.
5. *Report on the Epidemic Cholera, as it has appeared in the Territories subject to the Presidency of Fort St. George.* Drawn up, by the Order of Government, by WILLIAM SCOT. Abridged from the original Report, printed at Madras in 1824, with Introductory Remarks by the Author.—1849. 8vo, pp. 212.
6. *Cholera and its Cures.* By J. S. BUSHNAN, M.D.—1850. 8vo, pp. 169.
7. *The Phenomena of Pestilential Cholera, &c.* By G. M'CULLOCH, M.D., and A. C. MACLAREN.—1850. 8vo, pp. 123.
8. *Reports of the Origin and Progress of Pestilential Cholera in the West*



*Yorkshire Lunatic Asylum.* By T. G. WRIGHT, M.D.—1850. 8vo, pp. 136.

9. *Die Asiatische Cholera und die Gesundheitspflege.* Von Dr. C. F. RIECKE.—1850. 8vo, pp. 82.

IN our Number for last January, we intimated our intention, as soon as the official documents then in preparation were published, to examine at some length the very important question of the alleged connection between the development of Cholera and the virulence of its effects in certain localities, and the existence in these localities of known causes of insalubrity, which are, more or less, remediable, if not altogether removable. Eighteen months before that time, when the pestilence had just begun to rage at St. Petersburg, and ere the first droppings of the thunder-cloud were felt at Berlin, we had expressed our own decided conviction, derived from personal observation as well as from the recorded experience of others, that the terrible power of cholera, in the vast majority of instances, is traceable to accessory circumstances or conditions which attend its invasion, and that its worst ravages have very generally occurred in situations notoriously bad, in a sanitary point of view. Especial stress was laid on the exceeding danger, in the East Indies, of large masses of men, as in armies, being congregated in places where there is reason to suspect the germs of the disease, and of crowding many persons together in barracks, &c., without due regard to ventilation; and it was also shown that the danger was much increased if the health of the men was depressed from fatigue, as after prolonged marching, or if they were at the time exposed to the effluvia that arise from decaying animal and vegetable matters. Upon the same occasion we expressed our high approbation of the labours of the Metropolitan Sanitary Commissioners, and our general concurrence with the views propounded in their two Reports, then recently published. We also took the opportunity of stating our opinion of the immense benefit that would accrue to India and its people, if the principles so ably laid down in these Reports were carried out into practice, at the parent source of that disease which has spread desolation and dismay wherever it has appeared. These things we mention, that the reader may see that the subject to which his attention is now again invited, has been long and seriously under our consideration; and well may its magnitude and importance claim the earnest study of every medical man. The very circumstance of the late visitation being so much more fatal in our own land than its predecessor, must at once arrest the anxious thoughts of all. How is it that after the experience we had of the terrible enemy in 1832, and after all the labour and learning spent in its investigation, a far larger number of victims among our own people fell a sacrifice to its fury, when it returned in 1849? There are but two ways of accounting for this most painful truth;—either that the essential virulence of the poison was greater, or else that the conditions favouring its development and fatal effects were in more active operation. No one, as far as we know, has adduced any facts to warrant belief in the first of these opinions, while there is much that is incompatible with its adoption; whereas a very large mass of evidence has been brought forward in support of the latter. It is the object of the present article to consider and weigh this evidence, in the hope of arriving

at some definite conclusions on a subject not only of professional but of national interest. Our task is comparatively easy, from the abundance of details before us; the only difficulty lies in their selection and arrangement. It is one of mingled pain and pleasure;—of pain, from the extent of human wretchedness and loss of life which it discloses; of pleasure, from what appear to us to be satisfactory proofs of the control which it is permitted to man to exercise over the terrible enemy, or, at least, of his power to evade its fatal assaults, and, consequently, from the hope that much of that wretchedness may be prevented in future. May the endeavour be blessed to some profit;—and this it can scarcely fail to be, if it serve to fix, not merely to draw, the attention of the profession.

The Sanitary Commissioners in their Second Report, published in the spring of 1848, made the following predictive remark upon the state of the Metropolis, in reference to the then anticipated invasion of the epidemic:

“The improvements reported since 1832 have not been even commensurate with the increase of building and population; so that, in the present sanitary condition of the Metropolitan districts, not only is there no reasonable ground to suppose that were Cholera to reappear in them, it would be less extensive and mortal than on its former visitation; but, on the contrary, as the density of the population is in many parts increased, there must be a proportionate increase in the density of the deleterious agencies in operation.”

There had moreover been, during the preceding five years, a progressive increase, in London, of the whole class of zymotic diseases, amounting to an excess above the average of 31 per cent. The mortality from typhus had been steadily rising, and, in 1848, exceeded, by several hundred deaths, the mortality of any previous year;—an infallible indication of a most unfavorable state of the public health. The influenza in 1847 and 1848 was so severe, that almost as many at the earlier periods of life, and more in old age, perished by it, than by the more terrible epidemic that followed it. But the malady which all along continued its course with the most steady progress, was that which was the most nearly allied to the approaching pestilence, namely, diarrhœa. The deaths from this disease in the metropolis, during the five years ending 1848, amounted to 7580; whereas in the preceding five years, they were only 2828; while, taking separate years in this series, the deaths in 1848 were more than seven times greater than in 1839, and nearly five times greater than in 1841.

What is stated above of the sanitary condition of the metropolis, was, we need scarcely say, quite as applicable to most of our large towns, and to not a few of the smaller. Even had there been no increase of population, there must have been an augmented mass of putrescent refuse under and around the dwellings of the poor more especially; unless, indeed, some great improvement had taken place in the means for its removal. But it was well known that no such salutary change had been made, and the consequence was therefore obvious.

The foreboding so clearly indicated in the passage we have quoted, has been, as all know, too painfully realised. In the visitation of 1832-3, the number of persons attacked was (as reported to the Privy Council) rather more than 14,000, and the number of deaths was 6729; the population of London then being 1,681,641. In that of 1848-9, it is

estimated that the attacks were 30,000, and the deaths 14,601; the population at that time being 2,206,076. According to these statements, in the epidemic of 1832-3 one person died in every 250 of the inhabitants, or .4 per cent.; whereas in 1848-9, one person died in every 151 of the inhabitants, or .66 per cent. The mortality, therefore, on the first occasion, was about 2-5ths less than on the second; which is the same as to say that, in proportion to the population, about 5800 more persons perished of cholera in 1849 than in 1832.

In England and Wales in 1832-3, the attacks are stated to have been 71,606, and the deaths 16,437. In 1849, the total number of deaths in England and Wales from cholera amounted to 53,293,\* besides 18,887 from diarrhoea.

It is believed that between 7000 and 8000 persons were cut off in Scotland by the epidemic; but the lamentable absence of any registration there precludes anything like accuracy.

A like increase of mortality was experienced in Paris, as well as in Petersburg and some other cities on the Continent. But without dwelling just now upon this or upon any other topic connected with the history of the epidemic, we shall at once proceed to direct the attention of the reader to what appear to be the principal localising conditions and predisposing causes, which determine the manifestation of the disease in its virulent and fatal form. The first we shall notice is *Overcrowding*, whereby the air respired is rendered impure by the exhalations which proceed from living beings themselves.

I. And first, let us consider for a moment one or two points connected with Respiration. Of all the circumstances affecting health, none is so important as the condition of the air that we breathe. On it, more than upon the food we eat, depends the purity of the blood and the right exercise of every function of the body. Hence the especial care manifested for the maintenance of the respiratory process in all animals, alike by the admirable structure of the organs which subserve this purpose, and the beautiful arrangements and adaptations of the media in which they live, and the circumstances in which they are placed. In a state of nature, all goes on healthily and well. The same air is not breathed a second time until it has been rendered pure as before; for whenever the air has been brought into close proximity with the blood of a living animal, it immediately experiences a change which disqualifies it for healthy respiration. One fifth of the oxygen which it contained—we speak of the function in the human being—disappears, and its place is occupied with a nearly equal volume of carbonic acid. How, then, can the same portion of air be inhaled again without detriment? A determinate amount of oxygen is required, at each act of breathing, to enable the system to get rid of the carbonic acid, which requires to be discharged every

\* It is stated on indisputable authority, that in several places, more especially in some watering towns on the coast, whose prosperity is connected with the resort of visitors during the summer months, there was a much larger mortality from the epidemic than was made public. The clerk to the Board of Guardians at Yarmouth does not hesitate to state: "There were, I believe, a great many more cases than were known. I returned all cases that were sent to me to the Board of Health; but I believe that not one in ten was returned." Brighton and Hastings, not to mention some other places on the south coast, suffered, we were informed, much more severely than the public were permitted to know. Query. Is this kind of "*suppressio veri*," on the part of medical practitioners, consistent with their duty? We think not.

moment, and which, if not eliminated as nature intended, acts as a slow poison to the blood. If we take the ordinary computation of 20 cubic inches of air being drawn into the lungs at each inspiration, and of there being about 20 respirations in a minute, then 333 cubic feet, or 33 hogsheads, are made use of by each person *per diem*. Not less than between 10 and 12 cubic feet of carbonic acid are therefore evolved in the course of the twenty-four hours, a quantity which will be found to contain at least six ounces of solid carbon! The consideration of this fact alone must suffice to show the extreme importance of there being no impediment to the due exercise of the respiratory function, and prepares us at once for the pernicious results that inevitably attend upon the breathing of an atmosphere which does not enable the system to remove its self-generated poison. The retention or inadequate discharge of the carbonic acid,—besides the more direct effects of oppression, loss of appetite, and greatly impaired muscular energy,—renders the body more prone to every form of diseased action, and far less capable of resisting the influence of baneful agencies from without.

But the chemical change in its composition is not the only alteration which respired air has undergone. Besides the large amount of carbonic acid, it has become charged with a watery vapour, which of course renders the atmosphere of unventilated chambers unduly moist—a condition to which we shall afterwards have occasion to allude, as one of the causes favouring the development of epidemic disease. The quantity of fluid exhaled from the lungs, under circumstances of health, is believed to be at least from 16 to 20 ounces in the course of the four-and-twenty hours. Of course, if the process be imperfectly performed, as it must be liable to be in an atmosphere already loaded with moisture, nature must seek an outlet for the excretion at some other channel. And let it not be supposed that it is mere aqueous vapour that is thus discharged; there is effluvial animal matter mixed with or dissolved in it,—a matter which, upon being condensed, is found to be of a highly putrescent and rapidly decomposable nature. It is in fact one form of the waste, and therefore excrementitious, products of the system, which are being continually eliminated by the various emunctories of the lungs, skin, bowels, and kidneys; each of these organs having its special task to perform in the great process of excretion and purification, so indispensable to the maintenance of healthy life. That the atmosphere of ill-ventilated rooms, where a number of persons have been congregated for some time, is tainted with this animal effluvium, is obvious from the peculiar sickening smell perceived on entering a crowded dormitory in the morning before the doors and windows have been opened, or even a large school-room after children have been in it for several hours; unless, indeed, there has been—as ought always to be—a thorough and continual circulation of pure air. Medical men and others, after visiting the wretched abodes of the poor, often retain for several days in their clothes, the offensive smell of such localities;—how, then, must the bedding and furniture of such apartments, as well as the garments of the inmates, be impregnated with it! It clings to the very floors and walls of the rooms; and these, moreover, from the vicious construction of the windows—being seldom or ever made to open from the top—can scarcely be kept quite sweet,

even under the favorable circumstances of non-crowding and cleanliness. The pollution of the atmosphere from the cause now mentioned, viz. the existence of a noxious organic matter in it, is probably a more influential cause in generating certain forms of disease, and in powerfully predisposing to the invasion of epidemic disorders, than even the vitiation of it from an excess of carbonic acid. It is necessary, however, to keep both in view, as well as the increased amount of aqueous vapour, in estimating the baneful effects of deficient ventilation in the dwellings of the poor. But the lungs are absorbing as well as excreting organs; they are as ready to take in, as they are active in giving out. The blood is permeating every part of these with such rapidity, that the entire mass of it is believed to circulate through them, and to be consequently exposed to the air contained in their minutely-divided cells, in little more than three minutes. How favorable, then, is such an arrangement, coupled too with the exquisitely delicate fabric of the pulmonic tissue, to the imbibition or absorption of all volatile matters in the respired air, and their immediate transmission to every part of the body! There cannot surely be a reasonable doubt, that not only are the special morbid poisons generally received into the system in this way, but that also the gaseous products of animal and vegetable decomposition, and the effluvia given off from living bodies, are apt to be drawn into the circulation by the lungs, and serve to vitiate the blood—more or less rapidly and powerfully, according to age, vigour of constitution, and other accessory causes—and to deteriorate the health. The general effect of the absorption of such matters is to lower, in a very marked degree, the physical energies of the system, rendering it at the same time much more irritable and less enduring of fatigue; while the more special result is to induce a tendency to diarrhoea and other disorders of the bowels, a proneness to fever and inflammations of a typhoid type, and, as might be expected, a peculiar liability to all epidemic disorders.

It is, of course, an important point to determine what amount of space is required, under ordinary circumstances, for the healthy respiration of one person. Now, it has been the result of experience, the conclusion to which the most competent observers have come, — that unless extraordinary means are taken for the constant removal of the air by some special apparatus for ventilation, so that the impure air is carried off as quickly as it is exhaled from the lungs, health and strength cannot be maintained in a space of less than from 700 to 800 cubic feet; and that to live and sleep in a space of less than from 400 to 500 cubic feet for each individual, is not compatible with safety to life, even when there is no extrinsic or superadded cause of atmospheric impurity. And let it not be supposed that even the first-named spaces would be sufficient in an hermetically closed box or chamber; for life would become extinct long before all the oxygen had been consumed. Air containing only ten per cent. of carbonic acid is incapable of supporting life.

It is scarcely necessary to say, that the extent of space to each person ought of course to be greater during the prevalence of any epidemic or contagious disorder. Regard should also be paid to the nature of the locality, the conditions of the atmosphere, heat of climate, &c.; for what may be perfectly safe in a cold season, and in this country, may be quite



inadequate within the tropics, where not only the air is greatly more rarified, but also the exhalations from the skin and lungs are much more abundant. Yet a larger space is allowed to a prisoner in England than to a soldier in the East Indies;—1000 cubic feet being granted to the former, and only 800, we believe, to the latter! and even this insufficient space is often encroached upon. And now for facts illustrative of our subject.

One of the most striking illustrations of the effects of *overcrowding* on Choleraic invasion, is that afforded by what took place at Taunton in November, towards the close of the epidemic. The town, which contains about 16,000 inhabitants, although, like every other place in the kingdom, very faulty in its sanitary arrangements, is better than most; and, with the exception of a good deal of diarrhœa, had remained tolerably healthy. Suddenly, a violent attack of the pestilence took place among the inmates of the workhouse; diarrhœa had existed for some time among them. The first case of cholera occurred on the 3d of November; and in ten minutes from the time of seizure, the patient was in a state of hopeless collapse. Within the next forty-eight hours, no fewer than 42 cases and 19 deaths had taken place; and in the course of one short week, out of 276 inmates, 60, or nearly 22 per cent., were swept away, and nearly all the survivors suffered to a greater or less degree from cholera or diarrhœa. The greatest mortality occurred among the girls; for out of 67 one half were attacked, and as many as 25 died. Ten deaths occurred among the boys, and the rest among the older inmates. Now what was the condition of the workhouse? It is described by Dr. Sutherland as being low, badly drained, and most imperfectly ventilated; there were numerous nuisances within the walls; the people had insufficient space allowed them, and personal cleanliness was very much neglected. The medical officer had repeatedly called the attention of the Board of Guardians to the defective arrangements of the house, in consequence of the amount of disease among the inmates, but without effect. “It is very subject,” to use his own words, “to epidemics, to measles, scarlet fever, typhus, diarrhœa constant (especially in children), dysentery, scurvy.” He complained much of the closeness and offensiveness of the ward at night, and considered that the space allowed each inmate was not above two thirds of what was requisite for safety. So much for the general condition of the house. But how came it that the stress of the attack fell upon the girls’ school-room?—was there any peculiarity in its condition? Dr. Sutherland tells us that—

“The greatest degree of overcrowding existed in the girls’ school-room, which was a slated shed, 50 feet long, 9 feet 10 inches broad, and 7 feet 9 inches high to the top of the walls, over which was a sloping roof. In this miserable place were huddled together 67 children, with about 68 cubic feet of air to each.” (p. 11.)

And this, too, during a season of epidemic sickness, and when the premonitory disease was actually prevalent in the house! A curious circumstance, that well deserves notice, occurred with respect to the boys’ school. This apartment was even somewhat worse than that of the girls; but the boys, finding doubtless the air to be so close, could not be kept from breaking the windows. “Mr. Smith, the chaplain of the work-

house, stated his firm belief that it was to the better ventilation which the broken windows maintained, that the children in some measure owed their lives."

While the pestilence was raging in the workhouse, and the inhabitants of the town were suffering a good deal with bowel complaints, although no actual case of cholera occurred among them, it is a very remarkable and most instructive fact, that *not a solitary case even of diarrhœa occurred among the prisoners of the gaol.* The prisoners were much better cared for than the poor inmates of the union. Each cell contained from 800 to 900 cubic feet and upwards of air, besides being systematically ventilated and warmed, to maintain an even temperature throughout the twenty-four hours. Moreover, each prisoner had the means of personal cleanliness, and attention to this was strictly enforced throughout the building. The result was, that the health of the prisoners remained throughout perfectly good. "Opportunities rarely occur," remarks Dr. Sutherland, "such as those afforded by the instance before us, of testing the truth of the principles of preventive science."

The "'Footing case'" was a remarkable one in the history of the late epidemic; and the more so, as it occurred before the disease had fairly manifested itself over the metropolis, and indeed whilst its very presence was denied by some. Here was an establishment where no fewer than 1400 pauper children of different ages were lodged, and, as usually happens, without much attention to sanitary arrangements. The greatest evil was the insufficient space allowed to each child at night. The numbers crowded together in the dormitories were so great, that each boy had only 150 cubic feet, and each girl 133 cubic feet of air, allowed for respiration; and some of the apartments were at the same time so faultily constructed—there being windows on one side only—that no effective ventilation could possibly be kept up. And what was the result? At a time when no case of cholera had occurred in the neighbourhood, and when indeed even diarrhœa was not at all prevalent in the village, 300 of the inmates of this establishment were smitten with the secret pestilence, and of these no less than 180 died! The girls,—whose dormitories we have stated were the most overcrowded, and were the worst ventilated,—suffered more severely than the boys; although the former, as is usually the case, were altogether in better condition as to general health than the latter. Mr. Grainger, it will be remembered, made an elaborate report to the Board of Health on this remarkable case. The conclusion to which he came is expressed in the following words:

"After maturely considering all the circumstances connected with the painful occurrence, I am induced to express my firm conviction, that the essential cause of all the mischief has been the inordinate overcrowding of the establishment. Dangerous as I conceive the effluvia from the foul sewers and stagnant ditches to be, yet, diluted as they were by the winds, they might not alone have been sufficient to produce this outbreak, as may be presumed by the immunity up to this time of the inmates of other houses near these moats and ponds. But the state of overcrowding was such as must have lowered the health of the inmates in ordinary times, and was almost certain to produce disease under the depressing influence of an epidemic condition of the atmosphere. Without doubt, both the noxious effluvia and the overcrowding contributed to the calamitous result; but of these two causes I attach the utmost importance to the overcrowding, which, on any site, when carried to



such an extent as will be immediately shown to have prevailed in this establishment, must have lowered the health of the inmates and predisposed them to disease."

A somewhat similar case took place in another public institution at a very early period of the epidemic, when there was little disease in the neighbourhood, and at a different part of the environs of London, viz., Hackney. Out of 96 female inmates, 46 were attacked, and 15 died. After a careful investigation of all the circumstances connected with this unexpected attack, the only reasonable cause that could be assigned was an impure atmosphere in the dormitories, these being much crowded and most insufficiently ventilated.

We may remark, in passing, that in none of these three instances was there the slightest reason to suspect the introduction of the disease in the way of personal communication.

The evils of an impure condition of the atmosphere, from defective ventilation, were well shown in the case of the Millbank Penitentiary, which has also, be it remembered, the great disadvantage of a bad locality, besides certain structural mis-arrangements that necessarily impede a free circulation of air through its buildings. Previous to the appearance of cholera in England, there had been a great increase of disease generally, but especially of fever, among the male prisoners, the number of whom had been recently considerably augmented. Dr. Baly, the talented physician of the establishment, expressly regarded this—the increase of the numbers, and especially the accumulation of too large a number of prisoners in the long common rooms or wards of the prison—as the chief cause of the greater amount of sickness. After showing that the proportion of cases of fever at once abated by thinning the numbers, he states,—“There is every reason to believe, therefore, that the number of prisoners in the rooms and wards referred to, had exceeded the limit which is compatible with the health of the prisoners, and that the continued and increasing prevalence of fever in the summer and autumn of 1848 was due to this cause.” The same effect was observed when the cholera broke out in the prison. Millbank Penitentiary suffered far more severely from the pestilence than any other prison in the metropolis. Indeed, we shall afterwards see that most of them escaped almost entirely, or, at least, that no fatal cases occurred. But in the Penitentiary there were 113 attacks and 48 deaths, out of a population of about 1100. The subsequent very interesting statement is from Dr. Baly's report; it affords, we think, a most instructive illustration of the point under present inquiry:

“In the month of July, when the epidemic was becoming general and severe, it was deemed advisable to reduce the number of prisoners at Millbank, and on the 21st of that month a large body of male convicts were removed to Shorncliff Barracks; and in the latter part of August a further number was removed. Those who remained were distributed through the five different pentagons ordinarily occupied by male prisoners. The number of female convicts in the female pentagon remained the same as before. The comparative prevalence of cholera and the mortality from it, amongst the male and female convicts respectively, before and after the reduction in the number of the former class of prisoners, are shown in the subjoined table, together with the mortality at the same periods in London:

	June and July.	Aug. and Sept.
Number of deaths from cholera in London . .	2189 . .	10,896
Mortality per 1000 of population in London . .	0·9 . .	4·5
Number of female convicts in Millbank Prison . .	120 . .	131
Mortality per 1000 from cholera amongst the female convicts . .	8·3 . .	53·4
Cases per 1000 amongst the female convicts . .	16·6 . .	61·0
Number of male convicts in Millbank Prison . .	1039 . .	402
Mortality per 1000 from cholera amongst the male convicts . .	23·1 . .	9·9
Cases per 1000 amongst the male convicts . .	53·9 . .	37·3

“It will be seen that while in the metropolis generally the mortality from cholera was five-fold greater in August and September than in June and July, and while an equal increase took place in the mortality amongst the female convicts in the Millbank Prison, the mortality and the number of cases of the disease amongst the male convicts underwent an extraordinary diminution; and it cannot but be admitted as at least highly probable, that this favorable alteration was the result of the diminished number of the male prisoners in the parts of the establishment occupied by them.” (App. B, pp. 66-7.)

Mr. Grainger states, as the result of his observations on the workhouses of the metropolis, that the attacks of cholera were uniformly most numerous in those where the wards were most crowded and most defective in proper aëration. He particularly specifies the workhouse of Shoreditch, which suffered very severely; for among the inmates, as many as 109 cases (61 fatal), besides a large amount of diarrhoea, occurred during the prevalence of the epidemic. The sleeping apartments and other rooms in this establishment are notorious for being low, dark, and ill ventilated. The remark of the medical officer, Mr. Clark, strictly accords with the experience of others:—“I am convinced,” says he, “that wherever large numbers of human beings are congregated together, and who eat, drink, and sleep in the same apartment, as in the case with the young and old in workhouses (among which classes diarrhoea has in our house been most prevalent), there the inmates are most liable to suffer.”

But the crowding in workhouses and similar institutions never reaches that degree, which is unfortunately but very common in the dwellings of the poor, and especially in the low lodging-houses in our large towns. It is no unfrequent thing to find two, three, and four families living in one room, which does not exceed ten or twelve feet square. Occasionally the accumulation is much greater than this. In the report of the Whitechapel Association, just published, we read of 37 human beings, men, women, and children, being crammed into a room 10½ by 13 feet; the majority of them were nearly naked and very filthy. What a scene of horror! The inevitable pollution of the air under such circumstances is, of course, extreme; and it is only wonderful that life is not more frequently extinguished by suffocation. The amount of sickness, however, is truly dreadful. When a single case of febrile disease occurs in this corrupted and corrupting atmosphere,—not air, but putrid vapour, as some one has called it,—the morbid fermentation (zymosis) proceeds with extreme rapidity, and the ravages of death are often quite appalling, if the residents are not at once removed into a purer locality.

Some severe invasions of the disease took place in such places at an early period of the epidemic, while the adjacent neighbourhoods were as yet quite intact. Thus, in Jennings's Buildings, a low Irish nest in the healthy suburb of Kensington, there was a very fatal attack of the pestilence in the beginning of 1849; and about the same time, a similar occurrence took place in two of the lodging-houses of the Mint, Southwark. "The beds," says Mr. Grainger, "were placed in every possible direction, occupying not only the rooms but the landings on the stairs and passages; the people were huddled together two, three, or more in a single bed, children occupying the foot and the adults the top of the same bed. Some of the houses I examined professed to receive 100 lodgers, others 60. All this causes an oppressiveness and stench utterly insupportable to strangers," and inevitably productive of disease.

It is in such dens, that the Cholera made almost invariably the most dreadful devastations; and as actual narratives always leave a more lively and lasting impression than any general description, we shall give one instance out of many which might be quoted of such a catastrophe. It occurred at Plymouth, in a locality that has long been notorious for its being the abode of sickness and vice. The situation itself is by no means hemmed in, nor are the houses packed together as in the dense parts of our large towns; indeed, Quarry Court, where the greatest mortality occurred, is open in a measure to the fields. The dreadful evil was the overcrowding in rooms which had no means of ventilation. "One room, where 16 persons dwelt, was only 8 feet 4 inches square; while another, where 19 persons ate, drank, and slept, was only 11 feet by 9." In the former instance, each person had scarcely 40 cubic feet of air allowed for respiration! This is even worse than in the worst parts of that sink of misery and hotbed of disease, Church Lane, St. Giles, where—according to the report of the Statistical Society, published two or three years ago—the smallest space to one person was 52 cubic feet, the average being 175. The particulars are detailed by Dr. Roe, who was so actively and honorably engaged during the whole time that the epidemic continued in the town, and are contained in a series of excellent papers now publishing in the 'Medical Times:'

"On the night of July 9th the great outbreak in Stonehouse Lane took place, and between that time and the 17th there were 64 deaths in a circle, the diameter of which did not exceed 80 yards. The inhabitants of that dismal place were chiefly Irish, of the lowest and most degraded class, herded together like cattle, ragged, half naked, and inconceivably filthy; dead to all proper feeling, even the scenes around them made no good impression, for they stole the spirits and everything they could lay their hands on, even the blankets from off the dying. Situated as they were in small, close, and overcrowded rooms, it was impossible to afford the assistance their necessities demanded, and the difficulty was increased by the determination of the living to retain the dead, sometimes until long after decomposition had been established, for the sad but noisy obsequies of their country and religion. . . . Of one family, consisting of nine persons, four died; and in another house a whole family, root and branch, was swept away. The dead and the dying lay together in one small room, which was in so horrible a condition, that when the bodies were removed, the only way it could be cleansed was by introducing the hose of a fire-engine through the windows, and washing all down stairs . . . . The consternation among all classes, when the facts were made public, was such, that not only none in the town would receive those who desired to fly from the infected place, but when the attempt was made, the fugitives were, if discovered, driven

back to the scenes of death whence they had in vain endeavoured to escape. This feeling was manifested even in those parts of Stonehouse Lane which the cholera had not yet invaded, and where the proprietor of a lodging-house was threatened to have his house torn down about him if he did not turn out two poor creatures who had hastened from the pestilence, and sought shelter with him; reluctantly he complied; and 'I saw them,' says Dr. Roe, 'hooted back to the plague spot whence they came.'"

'These are the horrors that ensue under such circumstances; horrors which it is surely to the dishonour of this country, that they should ever be liable to recur among us at the present day.

The case related by Mr. Grainger of the hop-pickers at East Farleigh, on a farm on which about 1000 persons of all ages were employed, is in many respects similar to this one at Plymouth. As an example of the manner in which these people were lodged in sheds and outbuildings, Dr. Plomley of Maidstone informs us, that in a room containing 700 cubic feet 14 persons slept, so that each individual had for respiration about 50 cubic feet of air. Within four days of the first seizure, there had occurred upwards of 200 cases of diarrhoea, 97 of developed cholera, and 47 deaths. It is a curious fact, mentioned by Mr. Grainger, that on the same farm a severe attack of Asiatic cholera occurred in 1834, when 34 hop-pickers died, although the disease did not prevail elsewhere. Moreover, in 1849, on other farms in the immediate neighbourhood, where superior accommodation was provided for the labourers, there was comparatively little sickness, and scarcely any mortality.

Numerous and emphatic observations on the effects of overcrowding, in exciting a virulent development of the choleraic poison, might be quoted from the writings of our East India practitioners. Mr. Kellie, for example, the author of a most interesting paper published in Mr. Rogers's 'Reports,' noticed in our Number for January, 1849, says:

"The disease commits its greatest ravages in crowded ill-ventilated barracks, bazaars, densely-populated towns,—particularly such as are surrounded by walls preventing the ingress of pure air, and in that portion of them where carbonic acid gas (P) is by the decomposition of animal and vegetable refuse being evolved,—crowded schoolrooms, the inhabitants of a portion of a barrack in the vicinity of an open drain: native huts, into which there is but one opening, and that closed at night; whole families are frequently swept away from their exposure to this exciting cause."

Wherever large assemblages of people, for example, at native festivals or in crowded camps, are brought together in places where the seeds of the poison are known to be usually found, there is the disease apt to appear, though the district may have been quite healthy before:

"At Juggernaut it is an annual visitant. The town of Pooree contains 35,000 inhabitants, and the number of pilgrims sometimes amounts to 150,000. The inhabitants are usually quite healthy before the occurrence of the festival, which takes place in June or July; but immediately on the arrival of the pilgrims, and when the lodging-houses are literally crammed with inmates, cholera suddenly breaks out, and in the space of a few days hundreds are cut off by it. This is not an occasional or incidental occurrence, it is an invariable one; and the disease, which had thus been generated, as suddenly disappears on the dispersion of the crowd, a few isolated cases only occurring for two or three days after the departure of the pilgrims."

Similar testimony is adduced by Mr. Thom, in his account of the dreadful outburst of cholera at Kurrachee in the summer of 1846. We gave the substance of his important evidence on a former occasion, and to it

(No. III, p. 88), we beg to refer the reader for particulars. Just think of 10 or 12 men being cooped up in a tent not more than 14 feet square, exposed to a burning sun in a still sultry air from sunrise to sunset, and altogether deprived of the influence of the sea breeze. While the poor privates were swept down by hundreds, the officers nearly escaped; and not one out of 42 ladies was even so much as attacked, although no fewer than 23 out of 159 soldiers' wives perished! A dreadful proof, surely, of criminal neglect somewhere. Speaking of one of the regiments, Mr. Thom says: "I feel persuaded that this corps would have had very little sickness, had they been less crowded in barracks and their rooms ventilated by better arrangements, such as a medical board recommended at the moment the disease was breaking out."

Still more instructive is the record of the circumstances connected with the almost constant development of the disease at Fort Bellary, that grave of nearly every regiment that has been quartered in the barracks there during the last 30 years, although the pestilence is scarcely known two or three miles off. The case altogether is one of the most interesting and admonitory in the whole history of Cholera. The details are given at ample length in our Fifth Number, and deserve the most thoughtful attention of every one. We shall here only state the deliberate opinion of one who knows the place and its tragedies well, Dr. M'Gregor: "If the remediable causes of disease were removed, Bellary might become one of the most favorable stations for the health of H. M. troops." Is it not painful to read these few words?—that "if" involves an awful responsibility. The stewardship of that magnificent country has been committed to Britain for some wise purpose; and for the welfare of its inhabitants, as well as for that of her own people there, she will be made accountable. Is it not a remarkable circumstance, that a disease which was formerly almost confined to India, and but unfrequent there, has within the last thirty years not only assumed the character of an abiding scourge to the land of its nativity, but has twice girdled the globe in its unopposable march of destruction? Has this been fortuitous?

It is well known, that severe and very fatal outbreaks of cholera have repeatedly occurred on board ship, more especially in emigrant or troop vessels. The only case of this sort mentioned in the Report of the Board of Health, is that of the 'American Eagle,' which left London on the 31st of May, 1849, with emigrants, &c., for New York. She was of about 1000 tons burthen, and the whole number of souls aboard was 370. Some of the passengers had doubtless the germs of the disease in them when they started; for on the day following a man was attacked very suddenly, and died in the course of 12 hours. During the next eight days, before she arrived at Plymouth, 21 cases and 13 deaths occurred among the steerage passengers, besides numerous cases of diarrhoea. Measures were immediately taken for the removal of all the emigrants and others from the ship, and the disease was speedily arrested. Dr. Sutherland has made an interesting report on this case. His views will be seen from the following extract:

"The total number of passengers accommodated in the steerage was 250—a number considerably below the proportion admitted by the regulation, which is, I believe, one passenger to fourteen feet superficial area: but, after all this is admitted, it will be seen that the cubic space only amounted to about 173 feet for



each, including baggage. There can, in my opinion, be no question as to the defect of ventilation on board this vessel. It is quite true that, in this respect, she is better than the great proportion of our own emigrant ships; but to a practised eye the whole amount of air which it is possible to supply to such a great number of inmates, especially during the night, must appear wholly inadequate for the ordinary purposes of ventilation. I have seen no conditions on shore so defective in this matter as those presented by the ship in question, and yet the results on shore are sooner appreciated than they are on board ship. The fresh breeze to which the passengers are exposed during the day on deck, is evidently their only safeguard from certain destruction." (App. A, p. 144.)

He adds :

"Under ordinary circumstances there was nothing that would have produced more than an ordinary amount of sickness, but quite enough during an epidemic to determine its localization." (Ibid.)

On the very important and hitherto insufficiently-appreciated question of ventilating ships, Dr. Sutherland remarks that this can never be adequately effected by open hatches, windsails, or ventilating tubes, and that it is only with the aid of a *moving power*,—as, for example, one of Dr. Arnott's air-pumps,—that it can be properly done. We believe that he is quite right. *Active*, not merely *passive*, ventilation is required wherever numbers of people are congregated in a limited space, as must be the case with ships of war, and troop and emigrant vessels. The necessity for some means to withdraw the vitiated air from the sleeping berths is trebly great at night, when the ordinary means of ventilation are usually less attended to than during the day.

Mr. Thom incidentally mentions a fact bearing on the present subject, viz. :

"That almost every ship carrying coolies from Calcutta to the West Indies was attacked with cholera in the first fortnight of the voyage; these poor creatures, in all probability labouring under a choleraic diathesis on shore, by sleeping in the open air did not suffer; but no sooner were they cooped up on board a vessel, and hundreds of them shoved down into the between-decks, at night at least, or in the day-time also if the weather was bad, than they got cholera."

Among the most curious facts in the history of Cholera, are those occasional outbreaks of it at sea in crowded ships, when, as well as we can judge from the narrative of the circumstances, there were not any germs of the disease among the passengers or crew at their departure from land. The two cases cited from the 'American Journal' by Dr. Parkes, in that most valuable "Inquiry into the bearing of the earliest cases of Cholera which occurred in London during the present epidemic," published in the Number of this Journal for July, 1849, are especially interesting. We strongly suspect that the predisponent and localising cause of the disease which we have been considering, is to be regarded as one of the chief elements at work in its development upon those and such-like occasions.

The same view is taken by the Philadelphia Board of Health, in their interesting Report. "The appearance of the Cholera at State Island (New York), and its almost simultaneous outbreak at New Orleans, is one of those peculiar coincidences which will for ever be shrouded in obscurity. Carried into both ports by emigrant ships from Havre, where when they sailed there was no Cholera known to exist—the one leaving on the 3d

of November and the other on the 9th, following nearly the same track, the disease appearing at sea on the 25th November in one, and on the 28th in the other, when most probably in the vicinity of each other, crowded with emigrants, uncleanly and badly ventilated. The inference is that they must have passed through a stratum of atmosphere loaded with some peculiar influence which, under favorable circumstances, produced in both cases the cholera poison." This statement, we may remark, is coincident even in language with the sentiments expressed in the above Paper by Dr. Parkes, whose clear and candid judgment is conspicuous in all that he writes.

Before quitting the subject of the cholera in ships, we would remind the reader, that there is every reason to believe that a much greater amount of sickness prevails in merchantmen and other descriptions of vessels, than is generally imagined. The accommodation for the men, especially at night, is usually wretched, and without any regard to the necessary conditions of health. There is some interesting evidence upon this point in the Board of Health's 'Report upon Quarantine.' Nor should we here omit to remark that in many medical communications, especially when the writer's object is to establish some favorite view, the statements are often so vague and imperfect on points that are essential to be known before any accurate conclusions can be drawn, that their value is next to nothing. For example, we sometimes read of cholera and other zymotic diseases having appeared on board vessels described as clean, commodious, and handsome; and the inference is immediately drawn that the development and spreading of the disease were quite independent of any neglect of sanitary precautions. Unless particulars are given as to the number of persons on board, the amount and nature of the accommodation, the condition of the holds, &c., the narrative cannot be received as evidence. In medicine, loose and imperfect statements are little less hurtful and misleading than positively false ones.

In closing our remarks upon this branch of our subject, we cannot do better than quote the opinion of the Academy of Medicine, as laid down in their instructions to the French people, issued in the spring of 1849:

"The first, and without doubt the most important case, is to maintain *around each person a pure atmosphere*; experience having shown that those who neglect this precaution in the time of the epidemic are the most exposed to be attacked by it; consequently persons should avoid, as much as possible, sleeping in too great numbers in the same room, &c."

II. The next determining cause of choleraic invasion that we shall illustrate, is the vitiation of the atmosphere from *Putrid Effluvia*. A few years ago, it used to be gravely disputed by some learned writers, whether emanations from putrescent matters were really injurious to the health at all; and much stress was laid upon the fact that night-soil men are often seen to be hearty and vigorous, and upon the statement of travellers, that Esquimaux savages did not suffer from the stinking blubber within their dwellings. It is certainly not much to the credit of our profession, that such reasonings had ever weight with any one; although we regret to remember that they have been reproduced upon public occasions, even within the last year or two, by gentlemen of some repute among us. It is, therefore, not to be much wondered at, that the laity are far from being satisfied on the point. Mr. Grainger amusingly tells us of a worthy member of



a Board of Guardians, who would not admit that the effluvia from cess-pools could be at all noxious, because, as he said, he had lived near one all his life! The same sort of argument was generally wielded with great power, during the late inquiry into the state of the graveyards in the Metropolis and elsewhere, by certain parties, when the attempt was made by the Board of Health to put a stop to interment during the raging of the cholera; an appeal was always made to the case of the sexton and his family, as being in as good health as any in the parish. But without examining the force of these cases, or discussing whether, as believed by some, it is salutary for children with whooping-cough to be held over a privy "early in the morning," or for consumptive patients to inhale the atmosphere of a close crowded cow-stall, we shall proceed to adduce some evidence which to us appears to prove that foul putrid exhalations were very far from being harmless during the recent epidemic. We begin with the following quotation:

"Immediately opposite Christchurch Workhouse, Spitalfields, belonging to the Whitechapel Union, and only separated from it by a narrow lane a few feet wide, there was in 1848 a manufactory of artificial manure, in which bullock's blood and night-soil were desiccated by dry heat on a kiln, or sometimes by mere exposure of the compost to the action of the sun and air, causing a most powerful stench. The workhouse contained about 400 children and a few adult paupers. Whenever the works were actively carried on, particularly when the wind blew in the direction of the house, there were produced numerous cases of fever, of an intractable and typhoid form; a typhoid tendency to measles, small-pox, and other infantile diseases, and for some time a most unmanageable and fatal form of aphthæ of the mouth, ending in gangrene. From this cause alone 12 deaths took place among the infants in one quarter. In the month of December, 1848, when cholera had already occurred in the Whitechapel Union, 60 of the children in the workhouse were suddenly seized with violent diarrhœa in the early morning. The proprietor was compelled to close his establishment, and the children returned to their ordinary health. Five months afterwards, the works were recommenced; in a day or two subsequently, the wind blowing from the manufactory, a most powerful stench pervaded the workhouse. In the night following 45 of the boys, whose dormitories directly faced the manufactory, were again suddenly seized with severe diarrhœa; whilst the girls, whose dormitories were in a more distant part and faced in another direction, escaped. The manufactory having been again suppressed, there has been no return of diarrhœa up to the present time. In the summer of 1847, a similar manufactory was established in the parish of St. George, Southwark, in the midst of a dense population. It is stated that on the very first occasion when the operations of this manufactory were commenced, a most powerful stench pervaded the neighbourhood, so as to attract general notice, and that soon afterwards a large number of persons living around were suddenly seized with diarrhœa." (pp. 41-2.)

The supervention of bowel disorders upon exposure to putrid effluvia, is of common occurrence at all times, especially during warm weather. Severe attacks of diarrhœa, and even of dysentery, often occur on board ship, when the smell of the bilge water has become more than usually offensive; and the men sleeping near the pumps are generally observed to suffer most. Medical students are frequently obliged to discontinue their attendance at dissecting-rooms, when these are not properly ventilated, in consequence of loss of appetite and irritation of the bowels. The same effects were experienced in a marked degree by Dr. Lewis, from the offensive atmosphere of the crowded church vaults in the metropolis,

as related by him in the Interment Report of the Board of Health. Again, the emptying of a cesspool is often followed by diarrhoea among some of the inmates of the house; occasionally it has induced alarming attacks of enteric disease. The case of the boarding school at Clapham, many years ago (before the first visitation of cholera), will be remembered by some of our readers. The facts were these: A foul cesspool had been opened, and its contents spread over a garden adjoining the playground; this was in the month of August. Within the course of two or three days, no fewer than 22 of the boys were seized with alarming symptoms of violent irritation of the stomach and bowels, subsultus of the muscles of the arms, and excessive prostration of strength. Two cases proved fatal. If such an event is liable to take place in ordinary seasons, how great must be the danger when the choleraic poison is abroad! The Reports of Mr. Grainger and Dr. Sutherland abound with the most painful illustrations of the fact.

Take, as a first example, the case of the Witham suburb, at Hull, described at page 45 of the Report, where about two acres of ground are used as a place of deposit for dung and night-soil, "which is interspersed in heaps among the houses and close to the doors of the dwellings." And what was the result? It will be remembered that what were deemed by many the first cases of the Cholera in this country, occurred, in September, 1848, in the Port of Hull, on board the 'Pallas,' which had just arrived from Hamburg, where the epidemic existed; and that subsequently several other vessels with the disease on board arrived from the Continent. Already, however, before the arrival of the 'Pallas,' two fatal cases had occurred in the town. Notwithstanding these facts, the pestilence—as was predicted at the time by Mr. Grainger and Dr. Sutherland, who had been sent down by Government to inquire into the facts—did not manifest any tendency to spread in Hull. The necessity, however, of removing existing nuisances, and taking other steps to improve the sanitary condition of the town, was strongly impressed on the local authorities; and earnest representations were made, as to the extreme danger of the spot described above; but these representations were not heeded. The subsequent history is very instructive. It was not until nine months afterwards, viz., in July of the following year, that the disease became fairly localised in any part of Hull; only one or two sporadic cases had taken place during the previous interval. Dr. Sutherland again visited the town, and found that no preventive measures had been taken to meet the storm, although there was now every reason to apprehend an impending outbreak. No cases of the disease had as yet occurred in or near to the district of Witham; but there was a moral certainty in Dr. Sutherland's mind that, if it once appeared there, its ravages would be most severe. The warnings given were still neglected. At length the epidemic struck the district, and his predictions were fearfully realised. On the outskirts of a triangular space, measuring little more than 200 yards, there occurred 91 fatal cases. "I have never known," says he, "an *open* neighbourhood of this size yield so large a number of deaths."

But let it not be supposed that such or similar instances are unfrequent; the Report teems with them. Take, for example, the case of a particular district in Bethnal Green Parish, situate behind Shoreditch Church, where within little more than a space of 400 yards by 150, there took place in

sixteen days, from August 16th to 31st, the fearful mortality of 211 deaths from cholera, and 18 from diarrhoea.

"In the year 1838 a report was presented to the Poor Law Commissioners describing certain localities in Bethnal Green, in which typhus was then or recently had been so prevalent, that it had attacked in some streets every house, and in some houses every room. From that time to the present, these localities have been the special seats of fever and every other epidemic that has chanced to be prevalent. From Dr. Gavin's careful and painfully descriptive Report on the recent progress of cholera in this district, it appears that in one of these places (Old Nichol-street), in 23 houses 50 persons were attacked with cholera, of whom 33 died, three deaths having taken place in one house and four in another; the visitors finding besides nine cases approaching to cholera, and 197 cases of diarrhoea. In New Nichol-street, closely adjoining, 21 persons perished of cholera; 30 more were attacked with the same disease who recovered, and there were besides 2 cases approaching to cholera, and 135 cases of diarrhoea. In a neighbouring street, Collingwood-street, 6 deaths took place in one house. Taking together 99 houses in this immediate locality, the deaths from cholera amounted to the enormous number of 147; being in the ratio of  $1\frac{1}{2}$  deaths to each house. In Beckford-row, in the same district, consisting of 16 houses, there occurred in the year preceding the outbreak of cholera, 23 cases of fever and one of erysipelas; and on the outbreak of cholera, 8 persons perished of this disease and 2 others of diarrhoea." (pp. 19-20.)

The sanitary condition of these localities is truly wretched. Among the principal causes of filth, Dr. Gavin places the accumulation of ordure, which appears never to have been removed from the premises, "so that in process of time, the back yards in several localities have been so raised as to be nearly on a level with what might be termed the first floors of the houses." For further details, the reader may consult Dr. Bushnan's work, which contains a great deal of matter that will much interest the sanitary inquirer as well as the medical philosopher.

In one court in St. Giles, Lascelles-court, consisting of ten or twelve houses, 17 deaths took place within a fortnight. It is one of the most disgusting places imaginable. Besides other causes of insalubrity, there is but one privy for the use of all the inhabitants, 80 or 90 in number; it is usually overflowing, and the surface of the pavement is bestrewed with excrement, and reeking with urine. Not a breath of air can reach this court, except by downward and upward interchange; so completely is it hemmed in on all sides. Shall we continue to wonder at the ravages of disease, while such enormities abound?

The west-end of London has its plague spots, as well as the middle and the east; and, if Whitechapel has its manufactories of ordure, and Bethnal Green and St. Giles their loathsome abominations in courts and streets, Notting Hill has its "Potteries," or, as they would more appropriately be called, its "Piggeries," from the vast number of swine (3000 it is said) that are kept there in the most disgusting filth. It is scarcely possible to give an idea of this place, in the immediate neighbourhood, too, of handsome villas and rows of stately houses. The dwellings, or rather hovels, in which the people live, are unsurpassed in nastiness by anything known in Ireland. The streets, courts, alleys, and yards, are without a drop of clear water; there are no sewers in any part of the locality, and all the drainage of the pigsties and privies is left on the surface, close to the very doors and windows of the houses, or flows lazily into a large stagnant pond, called the "Ocean," which is covered with a

filthy slime, and is continually bubbling with putrescent gas. The atmosphere is of course constantly charged with the most offensive effluvia. What the result of such a state of things has been on the health of the residents, is shown by Dr. Lewis's Report. (App. B, p. 48.)

"In the first 10 months of 1849, there have been 50 deaths among (less than) 1000 persons. Of these, 21 were from cholera and diarrhoea, and 29 from typhus and other causes. Mr. Frost, the Medical Officer of the district, pointed out rooms where three or four persons had recovered from fever in the spring, to fall victims to the cholera in the summer."

Mr. Frost, to whom the utmost credit is due for his persevering exertions to improve the condition of the place, states that, notwithstanding the population of the Potteries is short of 1000, the cases of sickness there are as numerous as in the whole of the rest of his district, with a poor population of more than 4000.

The evils of a locality like this are not confined to itself; the residents in the neighbourhood are made to suffer from its pollutions. Some 1200 or 1300 feet distant from the Potteries, reports Dr. Lewis—

"Is situated a row of clean, respectable houses, called Crafter-terrace, Latimer-row; the situation, though rather low, is clear and airy. On Saturday and Sunday, the 8th and 9th of September, 1849, the inhabitants complained of an intolerable stench, the N.E. wind blowing directly upon the terrace from the Potteries. Till this time there had been no case of cholera among the inhabitants. The next day the disease broke out violently, and on the following day, the 11th, a child died at No. 1; on the 12th, a person died at No. 2; on the 13th, one died at No. 5, and another at No. 7; on the 14th, another child at No. 1; on the 15th, a second child at No. 5; and on the 22d, an adult at No. 9." (p. 44.)

Glasgow and Edinburgh suffered most severely from the epidemic in 1832 as well as in 1849. We need not say that there is a prodigious amount of filth in both these cities. The utter want of drainage, and even of privies, in many districts, is notorious, and the state of the wynds and closes is of course generally disgusting. But it is to a peculiarity in the condition of their older buildings,—once the residence of the gentry, but now almost entirely tenanted by the lower classes,—arising from their structure, that we wish to draw attention at present. Many of them are eight or ten stories high, and each story or floor usually furnishes accommodation for several families, so that within a single tenement there is frequently an immense population; which have but one stair common to all, by which to approach their numerous dwellings. Not only is there no supply of water to these houses, but they are not even provided with ordinary conveniences of any sort, so that all filth and refuse is either thrown into the streets, or is retained in the rooms until the scavenger's cart comes round; and as it must be no inconsiderable labour to carry it down from such an altitude as the upper floor of these buildings, it is often accumulated for several days or even weeks before removal.\* Not unfrequently much of it is deposited on the common stair, which is then in the most filthy state imaginable. What then must be the condition of the atmosphere, as it enters from such avenues the dwellings of the inmates? the evil being aggravated by the circumstance, that the door or lower entrance of the

\* It is not a rare occurrence, according to Dr. Sutherland, to find large accumulations of decomposing matter, which appear to have lain *for years*, in garrets and empty apartments of these houses. We have heard that pigs have been known to be kept at the very topmost story of such tenements.

"land," as such a tenement is called, often opens from one of those closes to which allusion has been made. The windows, as usual in the dwellings of the poor, are seldom fitted for ventilation. Now these "lands" have always been notorious for the amount of epidemic disease among their inhabitants; and it has very often been remarked, that there is more sickness and mortality in the upper stories, than in those lower down the stair. Dr. Sutherland accounts for this in the following manner:

"These circumstances [the filthy state of the rooms, stairs, &c.] fully explain the reason why large tenements are so liable to epidemic disease, apart from considerations of drainage or surface-cleansing; but there is yet another element of unhealthiness in the overcrowded population which inhabits them, and in the entire absence of any means of ventilation. Where there are a large number of families, there must be a corresponding number of fires burning at all seasons, so that the temperature of the whole internal atmosphere is higher than of that without. There is a constant tendency of this warm impure air to ascend toward the higher flats, by the staircases, through crevices in the ceilings, and even through the floor and plaster, both of which are porous. If what has been already stated as to these peculiar causes of disease be correct, we should naturally look for marked effects of an epidemic in the upper flats. I had been several times so forcibly struck with the occurrence of epidemic disease in the loftiest parts of Edinburgh, that when cholera appeared in Glasgow, I requested the district superintendents of the city parish to keep records of the precise flats in which the cases occurred, and the results of this classification have confirmed the views above stated in a most remarkable manner." (p. 22.)

An account was kept of the precise localities of 1106 cases of cholera in such houses in Glasgow; and the relative unhealthiness of the different stories was found to stand thus:

"The middle floors are the most healthy, as being equally removed from the effects of the upward drainage of the foul and unwholesome internal atmosphere, and the offensive exhalations from the uncleansed and undrained streets below. From their greater proximity to the latter cause of disease, the ground flats rank next in unhealthiness; while the top flats, from becoming, as it were, cesspools for the aërial drainage of all the stories below, were found to be by far the most liable to attacks of epidemic cholera. The result is very striking, and points to the existence of causes of epidemic disease in the Scottish cities, which have hitherto attracted too little attention. They are the same in character, but far more aggravated in degree, than those which have been observed to exist in the upper flats of unventilated cottages and workshops by Mr. Chadwick and other observers." (pp. 22-3.)

It is not to be overlooked, that the occupants of the upper stories are usually poorer than those who live lower down, the amount of rent decreasing with the elevation of the flat. Of course, the element of destitution must not be left out of consideration; but then, unfortunately, the greater the poverty the greater generally is the filth at the same time; and from what we have said as to the want of water supply, and of any conveniences to such building, it is obvious that it must be utterly impossible to maintain any degree of cleanliness, when not only every drop of water used has to be carried up many flights of stairs, but all the house refuse to be brought down.

No sets of cases are more interesting and suggestive in a sanitary point of view, than those in which very partial and circumscribed outbreaks of zymotic diseases occur in districts that are noted for healthfulness, and for general exemption from their ravages. Of such outbreaks, the late epidemic afforded some striking examples:—In Highgate, with a population of about 3000, only six cases of cholera occurred, and all of



them among the members of one family, living in two adjoining houses, situated at the top of Swaine's-lane. This is a place which has long been noted among the inhabitants for its offensive nuisances, and which Mr. Moger, the medical officer of the district,—knowing that it was almost invariably the *habitat* of whatever epidemic disorder happened to be prevalent,—felt so convinced would be the spot attacked by cholera if the pestilence should visit Highgate, that he had made, in the course of the preceding summer, repeated complaints respecting it to the Board of Guardians, in the hope of correcting its sanitary condition; but without avail. Besides a large cowshed on one side, where an immense accumulation of dung was collected, there was on the opposite side of the lane, in the backyard of the house where the first cases occurred, a foul overflowing privy, immediately facing and within a few feet of the door. The stench of this was horrible, Mr. Moger stated, especially at night, when he had to remain some time in the room with his patients, while the disgusting effluvium from the filthy cowshed opposite quite sickened him as he approached the house.

Equally instructive is what took place at Hampstead, where, during the season, only nine fatal cases of cholera occurred. One was in the person of a gentleman who had fled from his house in Albion-terrace in the Wandsworth-road to the well-known inn, Jack Straw's Castle, on the top of the Heath, in the hope of escaping the pestilence; but the disease was already in his system, and he died on the following day. Of the remaining 8 cases, no fewer than 6 (of which 4 were fatal) occurred in a family living in rooms over a stable,\* in the rear of the George Inn, near Downshire-hill. "At the stable door was a dungpit, which had not been emptied for some weeks; in the stable, in addition to the ordinary sources of impurity, there were three or four pots to collect the urine of the animals, and usually emptied twice a week. In the yard at the rear, and into which one or two windows looked, was a privy that stunk abominably, and within two or three yards of it a pigstye, scarcely less offensive; the privy had been most injudiciously emptied out, a day or two after the first death took place." Two deaths took place, we believe, in Flask-walk, where most disgusting nuisances existed, and which had been specially mentioned for its unwholesomeness by Mr. Lord, in an interesting pamphlet on the 'Sanitary Condition of Hampstead,' published in 1847.

Two localities in Holloway—another suburban district of the metropolis, where the air, if not polluted by local nuisances, is pure and wholesome—equally attest the sad effects of sanitary neglect. In Rutland-place, out of about 120 persons occupying ten houses, 27 cases of cholera occurred, and 14 proved fatal; and every person in the row had severe diarrhoea. "The houses," says Mr. Liddle, "are double fronted, and have, consequently, no windows behind for ventilation. In one of the rooms of No. 4, seven persons lodge. Only two of the houses are supplied with water. There is no provision of any kind made for the drainage of this place except at the privies, which, I believe, empty themselves into a

\* The disease was observed in several instances to be extremely virulent in such dwellings. Three fatal cases occurred in a room over a stable in Paradise-place, St. Pancras: "The floor was full of openings, so as readily to admit the foul effluvium to pass through." Two instances of the *hydrotyphus* form of the disease, where the patient is struck down as by a deadly poison, and dies without vomiting or purging, are mentioned by Mr. Cooper, of Southampton; the victims lived in rooms, underneath which was an immense accumulation of dung. Many other instances might be adduced.

drain ; and as the ground floors are not raised above the surface of the earth, they are very damp." The description given by Mr. Liddle of the other locality, Brand-street, reveals a still more distressing picture of the criminal negligence of the proprietors of low dwellings, and of the terrible loss of life that is fairly attributable thereto : 21 deaths occurred in 40 houses. "In two of them the privies were quite full, and in so dilapidated a state, that in wet weather the water and the soil overflow into the sitting rooms."

It will be remembered, that one of the most severe outbreaks of cholera in the metropolis occurred at Albion-terrace, Wandsworth-road. As this case excited very painful attention at the time, and the facts connected with it appear to us to have a well-marked bearing on the important question, as to the influence of a tainted local atmosphere in drawing down and intensifying the choleraic poison, it deserves especial notice. The houses in the terrace are commodious comfortable dwellings, and were at the time of the outbreak occupied by highly respectable families. The first case of cholera occurred on the 28th of July, at No. 13 ; and within the next fifteen days, no fewer than 42 cases took place, of which 30 proved fatal. The report made by Dr. Milroy on the circumstances connected with this severe attack, is too long for insertion, and cannot be well abridged. Suffice it to say, that besides a black sewer-ditch at the distance of some hundred yards in the rear, there were noisome effluvia from the drains in almost every house, besides an enormous collection of offensive refuse in the cellar of the house where the first case occurred ; also that the water was polluted by the overflow of the drains into the cisterns. The houses on the other side of the road, immediately facing Albion-terrace, escaped ; a strong proof of the localisation of the disease.

There is certainly not a more frequent or more perilous source of mischief during the prevalence of epidemic disease, than foul and obstructed drains ; and it is unfortunately the fact that the construction of these works has hitherto been so very faulty, that in many parts of the metropolis, as well as in every town and city throughout the kingdom, instead of being conduits for the removal of decaying refuse, they are no better than elongated cesspools, which detain their stagnant contents, and regurgitate their noxious effluvia into the interior of the houses with which they are connected. Everyone who has been in the habit of visiting the dwellings, not of the poor only, but also of many of the respectable working classes and of small shopkeepers, must be aware how disgusting is the stench from the drains, more especially in certain states of the atmosphere—the very states, be it observed, that are apt to prevail in sickly seasons. What then must be the condition of such dwellings at night, when all the doors and windows of the house are closed, and ventilation is thus most effectually prevented, and when the atmosphere of the rooms is moreover vitiated by the respiration of the numerous inmates ? So thoroughly bad is the state of the drains in most of the humble districts in every town, arising from ignorance and neglect in the mode of their construction, that it would be infinitely better if there were no such works at all, and if the removal of house refuse was effected by systematic and regular scavenging. There is not a single report of any of the engineer-inspectors on the towns examined under the Public Health Act, which does not contain conclusive evidence that the force of fever, as well as of cholera,

has very generally fallen on those localities, which are either without any drainage at all, the refuse being accumulated close to, or perhaps actually within, the dwellings, or in which the drainage that has been attempted has been so unskilfully performed, as to have made the evil worse than ever.

As a specimen of the effects of badly-constructed drains, the following is an instance of what occurred in some courts in Bristol, where 44 deaths took place out of 66 dwellings :

“A more deplorable event, perhaps, never occurred than these tables describe. A very slight consideration of the whole circumstances is, in my opinion, sufficient to prove that this great sacrifice of human life was occasioned by ignorance or negligence, as flagrant as any which from time to time gives rise to railway or other accidents. A glance at the plan will show that something like sanitary improvements had actually been contemplated; and no doubt it was believed that the object would be attained, if only a sufficient number of drains and privies were constructed. Like every other step taken in a false direction, the so-called improvements increased the evils they were intended to mitigate, and with the other circumstances above detailed, caused the untimely death of many innocent persons.” (p. 56.)

The report of Mr. Clark, the engineer-inspector, gives details of the disgraceful misconstruction of the drains: “the fall, instead of being to the main sewer, was found, on the contrary, to favour the flow of sewage upon the courts.”

The evidences of the localising influence of sewage effluvia are sometimes rendered very marked by the limitation of the disease to one or two houses, or to a single ward of a public institution, where the nuisance is experienced. Thus, three fatal cases of cholera occurred in two adjacent houses in Union Street—a wide and airy street—Plymouth, among the earliest which took place in the town, and when there was no sickness at all in the immediate neighbourhood. The circumstances which appeared to have determined the attack were these. In consequence of the construction of some works connected with the railway terminus, the drains of several houses had become dammed across, and the result was that their lower premises were overflowed with sewage water. “Notwithstanding various energetic remonstrances to the authorities,” says Dr. Roe, “this state of things continued for several weeks, and had not been productive of sickness; but as soon as the weather became warm, a stench was perceived, which increased to an intolerable degree, and then cholera manifested itself.” Three of the inmates were seized with the malignant form of the disease (which at the time had not yet manifested itself even in the filthiest parts of the town), and were rapidly carried off. Several of the other inmates were affected with diarrhoea. One of the fatal cases occurred in a young man, who went down into the cellar of one of the houses to get a bottleful of the foul water which had inundated the basement story. Within an hour he was attacked, and he died in the course of thirteen hours from the commencement of the symptoms. “He said himself that he felt certain that the stirring up of that filthy stuff was the cause of his illness.” No further sickness occurred in either of the houses after the nuisance was thoroughly corrected, nor in the neighbourhood, although the epidemic subsequently raged with great violence in different parts of the town. Dr. Sutherland informs us that



in Chorlton, Manchester,—“although the wide, open, regular streets escaped the disease, there was one exceptional case in which a number of houses built over the course of an old brook, *now a common sewer*, and having their drainage connected with it, were attacked. The exception in this case is a valuable piece of evidence.”

An outbreak of cholera occurred in one of the wings of Greenwich Hospital, hitherto exempt, immediately after the opening of a foul drain; the pensioners, whose rooms were nearest the nuisance, suffered first and most severely.

It is stated on the authority of Dr. Allen, the resident physician of the Marylebone Infirmary, that close to a ward in that institution where cholera first appeared—while the rest of the house was quite exempt from the disease—there was an open untrapped drain which emitted very offensive effluvia. The nurses remarked that the smell was always worst when the windows were first opened in the morning. The nuisance was immediately corrected, and thereupon the disease ceased to manifest itself in this particular part of the building. A somewhat similar occurrence took place some years ago in a particular gallery in Bethlem Hospital. Although this gallery was lofty, very airy, and one of the most favorably situated in the whole building, and the patients were of the healthiest class, it was notorious for cases of fever and diarrhoea. Upon examination it was ascertained, that owing to some defect in the water-closet, a leakage of the soil had taken place under the floor. The evil was rectified, the sickness ceased, and the gallery has ever since continued as healthy as any part of the institution, which, as we shall afterwards see, was altogether unscathed by the late epidemic. The following statement from a foreign source may be aptly quoted here :

“At a late meeting of the Institute was read a highly interesting memoir on the epidemic attack of cholera in the prison at Brest, occupied by the galley slaves. Some facts, which seem to connect the development of the disease with malarious causes, may be worthy of record. The prison contained 2662 inmates, distributed in four wards and in an infirmary. The four wards are furnished each with twenty-seven water-closets, in order that the prisoners of each row may be enabled to reach the closet without being unchained, for these unhappy culprits never quit their heavy chains for an instant. The water-closets communicate with a drain which opens into the harbour of Brest, and at low water, the south-west winds blowing up the unguarded drain, force back the mephitic vapours into the very wards. The infirmary and the condemned cell are free from this inconvenience. 189 cases of cholera occurred in the prison, and of these no less than 113 proved fatal. Now, of 2445 prisoners in the wards just alluded to, 165 were attacked by cholera; while of 217 individuals in the infirmary and condemned cell, only 3 persons were attacked. The very same result had occurred in 1832. At that period 53 prisoners were cut off by cholera in the wards furnished with water-closets connected with the open drains, while in the infirmary, which is free from this source of disease, only a single death took place.” (pp. 46-7.)

In none of the instances we have just related, was there any ground for suspecting the introduction of the disease by personal communication.

In confirmation of these statements, we may remark that those places on the banks of a river, where large sewers discharge their foul contents, have in very many instances been the special seats of cholera attacks. It will be remembered that among the earliest cases in the metropolis, in the autumn of 1848, several of the convicts on board the ‘Justitia’ hulk off

Woolwich were attacked and died. The vessel at the time was moored right opposite a sewer mouth, the effluvium from which was much complained of by the men on board, and especially by those who slept on that side of the vessel next the shore, and they were chiefly attacked. By merely changing the situation of the ship, so as to get out of the reach of the nuisance, there was an immediate diminution in the number of attacks, as well as a decided mitigation of the virulent character of the disease. The evidence given by Mr. Bowie, respecting the situation and condition of the vessels in the Thames in which the disease first manifested itself in 1831, points to the same result; and Mr. Roberton, of Manchester, has put on record the following well-marked fact:

“One morning it was discovered that several men had been seized with cholera during the preceding night, on board a vessel lying in one of the docks. The men were sent to hospital, and the vessel having been immediately warped into the river, another ship with a healthy crew took up her station. The next morning all hands on board were ill of cholera. On examining the dock, it was found that a large sewer discharged its contents under the spot where the vessel was placed.”

The first cases which occurred at Morice Town, Devonport, in the late epidemic, took place in a street which is wide and tolerably airy, although situated in a hollow, but at the lower end of which is the outlet of the main sewer of the district, at about three or four hundred yards from the sea, into which the fetid contents were conducted by a partially covered trench. In the course of the summer this trench had become obstructed, and the sewage in consequence overflowed a large open space situated between the outlet and the sea, and converted it into a stinking quagmire, from which the most disgusting effluvia were continually given off. The first death occurred in the corner house, which was nearest the seat of this nuisance.

Mr. Grainger adduces some important evidence on the subject we are considering. He states that:

“At Hamburg, in those streets which immediately face the spot where the numerous canals that have traversed the city and have become loaded with the excreta of 175,000 people, concentrate to pour their foul contents into the Elbe, the cholera raged so violently as to destroy 3·01 per cent. of the inhabitants, while residents near the other and purer parts of the river suffered much less.” (p. 50.)

There were of course other sources of insalubrity, besides the effluvia from the sewage, in most of the instances we have cited above, more especially in those where the nuisance existed on the slimy banks of a river or huge canal, into which filth of all sorts is being continually discharged. That, among other conditions, we may rank the exhalations from the *foul mud* which is more or less exposed every time that the water is low, is rendered highly probable by the following facts.

While cholera was prevailing in the town of Cardiff, in the month of June, 1849, a sudden attack of the disease took place in a cluster of houses about a mile and a half distant, under circumstances which, in Dr. Sutherland's opinion, “could leave no possible doubt as to the exciting cause” in operation. The houses, 22 in number (three however were vacant), and containing a population of 117 souls, were situated on the sides of the basin of a canal where it is about to enter the sea, from which it is only separated by a lock. The water had been drawn off, for the purpose of repairing the lock. In consequence of this, a large surface of

black putrescent mud was exposed to the direct action of a hot sun, and the most offensive effluvia were given off. The inhabitants of all the adjoining houses complained of the foul smell, and began to be affected with such symptoms as pretty clearly indicated the cause which gave rise to them. "The most common were general indisposition and oppression of the nervous power, marked by languor and lowness of spirits, and some degree of giddiness. To these in a number of instances succeeded general prostration, coldness, tremors, vomiting, diarrhoea, cramps in the bowels, developed cholera and death." Out of the 19 inhabited houses, 15 were affected, so that only four escaped. There were in all 43 cases of diarrhoea, 33 of developed cholera, and 13 deaths. The other houses in the neighbourhood, which were at a little distance from the side of the basin, and not subjected to the exhalations from its exposed bed, escaped entirely. The works of the canal were finished as expeditiously as possible, and the water admitted. Persons on the spot stated that the air felt purer immediately, and the disease was speedily arrested.

The account given by Dr. Milroy, of the Cumberland basin of the Regent's Canal, in the neighbourhood of the Regent's Park, and of the great prevalence of choleraic disease in houses adjoining to it, appears to confirm the observations of Dr. Sutherland at Cardiff. In addition to the evidence adduced in Dr. Milroy's report, it is worthy of notice that, according to the return given by Mr. Grainger of the amount of sickness in the different barracks in the metropolis, during the prevalence of the epidemic, there seems to have been a much larger proportion in Regent's Park barracks than might have been expected, considering the supposed salubrity of the locality. These barracks, it is to be noticed, are bounded on the east by the Regent's Canal, which was certainly at the time in a most unwholesome condition. The table in question shows that while in two battalions, amounting to 1101 men, and quartered at St. John's Wood and the Portman-street barracks, there were 61 cases of diarrhoea and 11 cases of cholera, of which 3 proved fatal, no fewer than 150 cases of diarrhoea and 8 of cholera (4 fatal) occurred in the Regent's Park barracks, out of only 385 men. Even in the Tower, a locality that is necessarily exposed to many noxious agencies from its very situation, out of a force of 1025 men, there were not more than 93 cases of diarrhoea and 33 of cholera, of which 10 were fatal.

Nowhere were the pernicious effects of stinking mud more conspicuously displayed, than in that horrible locality, which is described in the following passage by Mr. Walsh; to whom, aided by Mr. Martin, the district surgeon, many of the wretched inhabitants were indebted for a supply of water during the height of the epidemic:

"Jacob's Island is the name given to a portion of the parish of Christchurch, Bermondsey. It is surrounded by the tidal ditch or mill-stream. In the island and on the banks of the ditch are 300 or 400 houses. The drains and sewers of all the houses that are drained empty themselves into the ditch. The refuse of the neighbouring houses and the contents of their privies are also thrown into the almost stagnant water. One hundred and fifty of the houses have no water-supply whatever; many of the inhabitants are in the habit of using the water for cooking and other purposes; nay, even drink it unboiled. Some of the houses are totally unsupplied with water from any other source than the ditch; a few have wells which communicate with it. The analysis shows the immense quantity of organic matter which it contains. The ebb and flow of the tide are regulated by a floodgate; the

occupier of the mill lets the water in and out as he pleases : sometimes it is retained for many days, and sometimes the bed is nearly dry for the same length of time. The foulness of the water and of the mud at these times is incredible. Dead animals abound in it." (pp. 92-3.)

No description by word or pen can give the reader an adequate idea of the disgusting scene, whenever the water is low, in the foul tidal ditches; it must be seen to form an idea of it. The existence of such a state of things in our metropolis is a disgrace to us as a Christian people, and cannot but call down a judgment from Heaven. And what, pray, is the constant amount of sickness and death among the inhabitants of this place, but a judicial warning to our rulers? The people themselves, from long habit, seem to have become nearly indifferent to the abominations around them. Whoever reads with any attention the weekly reports of the Registrar-General, cannot fail to have noticed the numerous deaths that are continually occurring there. It was, of course, severely visited by the terrible "inspector of nuisances," on its late, as well as on its former visitation. Mr. Walsh says :

"In 1832 the earliest fatal cases of cholera occurred close to this ditch ; in 1849, also, the earliest fatal cases occurred here: diarrhoea and cholera abounded; hardly a house escaped, perhaps not one. On the south side of an irregular square, formed by the tidal ditch and its immediate neighbourhood, there occurred between June and October 41 deaths, in the centre 12, and on the west side 8, making a total of 61 deaths." (p. 94.)

The following Eastern picture forms a suitable pendant to that from the shores of the Thames :

"Cholera," says Mr. Rogers, "may prevail amongst the inhabitants of some streets in a town to a great extent, whilst the neighbouring streets and the whole of the surrounding country are free from it. I have several times known such an occurrence in Madras. The Coom river winds very circuitously through Madras, and in its meanderings it nearly encircles the village of Chintandrepett. This river was made a necessary of by hundreds of natives daily throughout the year ; and when the monsoon was heavy, and the bottom of this Augean stable was thoroughly cleansed, no ill resulted from it ; but if the monsoon failed, and the river remained uncleansed, when the hot weather returned, the water became low, and the filth at the bottom was exposed to the heat of the sun, the smell was most offensive, *and an attack of cholera was the certain result, its only victims being the inhabitants residing within a short distance of its banks.*"

Who can resist the force of such a statement as this?—it has all the value of a crucial experiment.

III. *Dampness.* The influence of a humid atmosphere in predisposing to the development of cholera has been very generally recognised ; and the late epidemic in this country, and elsewhere, has afforded much additional evidence as to its localising power. In 1849, as in 1832, the main stress of the disease in the metropolis fell on the districts near the banks of the river, and with especial severity on the Surrey side. The tinted cholera map, which accompanies Mr. Grainger's Report, is highly instructive, as it shows at a glance the varying severity of the epidemic in the different metropolitan districts. "Out of the ten parishes and unions in which the per centage of deaths was highest, eight are placed on the south of the river ; whilst, in all the southern districts, with a population of 585,067, or 26·5 per cent. of the whole population of the metropolis, no less than 8200 deaths, or 48·8 per cent. of the whole mortality occurred." The

coloured plan of the city of Hamburg also, given by Mr. Grainger, indicates a like excess of mortality there, in the districts adjoining the Elbe and the canals. An interesting diagram, published in the Registrar-General's Reports, exhibits the average elevation of the several metropolitan districts above high water mark (this diagram, and other valuable tables, will be found in Dr. Bushnan's work), together with the relative mortality during the 52 weeks ending Sept. 29, 1849. The general result is, that the deaths diminished in proportion to the altitude of the situation. Still there were, no doubt, a good many exceptions to this law, attributable in most instances to the influence of other local causes.

M. Fourcault, speaking of the influence of geological and hydrological conditions on the progress of cholera in France, remarks, that "the cities and villages disposed in an amphitheatre have, in general, presented three distinct zones;—the inferior, the most humid, has been the principal focus; in the middle, the epidemic loses a part of its activity; in the superior zone, it is almost or entirely extinguished." The remark holds quite as true of yellow fever, the plague, and other epidemic and endemic diseases, as it does of cholera. Numerous illustrations might be quoted, if space permitted, of the general—we do not say, invariable or universal—truth of the statement.

The extreme severity of the epidemic in St. Petersburg, on both visitations, has generally been ascribed to the exceeding moisture of its climate (although many other causes of insalubrity exist), in consequence of the ill-chosen situation of the city, below the level of the river, and surrounded with marshy ground, as is also the case with New Orleans, one of the favorite *habitats* of yellow fever. The inhabitants are always very subject to fevers, diarrhoea, dysentery, and scurvy. Severe outbreaks of cholera were often checked, by sending the people to higher spots in the neighbourhood. Constant reference is made by East India practitioners to the danger of encamping men in low-lying ground, at the foot of ghauts, or near river-courses, especially when the weather is hot and without wind; and to the almost immediate arrest of sickness, by removing to more open and dry ground a few miles off.

The exceeding moisture of the atmosphere just before the outburst at Kurrachee, is noticed by Mr. Thom; and the same thing has been observed on several other like occasions. It is to be remembered, however, that atmospheric humidity, like atmospheric heat, is far from being a constant accompaniment of the prevalence of cholera. Allusion has already been made to the physical condition of the rock of Bellary, and of the country round. It is also to be remarked that, during the height of the late epidemic in the metropolis, the air was unusually dry. But then—and this is a point meriting particular attention—it was very close and oppressive, and so stagnant, that when there was a strong breeze blowing at the top of the Observatory at Greenwich and over Blackheath, there was not the slightest breath of wind on the banks of the Thames. This stagnant motionless state of the atmosphere, especially if associated with considerable heat, is the most pernicious of all to health. Putrid effluvia are not dissipated or dispersed, while the process of putrefaction appears to go on with unusual activity; and the vapours exhaled from a river, canal, or ill-drained ground, rest like a thick fog over the spot whence they have risen. From what we observe of vegetable blights, it is probable, too, that the



molecular germs of zymotic disease are more rapidly developed under the meteorological conditions now noted. In tropical countries, dysentery more especially is extremely apt to occur immediately after such weather. But whether this conjecture be correct or not, there can be no doubt that the body is then peculiarly susceptible of morbid action. All the eliminating operations, without exception, are imperfectly or irregularly performed; but probably the functions of no organ or system are so much deranged and impeded, as are those of the skin. Attention has not been sufficiently drawn to the important part which the cutaneous tissue plays in relation to health and disease. In addition to a very large amount of watery vapour (30 oz. on an average per diem), thrown off by perspiration, there is from the skin, as from the lungs, a constant discharge of excrementitious matter, amounting, it is believed, to nearly two oz. in the course of the 24 hours. In close foggy weather, the perspiration clings to the surface, and the function of excretion is of course obstructed. But, besides the retention of effete matter thus induced, the redundant animal heat of the system is not duly carried off, in consequence of the diminished amount of cutaneous evaporation; and a state of feverishness and irritability is necessarily the result. Nor can we wonder at this, when we learn that it has been found by experiment, that, from a surface equal to a disc of six inches in diameter, with the dew point at  $83^{\circ}$ , and a temperature of  $90^{\circ}$  in the shade, the evaporation per minute would be equal to half a grain in a calm, 1.40 in a moderate breeze, and 2.10 in a brisk wind; thus making the quantity of fluid removed from the system to be nearly three times in a moderate breeze, and upwards of four times in a fresh wind, as much as in a calm or stagnant state of the atmosphere. The dreadful distress from a hot, moist, and stagnant atmosphere, in a tropical country, is well described by Mr. Thom. Besides the extreme languor and inability for exertion, the oppression of breathing, &c., we read of "the body being bathed in perspiration, and the skin corrugated, as if it had been immersed for a long time in water," less from the excess of perspiration than from the suspended evaporation.

But the skin, like the lungs, is provided with absorbing as well as with exhaling vessels. The activity of the cutaneous absorption is much influenced by the condition of the body, being less after a full meal than when fasting; also by the state of the air; for—the very reverse holds true of the excretory function—it is much greater in a moist than in a dry atmosphere. This point deserves especial notice: viz., that atmospheric moisture appears to stimulate the action of the cutaneous absorbents, and at the same time to impede that of the exhalants; whilst, vice versâ, in dry weather, the exhalation from the surface goes on with energy, while the absorbing process is comparatively inactive. This physiological fact has a bearing on our present inquiry. It has been suspected by some experienced observers, that marsh miasmatic and other forms of malarious poison find admission into the system chiefly by the skin, and several facts render the idea far from being improbable; at all events, every one recognises the great importance of protecting the surface with woollen clothing, as one of the most approved preservatives of health in agueish localities. Moreover, it is not irrelevant to remind the reader, that a very large majority of choleraic seizures take place at night, or very early in the morning; and it need scarcely be stated, that the potency of all miasmata

has invariably been recognised to be greater after sunset, more especially when the evenings are very humid. That the aërial poison has a tendency then to descend with the atmospheric moisture nearer to the surface of the ground, is rendered probable by many circumstances; and, among the rest, by the greater insalubrity of ground-floor and basement rooms, than of higher ones, for sleeping apartments; although it is not to be forgotten, at the same time, that the former are less removed from the seat of most domiciliary nuisances. Without, however, dwelling at present on these and other kindred points, we shall only remark, that it is of first-rate importance to maintain a circulation of air—warm and dry, if possible—through all sleeping rooms, during the prevalence of an epidemic disease like cholera. With this view much good has been obtained from keeping a small fire continually in the chamber; also from the use of Dr. Arnott's valves, air-bricks in the walls, &c.

The utter neglect of subsoil drainage, in districts and localities which are chiefly occupied with the dwellings of the poor and working classes, and which, from their geographical position, often require it the most, is a sanitary evil of immense magnitude. In many spots in the outskirts of our metropolis and other towns, rows of houses have been built of late years right upon the naked ground, without even so much as the greensward having been cut away, and this too with damp clayey soil beneath. The result is, that the walls of such houses never become fairly dry, and everything within smells musty and offensive. The exceeding dampness of the lower stories of houses of a better description, is illustrated in the case of Speedwell Street in Oxford, where so many fatal cases of cholera occurred; and Mr. Rawlinson alludes to some houses in Alnwick, which also suffered from, apparently, the same cause of insalubrity: "they have a superior appearance, and are not unduly crowded; but they have cellars underneath them which are always damp, *because they stand upon an undrained site.*" But mere dampness is not the only evil; in very many instances the foundation is not unfrequently saturated with the contents of privies and cesspools, situated close to—it may be, under—the dwellings, or, if these stand upon sloping ground, with the foul drainage from the houses above. We have seen cartloads of soft black mud dug from under the foundation of houses in St. Giles and Whitechapel; and the same disgusting state of things is common elsewhere. What must be the condition of the basement stories of such houses, unless a thick solid stratum of concrete has been first laid down?—a precaution, we need scarcely add, that is seldom or never taken. A striking instance of the infiltration of the subsoil of a district that suffered most severely from cholera, with the filthy drainage from the land above, occurred at Alnwick. The particulars are given in Mr. Rawlinson's report of his inspection of the town under the Public Health Act. Dr. Sutherland, after adducing some very interesting evidence, illustrated by examples, to show the mischievous effects of a wet subsoil in houses built in open airy situations on hill-sides,—selected as a place of residence from their supposed salubrity, but whose foundations were never dry in consequence of their proximity to, or of their being under the level of, canals, &c., or from their being built across the natural course of the drainage,—adds:

"The evils described are greatly aggravated if pigsties, manure-heaps, or other nuisances, are placed higher than the houses, especially if the ground be at all of a



porous nature. In such cases the lateral drainage becomes polluted with organic matters.

“Even surface-drainage, flowing from the higher to the lower parts of towns, at times produces much mischief. Such an instance occurred when cholera was prevalent in Edinburgh. The disease carried off four or five individuals in a single house, fronting the open country, at the foot of one of the closes in the Canongate. There was not a single case of cholera in the neighbourhood except these, and the house was perfectly clean, and the locality well ventilated. The catastrophe arose as follows:—The drainage of High Street and Canongate takes place on the surface, and is continually impregnated with night-soil and other impurities. In passing the mouth of the close in question, from some defect in the gutter, part of the drainage was turned aside and ran down the close. There was no escape for it at the lower end, where it accumulated and became extremely offensive. Only two or three families were exposed to the effluvia, and one of them was almost entirely destroyed. The cause was then recognised and removed. I cite these facts as affording individual illustrations of a class of causes, which operate in rendering localities unhealthy which otherwise should not be so. Houses and towns built on hill-slopes, evidently require sanitary precautions of a particular kind, and proper means should be taken to cut off the natural drainage from the site chosen, and to divert it in such a way as to render it innocuous.

“Much of the evil resulting from the close proximity of rivers and canals arises from lateral infiltration of the subsoil, and not merely from the aqueous vapour which rises from the surface of the water itself. In the village of Spring Bank, already referred to, many of the houses most severely attacked by cholera had their floors nearly on a level with the canal. A small cottage, in which the first cases occurred, is thus situated. It contained two inhabitants, both of whom died, and there is no other appreciable reason for the attack.

“The epidemic seizure of the lower part of Inverness in April, 1849, affords another similar illustration. The site occupied by the houses is a flat gravelly piece of ground on the banks of the river Ness, and the foundations are rather below high-water mark. The whole of this gravelly subsoil receives the brackish water of the river, which can be obtained by digging a few feet below the surface.” (pp. 51-2.)

The condition of a large portion of the metropolis, more especially on the Southwark side of the river, is a great deal worse; for there the subsoil is kept continually damp, not only by the lateral infiltration of the river, and by the quantity of refuse water thrown out from dwellings upon unpaved courts and ill-paved streets and alleys; but also by the regurgitation of the contents of the sewers, when their outlets are dammed up at every rise of the tide. The sewage is sometimes driven back with such force, as to escape by the privies or any other openings which it can find, and overflow the basement stories of the dwellings of the poor inhabitants. Shall we, with such a state of things, continue to wonder at the amount of sickness that is never absent from such localities, or at the desolating ravages of an epidemic disease like that from which we have just escaped?

IV. We have devoted so much space to the illustration of those determining causes of choleraic invasion, which have been already considered, that we can now only very briefly notice the influence of some others, such as the use of *Impure Water*, *Unwholesome or Insufficient Food*, *Intemperance*, and *Fatigue* from whatever cause induced;—each and all of them, however, affording much matter for serious thought. But we have deemed it better to endeavour to fix the attention of the reader strongly upon two or three points, than to disperse it over a number at once, especially as we

look forward with much interest to the publication of the expected Report from the College of Physicians, founded upon the large mass of very important evidence from all parts of the country, and from the Navy and Army Boards, in their possession.

Allusion has already been made to the disgusting state of the *water* that is used for drink by the residents in and about Jacob's Island, Bermondsey. Little less impure is the quality of this most necessary article of life in many parts, not only of the metropolis, but of every town—aye, too, and of many villages—in the kingdom. Presuming that every member of the profession without exception, whether at home or abroad, will not fail to possess himself of the Board of Health's Report, with the two Appendices,—and without a knowledge of their contents, no one can now be said to be acquainted with the history of the pestilence in this country,—we would point to the details given at pp. 59-63 of the Report, and at pp. 14-16 of Append. A, as deserving most attentive perusal. Besides these, numerous facts upon the same subject might be quoted from the published reports by the civil-engineer inspectors of the towns which have been examined under the Public Health Act during the last two years, as well as from various communications that have from time to time appeared in the Medical Journals. The whole subject of the influence of water used as drink, in the production and aggravation of disease, is one deserving much more attention than it has hitherto received from the profession, and should be made the theme of a special discussion. The recent Report of the Board of Health on the supply to the metropolis is, in our opinion, a work of very high value, and its contents should be generally known.

With respect to the effects of unwholesome or innutritious *food* on the production of cholera, we have only room to remark, that the influence of underfeeding alone has been frequently somewhat exaggerated, or brought rather too prominently forward, in comparison with the other determining causes of the disease. For example, in that able document issued by the London College of Physicians upon the threatened outbreak of the pestilence in the metropolis, in October, 1848,—a document to which we have great pleasure in referring, as confirmatory of the views which this Journal has always advocated on the important questions as to the mode of its diffusion, and the impotency of cordons and quarantine to arrest its progress,—it is stated: "Nothing promotes the spread of epidemic diseases so much as want of nourishment." This is scarcely correct. The common notion that cholera is particularly prevalent and fatal among the extremely poor, does not quite accord with fact; for the chief sufferers in the recent epidemic were not paupers, but independent labourers, artisans, and the lower grade of shopkeepers; classes generally not destitute of food or clothing. The circumstance, too, of a very large proportion of the victims being in the prime of life, and in vigorous health at the time of seizure, is equally significant. In the East Indies, the rice-fed Hindoo is not more liable to the attacks of cholera than his robust and flesh-eating comrades. As has been already remarked, the truth seems to be that the state of health, as well as the proclivity to disease, is influenced much more by the condition of the air that is breathed, than of the food that is eaten. The foul and fetid atmosphere of our Whitechapels and Bermondseys—aided too often by intemperance—has more to do with the haggard looks and earthy complexion of their denizens, than even penury

or want. That the use, however, of the unwholesome food permitted to be sold in our streets, played its part on many occasions in exciting choleraic attacks, cannot be denied ; the interesting fact of the comparative exemption of the Jewish population of the metropolis, recorded at p. 82 of Appendix B, deserves notice under this head. There was a very remarkable outbreak of cholera at Bridgewater, at the same time with that at Taunton, previously noticed ; in which the influence of bad food, as a predisposing cause, was very strongly brought out. A cargo of spoiled oysters was given away, their sale being prohibited ; and within two days afterwards, numerous cases of cholera, and twelve or fourteen deaths, occurred among the children of a school that had partaken freely of them. Similar instances might be mentioned, if space permitted.

The pernicious effects of *intemperance* in predisposing to the disease have been recognised by all writers, in the East Indies as well as in the different countries of Europe. What then must have been the mischief done by this debasing and life-destroying sin in a country like ours, where it has been computed that upwards of twenty millions sterling are annually spent upon ardent spirits alone ?

In quoting the general conclusions to which Dr. Sutherland and Mr. Grainger have come, as to the influence of local agencies on the development and virulence of Cholera, let the reader bear in mind the very ample and varied field of investigation which these gentlemen, more, probably, than any other observers, in this country at least, have enjoyed ; and the peculiar advantages which their official position gave them, for ascertaining the exact circumstances of each invasion which they describe. It is scarcely possible to form accurate opinions on the leading features of a wide-spread pestilence from an experience, however large, in any one locality ; nor can error be easily avoided by him whose observations are limited to his own sphere of practice. Least of all, perhaps, are hospital physicians likely to judge correctly, if their inquiries be confined to what may take place within the walls of a single institution. It is necessary to examine with much patience the facts connected with the origin and spread of an epidemic, upon the spots where it makes its appearance ; and this, too, under all the varying circumstances of place, condition, weather, and so forth. The commander of a regiment may narrate more accurately than any other person the operations in a battle where his own men were engaged, but he is apt at the same time to attach somewhat of undue importance to what came under his own immediate notice. The staff-officers, who were moving to and fro in different parts of the field, and were thereby acquainted with what was going on everywhere about the same period, will better appreciate and more truly describe the bearings and results of the various movements upon the general issue. And so it is, in a great measure at least, with the correct history of an epidemic invasion. The disease requires to be seen in different localities, districts, and countries, among the different classes of society, and under the numerous differences of local peculiarity. Above all, the observer must know what is taking place simultaneously in different places, whether adjacent to or more remote from the spot where he may happen to be ; otherwise he will almost inevitably be misled. It is in vain for the hydrographer to attempt an account of the tides in a particular harbour, unless he notes well not only their rise and fall at other points on

the same coast, but also the general currents of the great ocean stream. We may also remark, that it is of the utmost importance, in investigating the circumstances affecting the local outbreak and prevalence of an epidemic disease, that the attention be drawn, not to one or two only, but to all of them without exception ; not to the existence or non-existence of filth or foul effluvia in, around, or near to dwellings, to the exclusion of the equally, if not more, important subjects of the bad ventilation of rooms, and the insufficiency of space for healthy respiration, nor yet of these subjects, to the overlooking of the food that is eaten, the water drank, the habits of the residents, &c. The consideration of each and all of these matters is doubly necessary, when inquiry is made into the history of the first or earliest cases in any particular place.

Dr. Sutherland states, as the result of his experience in between forty and fifty towns in different parts of the kingdom, that—

“With a few apparently exceptional cases easily accounted for, cholera has invariably localised itself in the bad sanitary districts of towns, while the portions in a better sanitary condition have as invariably escaped, either entirely or with the occurrence of the milder diarrhoeal forms of the epidemic . . . . . In every district which it attacked, its ravages were most fatal where the sanitary conditions were the worst.”

The evidence of Mr. Grainger, who had the opportunity of observing the disease at Hamburg and Berlin, as well as in numerous places at home, and more especially in the metropolis, is equally emphatic, in reference to the results to be anticipated from sanitary improvements :

“Having,” says he, “carefully gone over the whole of the evidence collected by the medical inspectors ; having well weighed a large number of facts communicated to me in a series of years by practitioners of all classes, residing both in town and country ; and having also considered all the various circumstances that have fallen directly under my own observation, I feel myself justified in stating, that in no one instance has a well-matured plan of sanitary amelioration failed in the great object of all these proceedings—the diminution of sickness, suffering, and death, and the consequent promotion of human happiness.”

Are these assertions borne out by the recorded experience of other medical men, and by the testimony of well-established facts ? The Provincial Medical and Surgical Association have, very greatly to their credit, instituted an inquiry among their members on the subject of the late epidemic ; and already a number of interesting communications from different parts of the country have been published in their journal. The following brief extracts will suffice to show their general bearing ; we must refer the reader to the original papers for particulars. Mr. Bloxam, of Newport, says : “During the prevalence of the disease there, inquiries were carefully made into the local circumstances of the neighbourhood where each case occurred ; and it was stated that almost invariably there was some impurity found to account for it. I think, however, that in some instances the impurity was of no very aggravated nature ; but, on the other hand, I should think that there was not one case occurred in a really well-drained and well-ventilated dwelling.”

Mr. Reid of Canterbury, after mentioning the localities, all of them filthy, where the disease was most severe, particularly specifies one place where four cases of cholera occurred : “The houses were supplied with water from a well which was in close contact with a cesspool ; and almost all the

persons drinking water, had been about that time affected with disordered bowels. *The same locality was also a few months before visited severely with typhus.*"

The experience of Mr. Smith of St. Mary Cray, Kent, is especially valuable. "After twenty years' attendance on the poor in the district, I have become acquainted with the fever nests, and have, indeed, taken pains to mark them. After the former epidemic, several of these pest-spots were effectually cleansed, drained, and permanently improved. The cholera in 1832 was most rife in these spots; but, on this occasion, although still occupied by the same class of poor, they have been comparatively exempt; the uncleansed localities have, however, again furnished the greater number of cases. What I then wrote still holds good;—where fever is found, there is the chosen seat of cholera."

Such statements as these are confirmed by the evidence of Dr. Duncan of Liverpool, of Dr. Malcolm of Dundee, of Mr. Goldney of Bristol, of Messrs. Noble and Golland of Manchester, and of many other gentlemen, given in Appendix A, as well as in various other sources of information. We might quote also extracts from the writings of East India practitioners, and from foreign authors to the same effect. The work of Dr. Riecke contains much important matter as to the localisation of the disease in Berlin; it is altogether a very valuable contribution to sanitary science. Not less instructive are the two official reports from America now before us. We can merely give one short extract from the latter. Dr. Clark, the city physician of Boston, says: "Isolated instances of the disease were noticed in even the most salubrious portions of the city; but, with a very few exceptions, the disease was confined to unhealthy, ill-ventilated, and crowded localities. The lower parts where the drainage is difficult and the cellars more or less invaded by the backwater, those reclaimed from the ocean and those in the vicinity of marshes, were most attacked by the pestilence."

How confirmatory of the great truth established by such concurrent evidence, has been the history of the first cases in almost every town and village where the pestilence has appeared! On this head there are, perhaps, no facts so thoroughly valuable as those contained in Dr. Parkes's paper already referred to. Sometimes the earliest cases in the last visitation took place in the same streets, nay, in the very same houses in which the disease first manifested itself in 1832; as at Leith, Pollokshaws, and also, we believe, at Torquay. They were all, of course, in the very worst localities. At Oxford, the first case in 1849, as in 1832, occurred in the county jail. This return of the cholera to its former haunts has been likewise noticed abroad. Thus, at Groningen in Holland, it attacked in 1832 only two houses in the better part of the city; and in these two identical houses the epidemic broke out in 1848.

Pass we on now to test the accuracy of Mr. Grainger's statement, and to show that sanitary ameliorations have invariably been productive of mitigation and diminution of choleraic disease. These ameliorations may have consisted either in the temporary abatement, or the removal, of the special causes of insalubrity which have been engaging our attention, or in the adoption of permanent and more complete structural improvements. The thinning of a crowded population, the suppression of nuisances, the clearing away of filth, the lime-washing of dwellings, the supply of pure water



where it was scanty or unwholesome, have invariably been followed, and often too very promptly, by a diminution or even cessation of sickness. The amount of benefit has always been greater when such steps had been taken for some time before the invasion of the epidemic, and indeed, has been pretty nearly commensurate with their systematic and persevering enforcement. In no town were they more efficiently carried out than in Sheffield, and nowhere were the good results more conspicuous. The salutary effects of active cleansing and limewashing of dwellings in foul localities, notorious as fever-haunts, were experienced in many places; particularly in some parts of Bristol, and upon a still larger scale and with more striking advantage at Edinburgh. Liverpool afforded a well-marked instance of the good to be obtained from the regulation of the low lodging-houses, in point of the number of the inmates, as well as from other sanitary measures. Numerous instances of a merely local nature might be quoted from the Board of Health's Report, if space permitted. The case of Camden Place, Kensington, mentioned by Mr. Grainger, is a very decisive one; and not less interesting is the statement of Dr. Adams of Glasgow, respecting two large tenements in that city. Similar results have been obtained abroad.

Mr. Rogers records the following interesting fact. The native regiments inhabiting the Vepery lines in Madras had constantly, in each succeeding year, whenever cholera prevailed in the neighbourhood, suffered from the disease. In 1841, the medical officer in charge pointed out the necessity of having the main drains, which were obstructed, cleaned out; of covering some offensive ground with a thick stratum of earth; of filling up a tank near the hospital; and of correcting the condition of a burial ground close by. "These suggestions," says Mr. Rogers, "were carried out; and, on inquiry after two years, I ascertained that the men inhabiting these lines had not suffered from the disease since, though it had raged in the neighbourhood severely several times."

The Boston Commissioners distinctly state that "though these precautionary measures, (consisting chiefly in the removal of domiciliary and other nuisances) which had been taken, did not prevent the anticipated attack, there is every reason to believe that they were effective in checking its progress and diminishing its virulence." Dr. Clark adds this most important testimony: "The experience of this epidemic has certainly given most satisfactory evidence of the power and value of sanitary measures; for, while no person was attacked without some obviously exciting cause, so in every case in which those much exposed were removed from the deleterious influences, and provided with cleanly, airy apartments, and suitable food, an attack of the disease was averted."

Not less emphatic is the language of the Philadelphia Board of Health. "By calling attention to the mitigated form of the cholera as it appeared among us, and to its limited period of duration, the conviction forces itself upon every intelligent mind, that the prompt organisation of sanitary measures for cleansing the city, and their practical application by the Board of Health, have been the active and efficient means for lessening the ravages of the pestilence."

But far more gratifying, in every point of view, is the clear testimony which the late epidemic afforded of the strikingly beneficial results of

substantial structural improvements, in averting the fatal effects of choleraic disease. From a large mass of evidence, we select the following facts as illustrative of this most important subject:

The three Model Lodging-Houses in the metropolis—two of them situated in a most unhealthy district, and where there were numerous fatal cases around—escaped almost entirely. There were a few cases of diarrhoea among the inmates (210 in number), and only one case of cholera, which occurred in an old man, intemperate and ill-fed. The complete immunity of the “Metropolitan Buildings” in Old Pancras Road, containing upwards of 500 inmates, was equally striking, although within a few hundred yards the epidemic was so severe that three deaths occurred in one house, and the whole neighbourhood was severely afflicted with diarrhoea. Of the Metropolitan *Prisons*, two suffered severely, while the seven others remained nearly exempt. In the Model Prison at Pentonville, whose sanitary arrangements are good, there was no cholera, and very little diarrhoea, among 465 inmates. Giltspur and Newgate Prisons enjoyed, the former a complete, and the latter an all but complete, exemption, although the district around suffered with extraordinary severity. The case of the House of Correction, in Cold Bath Fields, is, perhaps, the most instructive of all. In 1832, when the number of the prisoners was 1148, there occurred 319 cases of diarrhoea, 207 of cholera, and 45 deaths. At that time the drainage of the prison was most faulty, the sewers having in places fallen in and become choked with soil. Subsequently, the whole sewerage was rebuilt; and, on examination previous to the late epidemic, it was found to be in good order. The ventilation, also, of the cells had been improved; and a small open fire was placed in each of the day-rooms. Out of 1100 prisoners, there was not a single instance of cholera, and only a few cases of diarrhoea, which speedily yielded to prompt treatment. Bridewell prison afforded equally satisfactory results. In 1832 it was in a most filthy state, and the prisoners were much crowded. Sixteen cases, four fatal, occurred in the epidemic of that year. The sanitary arrangements of the prison have since that period been rectified; and while the pestilence raged on all sides of it, in houses separated only by a narrow wall, no case of cholera took place, though fresh prisoners of the very lowest class were daily brought in. There was only one case of the malignant form of the disease in Horsemonger-lane Gaol, which is situated in a district that suffered most severely.

The two public Metropolitan Lunatic Asylums of Bethlem and Hanwell escaped without loss of life, although cholera prevailed extensively and severely within a hundred yards of the former, and the latter was visited with a rather sharp attack of diarrhoea, showing clearly that the morbid influence was there. In most of the larger private establishments around the metropolis, more or less severe invasions occurred; and in some the mortality was considerable. Several of these, however, escaped altogether, or nearly so. Exact information as to each establishment is wanting; but Mr. Grainger says: “that in some which he visited, the water-closets and urinaries were in an offensive condition.” We suspect also, from what we have observed, that sufficient space is not allowed for each inmate in the dormitories. Mr. Grainger alludes to the great mortality which took place in the West Yorkshire Lunatic Asylum. Since his report was published, the particulars have been made known in Dr. Wright’s



work: they deserve serious attention. No fewer than 132 cases of cholera occurred, out of a population of only 620; and so virulent was the disease, that four-fifths of the attacked died.\* The institution appears to have been more than usually sick, for upwards of eighteen months before the outbreak. Diarrhoea and dysentery had prevailed to a considerable extent; and, indeed, there would seem to have been a marked tendency to bowel complaints among the inmates for a number of years. Independently of the deaths from cholera, the mortality in the asylum in 1849 was higher than it had been known for a long period; the deaths from diarrhoea and dysentery had increased in 1848-9 from 8 to 11 per cent. There cannot be a doubt, we think, that overcrowding had much to do with the exceeding severity of the choleraic invasion; repeated allusion is made to the subject in Dr. Wright's narrative. Besides want of sufficient space, defective ventilation, and the mal-position of some of the dormitories, there is evidence that the vitiation of the atmosphere of the wards from foul effluvia aggravated the evils most seriously. The sanitary arrangements of several of the wards and dormitories were obviously very faulty. As to the drainage of the asylum being "unexceptionable," the report of Messrs. West and Dawson clearly proves it to have been otherwise, and to stand very much in need of correction. Yet these gentlemen (not medical, we presume,) do not hesitate to declare that in regard to it, as well as to the ventilation, &c., they have nothing to recommend for improvement, nothing which can account for the appearance, or increase, or mortality of disease. We should strongly recommend Dr. Wright to correspond with Mr. Wakefield, the medical officer of the Cold Bath Fields Prison, respecting those measures, the adoption of which has proved so beneficial in that establishment. We must not forget to state, that not a single case of cholera occurred among the staff of the resident officers and servants of the asylum, amounting to about fifty in number, although most of them were affected with diarrhoea.

We shall very briefly notice a few examples upon a larger scale, of the great benefits derivable from sanitary improvements.

Hamburg, it will be remembered, suffered very severely in 1832. In 1842 occurred the great fire, by which nearly one third of the city was reduced to ashes. The part destroyed has been rebuilt with due regard to drainage and other sanitary requirements; and what was the result when the epidemic appeared in 1848? "The rebuilt part," says Mr. Grainger, "has experienced an exemption from cholera, which is as remarkable as it is important. All the medical men with whom I conversed upon the subject, expressed themselves unequivocally to this effect: . . . . comparing the poor residing in the rebuilt parts of the town with those living in the old portion, not more than one of the former had been attacked with cholera for ten of the latter."

In our own country there have been some well-marked examples of the same gratifying results. Exeter affords a notable illustration. Dr.

\* In one ward and its dormitory, which it is obvious from what is stated had been noted for insalubrity in the preceding year, "no less than 87 per cent. of its inmates were affected with epidemic disease; 29 per cent. with diarrhoea; 6 per cent. with dysentery; and more than one half (51 per cent.) with cholera; of whom eight-tenths died. Of sixteen cases of cholera in No. 3, ten were individuals that had about that period been sleeping in the basement story; only one of these recovered. Six were found collapsed on opening their room-door in the morning; and all died."

Shapter's work is full of interest on this point. In 1832, no fewer than 402 persons perished of the epidemic; and a vast amount of suffering, as well as heavy expense, was inflicted on the town. But the terrible lesson was not lost upon the inhabitants. Since then, important sanitary improvements have been effected; one of the greatest and best is an ample supply of good water in every part. In his preface, which is dated 10th July, 1849, and therefore before the late pestilence had generally manifested itself in the West of England, Dr. Shapter expresses his confident hope, in consequence of the improved condition of the city, that the cholera would be far less severe, should it reappear, than on its former visitation. "I am," says he, "the more strengthened in this view, from my own experience of the *present rarity of fever*, which, previously to the improvements referred to, was not unfrequent; the change in this respect has been very remarkable." From Dr. Pennel's statement in the 'Provincial Journal,' we learn that in 1849 not more in all than ninety-one cases of cholera occurred in Exeter, and that of these upwards of one half took place in the single parish of St. Edmund, in a low unwholesome district near the accumulations of a main drain from the city, and "immersed in putrid exhalations." Dr. Pennel ascribes the mitigated severity of the invasion, compared with that of 1832, to improved drainage, the pulling down of old houses, the removal of nuisances, and greater general attention to the sanitary condition of the poor.

Nottingham affords a still more instructive example. Upwards of 1000 cases of cholera, of which nearly 300 were fatal, occurred there in 1832. At that time Nottingham, like Exeter, was very badly supplied with water, besides being ill-drained, extremely filthy, and very densely populated. The ravages of the pestilence were confined in a great measure to the worst localities, the higher and better conditioned district escaping almost entirely. An immense deal has been done of recent years to improve the town. It enjoys an almost unlimited supply of wholesome filtered water, forced by day and night by high pressure along every street, and capable of rising to the upper stories of almost all the houses without cessation throughout the year. Besides this inestimable blessing, the labours of a Sanitary Committee, appointed by the Town Council three years before the recent epidemic, were highly successful in removing nuisances, improving the state of the dwellings of the poor, &c. And now mark the result. In the autumn of 1848, a severe attack of cholera took place in a filthy village within five miles of Nottingham, but that town remained entirely exempt; nor did a single instance of the pestilence occur there until December in the following year,—although there had been much diarrhoea during the season, a clear proof that the epidemic influence had been felt,—when five fatal cases occurred. It is worthy of notice that several very malignant cases took place in the course of last autumn in a notoriously filthy locality; but, as on the former occasion, the progress of the disease was promptly arrested by the immediate correction of the local causes of insalubrity. Dr. Hutchinson has favoured us with an interesting communication, wherein he expresses his decided conviction that "the improved sanitary condition of the town from that of 1832 to 1849 has much mitigated the severity of the cholera epidemic of the latter year, moderated its diffusion, but not its individual intensity, or the number of fatal cases in proportion to those attacked." The remark is important, as

indicating that the virulence of the aërial miasm was quite as great as in 1832, but that its development was much less frequent; in consequence, we may presume, of the comparative absence of those predisposing and determining local causes, which it has been the main object of this article to bring strongly under the attention of the reader. We need scarcely say, that these causes are the nurse, not the parent, of the pestilence; they serve to hatch or produce, not to generate, its poison-germs.

Never was there a disease in which the finger of Providence has been more conspicuous than in Epidemic Cholera. Its whole career manifests that it is a Divine judgment,—not, indeed, an infliction of wrath, but a chastisement of rebuke. We have been long neglecting our duties to the poor, and to those that cannot help themselves. Thousands upon thousands are annually swept away by diseases, which are more or less under control or even prevention. This solemn fact has been known, not of late years only, but for the last century. What did Dr. Rush say between sixty and seventy years ago? his words are pregnant with meaning:—“To all natural evil, the Author of Nature has kindly prepared an antidote. Pestilential fevers furnish no exception to this remark. The means of preventing them are as much under the power of human reason and industry, as the means of preventing the evils of lightning and common fire.” And has not all medical experience confirmed the accuracy of this statement?

But have we acted in accordance with the duty which the knowledge of its truth imposes? or have we not rather, in spite of conviction, allowed the most monstrous physical evils to spring up in our towns and villages,—evils that inevitably produce disease, wretchedness, and death? A new and fearful messenger has been sent into our land to alarm and warn us. Hitherto, the judgment has been tempered with mercy; we have had but a simple duty to do, and but a few simple rules to obey, and the invasion of the pestilence has been comparatively innocuous. It only requires the will of Him who sent it, to intensify its virulence by one degree, and its poisoned breath may spread destruction even in the purest and healthiest localities. Who can say that the third visitation may not come armed with treble power? Does not the history of the ways of Providence, in past ages, lead the thoughtful inquirer to believe that such will be the case, if duties are left undone and privileges remain unimproved? All classes of the community are deeply interested in the inquiry. How much, then, should it engage, not only the serious attention, but also the active and persevering labours of the members of that noble profession, the great end and object of whose mission is to save life, to mitigate suffering, and to ameliorate the condition of their fellowmen?

There still remain many and most important matters connected with the subject of Cholera, in its relations to Sanitary Measures, to which we have not been able even so much as to allude in the present article. The consideration of these must be reserved for another occasion.

## ART. II.

*Traité Pratique de la Colique de Plomb.* Par J. L. BRACHET.

*A Practical Treatise on Lead-Colic.* By J. L. BRACHET.—Paris, 1850.  
8vo, pp. 290.

WE regard the institution of Prize-Essays, and especially when they are adjudicated by Academies and Colleges, as a very potent means of advancing medical science and art. It is true that the competition they excite gives rise to much profitless labour and great heartburning on the part of defeated candidates, and that our literature is burdened by some useless publications from the pen of those who have succeeded. Yet when we consider how many minds of great original power have thus first had their energies directed into particular channels of thought or experiment, by the suggestion of the hiatus to be supplied, or of the points needing additional illustration; or in how many instances facts and observations might have remained buried in the portfolio, had not the stimulus thus supplied induced the observer to arrange and produce them, in the hope that they would go before the public with the sanction of the approval of competent judges;—it seems quite certain that the amount of resulting good much preponderates. On the present occasion the prize was offered by the Toulouse Academy of Sciences; and M. Brachet of Lyons, already well-known by his publications on hysteria, hypochondriasis, and other subjects, was the successful competitor. The subject may seem a somewhat hack-nied one, and we do not find that he has contributed anything which may be called new concerning it; but in respect to a disease concerning which so many contradictory opinions have prevailed, any contribution from observers of acute minds and large experience may prove of great utility. We regret that the programme of the Academy limited consideration to Lead-colic, in place of embracing the other forms of disease engendered by the ingestion or inhalation of this metal; but M. Brachet has rigorously confined himself to the question delivered for discussion, and to this alone we can now direct attention. The programme required an exposition, according to the present state of our knowledge, of the nature, seat, diagnosis, and treatment of Lead-colic; and M. Brachet was not unprepared for the discussion. Medical adviser for above thirty years to one of the trades (tinman) that furnishes a great number of cases of lead-colic, and physician to a large hospital, his views are based upon more than 300 cases which have come under his notice. In 1824 he published a *brochure* upon the subject, of which the present Essay may indeed be regarded as an amplification.

In order to display what is the actual state of our knowledge of this disease, the author enters into a critical examination of the views of the various writers who have directly or incidentally treated of it; and this portion of his work possesses considerable bibliographical value. We need not here follow him amongst all the contradictory and controversial views which have prevailed, the chief point of discussion being whether there has been only one form of endemic colic that is produced by lead, or whether the Poitou, Devonshire, Madrid, &c. colics have not been due to the operation of other causes, at least conjointly if not exclusively. Whether, too, the treatment of the disease ought to be evacuant, anodyne,

antiphlogistic, or specific, or a combination of these, has been a fertile subject of contention. Although many valuable essays had illustrated these different points in every variety of modes, the first complete dissertation upon the disease was published by Merat, first in 1804, and then in 1814; the last, and by far the most complete work, treating indeed of poisoning by lead generally, being that of Tanquerel-des-Planches.

If there has been much discrepancy of opinion respecting the causes and treatment of the disease, there has been, perhaps, still greater in regard to its *nature and seat*. Into these discussions M. Brachet enters at great length, but we must content ourselves with his summing up :

“Baumes, Gendrin, Borghi, Giacomini, Sandras, Bouchardat, Legroux, Bouillaud, Triberti, Mialhe, &c., have considered it a general disease, a toxicosis, a poisoning of the entire economy. Combalusier limits the toxicosis to the presence of lead in the *primæ viæ*, and has many followers.

“The majority have placed its seat in the nervous system, either in a general manner, or by specifying certain portions. Thus Macbride, Cullen, Vogel, Vitet, Pinel, the two Franks, &c., make it a nervous affection. Willis and Lepois refer all to the brain, and Renauldin to the brain and spinal marrow. Astruc, Sauvages, Lamure, Barbier, and Serre, localise it in the spinal marrow. Dehaen, Vantroostwyk, Tronchin, Ranque, Tanquerel, Orfila, Grisolle, and Piorry, consider it as a lesion of the nerves of the abdomen, or of the great sympathetic, either in its totality or in its abdominal lumbar diaphragmatic or intestinal portion. Brachet, Anquetin, Andral, and Galtier, regard it as a lesion of both the cerebro-spinal and the ganglionic nervous systems.

“Of all the organs, the intestines have been most generally regarded as the seat of the disease, either in a general manner, as by Stoll, Bordeu, &c.; or in a more particular one, as the mucous membrane by Stockhusen, Gardane, Desbois, Palais, Broussais, Roche, &c.; or the muscular coat by Merat. Boisseau makes the stomach participate, and Darwin the liver. Dubois places the scene of the drama in the mesentery, Sauvage in the peritoneum, Giacomini in the abdominal muscles, and Legroux in the hepatic circulation. Each author has been led to his localisation either by particular facts or theoretical reasoning. All furnish good reasons for their conclusions; but a deficiency in the number of facts has prevented them from observing the disease in its entirety, and has led them to generalise from too limited premises.

“We have seen that while a number of authors have only considered the affection as a poisoning, the majority regard it either as a general or local nervous affection. Bordeu, Henckel, Palais, Roche, Renauldin, &c., believe they can recognise the marks of inflammation. Stoll regards it as a specific disease *sui generis*; and such is the opinion of Brachet, as also apparently of many others, if we may judge by the difficulty they have of disposing of this disease in their classifications. We may observe that in this long series of opinions, almost every one admits a modification of excitement, irritation, or inflammation. Three, however, think differently, Darwin regards it as a diminished action of the intestine, Merat as a paralysis of the intestinal mucous membrane, and Giacomini as an hyposthenic affection.” (p. 57.)

In investigating the validity of these various opinions, M. Brachet enters upon an examination of the degree of assistance there is obtainable from *pathological anatomy and chemical analysis*. In respect to the first, while several authors have appealed to it in proof of the inflammatory character of the disease, a much greater number oppose this view, on the ground of the very large number of cases in which no appearances indicative of this are found, and the fact that when present they are often due to the existence of complications or the effect of treatment. The general result of



observation is the absence of important material lesion, the intestinal canal being found for the most part much contracted around its contents. The lesions sometimes met with in various parts of the body, tend rather to oppose than to support the views of localisation that have been endeavoured to be set up. However it may be with regard to acute poisoning, *chemical researches* for the presence of lead in the disease before us, have been too uncertain and too contradictory in their results, even in the hands of the most expert chemists, to enable any certain deduction to be founded upon them as yet.

Deriving no aid from these two handmaids of medical science, M. Brachet betakes himself to what he terms the *physiological analysis*, or examination of the pathognomic phenomena of the disease. These are found to be,—pain of a very peculiar character, obstinate constipation, with a retracted state of the abdomen, and a remarkably dry condition of the internal intestinal surface, the contents of the canal being deprived of that lubricity so favorable to their passage. The pulse, too, is slow and contracted, while the diminution of the various secretions produces a jaundiced and dry skin, and sparse urine. The *pain* and the contracted and inactive state of the intestinal canal, which produces the *constipation*, must be due to lesions of the cerebro-spinal nervous system, while the defective secretion from the mucous membrane giving the dryness and globular form to the contents of the intestinal canal, and in part producing the constipation, is dependent upon a perverted condition of the ganglionic nervous system. To this, operating through the heart, is also due the small and contracted pulse. This view of the seat of the disease is the same as that published by M. Brachet in 1824, and it is amusing to see him now quoting himself to be duly commented upon and criticised, necessary as this was when the essay was delivered in anonymously for the appreciation of the judges. In respect to the *nature* of the affection of these nervous centres, the author agrees with Stoll (whose opinion, “*non dubito hanc colicam esse specificam, et sui generis*,” he adopts as his epigraph), that lead-colic is a specific disease, one, *sui generis*, resembling no other disease and no other colic. The adstrictive and styptic power of lead is generally admitted, as well as its sedative power upon the nervous and circulatory systems; and long exposure to its emanations induces the perversion of the nutritive functions, manifested in the cachexia or marasmus induced. In this way the substance acts deleteriously upon both orders of vital actions, those which are dependent upon the cerebro-spinal system, and those which are influenced by the ganglionic system. To the latter belongs the adstriction of the exhalants and capillaries, the slow circulation and the vitiated nutrition; to the former, the convulsive, painful, and paralytic affections. Thus far, however, we have only a general affection of the two nervous systems; but the study of the phenomena of the disease teaches us that it is especially the nerves of the intestines in which the phenomena are produced, and to which therapeutical agents must be directed. The intestines receive nerves both of animal and organic life; and it is to the lesion of the former that the terrible pain and the contracted state of the intestines are attributable, while the lesion of the latter explains the absence of secretion.

The *diagnosis* of this disease is not in general a matter of difficulty, at least unless through neglect or bad treatment complications are set up. The

peculiar character of the excessive pain, relieved instead of augmented by pressure, and the obstinate constipation, joined to the history of the case, usually suffice. M. Brachet believes M. Beau's observations upon the presence of *anæsthesia of the surface* in these cases to be inexact, or at all events to require farther confirmation. The *blue mark along the gums* indicated by Burton, Schebac, and Tanquerel, would be pathognomic if it were always present. It is never seen but in those who have been brought into relation with lead, and it is almost always found in workmen who employ white lead, but M. Brachet has often found it absent in tin-workers. Differences in this respect may be due to the different modes in which the lead is introduced into the economy. Thus most of the cases in which the mark has occurred, have been observed in white lead manufactories, in which it has access to the mouth and respiratory organs; while those persons who only bring the metal in contact with the hands (as the workers in tin), or take it medicinally, manifest the mark much more rarely. Both Burton and Martin Solon have produced the colour, however, through the internal use of the acetate of lead.

We need not accompany M. Brachet in his examination of the distinctive characteristics of cholera, peritonitis, enteralgia, ileus, hepatic and bilious colic, and calculous nephralgia; but a few of his remarks on *copper colic* and *vegetable colic* will not be superfluous. Many authors have noticed that the workers in copper become affected with a colic very analogous to that produced by lead; and some, as Merat, Palais, &c., have gone so far as to assimilate them under the common term metallic colic. M. Brachet has frequently met with colic induced by copper, but has always observed this to be very different in its manifestations. The copper-workers, however, who make use of solders into the composition of which lead enters, exhibit all the signs of lead-colic. As to colic resulting from the emanations of copper, it must be very rare; for during the great number of years in which the author has practised at Lyons, which contains more than 2000 copper-workers, he has never met with such a case. The cases of colic he has met with have arisen from the copper having been accidentally swallowed, and then the horrible character of the pains, the *tormina* or internal twistings, is characteristic. There is no retraction of the belly, and pressure increases the pain; while neither obstinate constipation, nor the blue colour of the gums are present.

As to *vegetable colic*, much controversy still prevails; some believing the Poitou, Devonshire, Madrid, Surinam, &c., colics, due to the lead accidentally or purposely introduced into beverages which seemed to be the immediate causes of these; and others maintaining them to be examples of another form of disease. The investigations of Baker in regard to the English ciders, and of Bouvard with respect to the adulteration of the wines in France, with others elsewhere, pointed to lead as in all similar instances the active cause of the symptoms: but additional observations, and especially of the French while in Spain, have shown that similar symptoms may be produced endemically from some seasonal influence, independently of the ingestion of any injurious article of diet; the terms vegetable and lead colic being then equally inapplicable.

M. Brachet's chapter on the *treatment* of the disease is a very long one, occupying nearly half the book; for in it he enters into an investigation of the numerous modes of managing the disease which have been from



time to time recommended. As our readers must be familiar with these, we shall pass them over very summarily, detailing only that which the author has from repeated trials at last definitively determined to adhere to. The remedies which have been employed may be classed under the heads of purgatives, anodynes, antiphlogistics, and specifics. With respect to *purgation* its great efficacy has been known from a remote period. The treatment followed at La Charité, which for so many years was so renowned, consisted of an elaborate system of repeated vomiting and purging, of a most harassing description, which no physician would propose, and no patient submit to, at the present day. It is effectually replaced, as far as purgation is concerned, by croton-oil, of which M. Tanquerel is a warm champion, giving a drop for a dose, in tisane in preference to pills, three days in succession. When M. Brachet has employed it, he has found one and a half or two drops to be required for each dose, and this to be repeated for from four to eight days. It is now very generally used in the Parisian hospitals. *Anodynes* were first employed as a special means of treatment by Stoll, large doses of opium inducing rapid relief, though, unless other means be employed, this is rarely permanent. M. Brachet regards it as unfit for exclusive treatment, but as a powerful auxiliary. The *antiphlogistic* treatment, save for the removal of accidental complications, meets with little quarter at our author's hands.

Among the *specific remedies*, many of a chemical character have been suggested, with the view of neutralising the lead and eliminating it from the economy; but the chemists, supposing its existence proved, have not always advocated the same means, some desiring to effect soluble, and others insoluble compounds, with the poisonous substance. Alkalis, oxygen, sulphur, hydrosulphurets, sulphuric acid, mercury, antimony, and a great many other substances, have been in turn recommended and abandoned. The last of this class was brought forward by M. Melsens, viz., iodide of potassium. Supposing this substance to prove as efficacious in the treatment of chronic lead-poisoning, as he states it to be, it does not follow that the chemical explanation of its efficacy which he offers should be the true one, as its great power in modifying the morbid actions of the economy is familiarly known. It is to some such modification of the pathological condition, that the great efficacy of *alum* must be due; and in this sense it may be regarded as a specific, and measured by its utility, as the most valuable specific. Notwithstanding that we have enumerated the various remedies separately, it is usually upon a combination of two or more of them that most authors have relied.

M. Brachet likewise employs a combination of remedies, but it is to *alum* he looks as his sheet-anchor. During the first eight years of his practice, he employed the La Charité treatment, and with complete success as far as a cure was concerned. He more than once reflected, however, upon the probable ill effects of so violent a system of purgation, and was displeased at the tardiness of the cure, which never occurred before six and frequently not until twelve days. When the physiological system of medicine was in the ascendant, he had recourse to antiphlogistic treatment, but with such indifferent success that he was obliged to fall back again upon the empirical use of purgatives. Next, more than 40 cases were treated by leeching the abdomen, and full opiates. The patients were relieved in a few hours, or at the farthest in two or three days, and he

believed he had attained his object. In other cases, however, the cure was very slow, and in others again not permanent, although purgatives succeeded to the narcotics. After further attempts he had recourse to *alum*, as recommended by Gendrin, and was able in 1838 to report highly in its favour; and since that period he has constantly employed it, without accident or disappointment occurring. He prescribes from  $1\frac{1}{2}$  to 2 drachms in a ptisan, to be taken during the day, adding to this 40 or 50 drops of laudanum, and if the bowels do not act by the third day, giving a mild aperient: and the case is complete. More than 150 patients have been thus treated with complete success, the alum being continued a day or two after the symptoms have disappeared. This treatment, realizing the *citò, tutò, and jucundè* of Celsus, after so long a trial of others less efficacious, he now recommends as one not likely to be improved upon.

## ART. III.

1. *Alcoholismus Chronicus eller Chronisk Alkoholssjukdom ett bidrag, &c. &c. Chronic Alcoholism, or the Chronic Alcohol-Disease: a Contribution to the Knowledge of the Malady, &c. &c.* By Dr. MAGNUS HUSS, Professor of Clinical Medicine, &c. Part I. —Stockholm, 1849. 8vo, pp. 194.
2. *Voorlezing over den Invloed van Sterken Drank op het Ligchaam. Voorgedragen in het Natuurkundig Gezelschap te Utrecht.* Door J. L. C. SCHRÆDER VAN DER KOLK.—Utrecht, 1850.  
*A Lecture on the Influence of Strong Drinks on the Human Body, delivered before the Society of Natural History of Utrecht.* By J. L. C. SCHRÆDER VAN DER KOLK.—Utrecht, 1850. 8vo, pp. 57.
3. *The Physiological Effects of Alcoholic Drinks, from the 'British and Foreign Medical Review;' with Documents and Records of the Massachusetts Temperance Society, illustrating the Origin of the Temperance Reformation, and its Progress in the State of Massachusetts.*—Boston, 1848. 12mo, pp. 191.
4. *On the Use and Abuse of Alcoholic Liquors in Health and Disease. Prize Essay.* By WILLIAM B. CARPENTER, M.D. F.R.S. F.G.S. &c. &c. —London, 1850. 8vo, pp. 283.
5. *Temperance and Total Abstinence; or the Use and Abuse of Alcoholic Liquors in Health and Disease.* By SPENCER THOMSON, M.D., &c. &c. London, 1850. 8vo, pp. 184.
6. *Report of the Committee appointed to inquire into the Expediency of diminishing the present Quantity of Spirits, served out daily to the Seamen of the Royal Navy.* (Parliamentary Paper.)—1850. Folio, pp. 132.

A glance at the above list of titles will show, that the subject of the physiological and pathological effects of alcoholic liquors is occupying the attention of distinguished members of our profession in various countries; and as it has been recently the subject of much discussion in our own, we shall not apologise for bringing it under the consideration of our readers. It is far from our present intention, however, to enter upon the discussion of the entire question, or, rather, series of questions, comprehended under the general head of the "Use and Abuse of Alcoholic Liquors;" for this

even in its purely medical relations, would occupy much more space than we can allot to it ; and, when considered in its social and moral aspects, is one of the most extensive, as well as deeply interesting topics of inquiry to which the attention of the philanthropist can be directed. There are several points, however, which are mooted in the works before us, which will possess some novelty even for those who have already well considered the subject ; and it will be our special object to bring these prominently forwards, whilst we also throw out a few suggestions as to the attitude which the profession in this country may most advantageously assume in regard to the "temperance movement." In the first place, however, we shall give a short account of each of the works upon our list.

It appears, from statistical data, that Sweden is entitled to the "bad eminence" of being the most drunken country in Europe ; and the social and physical evils of the abuse of alcoholic liquors among *all* classes of society have recently attracted attention, on the part alike of the Government and of the medical profession in that kingdom. A grand temperance assembly, held not long since in Stockholm, was attended, personally, by the King and Queen of Sweden ; and the King, on that occasion, not only gave in his adhesion, and that of the Queen, to the principles and practice of the society, but offered full compensation to all distillers who would abandon their production of ardent spirits, which (we are informed) has been accepted by many ; besides which, he has employed, at his own cost, four able and zealous missionaries, to traverse every portion of the country, and to endeavour to awaken the people to a sense of the evils of intemperance, and the blessings of a temperate life, both to individuals and the community. At the same time, Dr. Huss, the distinguished Professor of Clinical Medicine in the University of Stockholm, makes public the results of his long-continued observations upon a certain peculiar form of disease, to which he has given the name of *Alcoholismus Chronicus*, and which he attributes to the abuse of spirituous liquors. Of this treatise, we shall presently lay an analysis before our readers, and shall then inquire how far the curious group of symptoms, forming the malady described by Dr. Huss, can be legitimately set down to the agency of alcohol.

Of Professor Schröder Van der Kolk's world-wide reputation, as a scientific pathologist, it is not requisite that we should inform our readers ; and the fact that the subject has been specially brought before the community of Holland by a man of such acknowledged eminence, will give to the movement, we should hope, not only in that country, but in others, a claim to the attention of the medical profession, which it might not otherwise have presented. His discourse relates especially to the use of distilled spirits, and contains a clear enunciation of the grounds upon which science and morality wage war against the use of them as articles of diet ; but he takes occasion, also, to show the inutility of alcohol under any form, in regard to the purposes which it is commonly reputed to answer. He first shows that spirituous liquors, instead of assisting the process of digestion, directly impede it ; and he then exhibits their injurious effects upon the mucous membrane of the stomach. He follows up the attack, by examining the secondary results produced after absorption, and proves that, as the process of oxygenation and depuration are necessarily impaired by the employment of ardent spirits during health, so all the powers of the body and the intellect are gradually undermined by the

“firewater.” He quotes the experiments of Müller, Schwann, Prout, Wagner, Frerichs, and others, in support of the arguments adduced; whilst he draws largely upon the article contributed by Dr. Carpenter to Dr. Forbes’s Review, for proofs of the fallacy of the common idea that spirituous liquors can afford assistance in the endurance of cold, or in the sustenance of bodily labour. In fact, a large part of his lecture consists of selections from that essay; to which he also refers for illustrations of the sanitary and moral effects produced by the abuse of alcoholic liquors in this country, more especially in regard to the connection between intemperance and crime. This connection he regards as clearly established, in Holland, by the statistics with which he has been furnished by several gentlemen, whom he regards as authorities upon the subject; thus, Heer de Bosch Kemper informs him that, by tracing the first beginnings of crime, he satisfied himself, that as many as ninety offences out of every one hundred were referable, directly or indirectly, to the influence of spirituous liquors. The Professor complains of the great opposition which the temperance movement meets with in Holland, where, if we gather rightly from his introductory remarks, the habit of taking an appetising *petit verre* before meals, has rendered the use of spirits one of the daily household customs. The injurious effects of habits of intemperance, he informs us, are forcing themselves upon the attention of the Legislature in another form,—that of a steady and continuous increase of pauperism. It appears, from a report made to the Dutch House of Commons, in 1849, that in 1841 the proportion of persons receiving public relief was 106 out of every 1000 of the population; in 1845, it was 142·58; and in 1847, it had risen to 160·82; one of the chief causes assigned being the increasing abuse of spirits. Prof. Schroeder Van der Kolk concludes with an earnest appeal to his countrymen to promote, by every legitimate means at their command, the cause of temperance.

The third work on our list consists in part of a reprint of Dr. Carpenter’s essay, already referred to, with the addition of documents illustrating the early history of the Temperance movements in the United States, “showing how, from a feeble and vacillating origin, these movements gradually acquired strength and impetus sufficient to carry this reform through a considerable part of the civilized world.” The publication is issued by the “Massachusetts Temperance Society,” of which we observe that the President is the distinguished surgeon, Dr. Warren (of Boston, N.E.), whilst the Secretary is Dr. Walter Channing; and we learn from the preface (as well as from private sources of information), that the cause has been warmly espoused by Mr. Everett, who was for some years the American Minister at the British Court, as well by the Governor and Lieutenant-Governor of the State of Massachusetts, the Presidents of its three Universities and Colleges, and the Mayor of the city of Boston. In fact, it appears that “temperance opinions” in New England are now considered rather as recommendation to office, instead of being looked upon, as formerly, in the light of a disqualification. It is not surprising, then, that, with such influential sanction from the medical and government authorities, the movement should have made great advance in New England; and that the habitual consumption, not merely of spirits, but of wine, beer, and cider, should have been entirely abandoned by a large proportion of the community, and that the use of wine should have been

relinquished at many of those public celebrations, at which it was formerly considered essential to social enjoyment. "The influence of such a change of habit among the wealthier classes," we are told, "has been great beyond calculation, in leading the mass of society to abandon the use of spirits, and to repeat an experiment, already made by those whom they are accustomed to respect and follow."—We learn from the record of the proceedings of the Society, which forms a large part of this little volume, that it originated in 1811; and that its first object was, "to discountenance and suppress the too free use of ardent spirits, and its kindred vices, profaneness and gaming; and to encourage and promote temperance and general morality." After an experience of more than twenty years, however, it was led to recognise the principle of "total abstinence," as the means by which the desired reformation may be most effectually attained; and this recognition it has expressed at various times, without making the signature of the "total abstinence pledge" a condition of membership. In this way it has done great good; cordially concurring with and encouraging those societies and individuals which have adopted the more stringent principle, and drawing into itself a large amount of valuable co-operation, which would not have been directly afforded to them.

The similarity in the titles of the two succeeding works arises from the circumstance, that they were competing Essays for a Prize of One Hundred guineas, to be given for the best essay on the use of Alcoholic Liquors in Health and Disease. The donor of the prize (Mr. Eaton) propounded the four following queries, to be answered by competitors:—1. What are the effects, corporeal and mental, of alcoholic liquors on the healthy human system?—2. Does physiology or experience teach us, that alcoholic liquors should form part of the ordinary sustenance of man, particularly under circumstances of exposure to severe labour, or to extremes of temperature? or, on the other hand, is there reason for believing, that such use of them is not sanctioned by the principles of science, or the results of practical observation?—3. Are there any special modifications of the bodily or mental condition of man, short of actual disease, in which the occasional or habitual use of alcoholic liquors may be necessary or beneficial?—4. Is the employment of alcoholic liquors necessary in the practice of medicine? If so, in what diseases, or in what forms and stages of disease, is the use of them necessary or beneficial? These four questions were answered by fifteen competitors, including the authors of the essays under notice. The adjudicators—Dr. Forbes, Dr. Roupell, and Dr. Guy—unanimously adjudicated the prize to Dr. Carpenter; they also thought it due to Dr. Thomson, to record their opinion of the great merit of his essay, and to express their belief, that the cause of temperance would be benefited by its publication; and they further thought it right "to speak in terms of commendation" of a third essay, which has not, however, made its appearance. Dr. Carpenter's essay became the property of Mr. Eaton, and is published according to one of the conditions under which he offered the prize; Dr. Thomson has published his, in compliance with the recommendation of the adjudicators.

• The last work on our list is the Report of a Committee of Flag and other Officers, recently appointed by the Lords of the Admiralty, "to inquire into the expediency of diminishing the present quantity of spirits served out daily to the seamen in the royal navy;" the precise object to which



the inquiry was directed being, to determine "whether a reduction might not be made in the daily ration of spirits, with benefit to the seamen, and with advantage to the discipline of the fleet."—Everyone knows that there is a great difficulty in maintaining discipline on board a man-of-war. A large number of men are penned up together, within very narrow limits, and are, for the most part, not sufficiently employed to keep them out of mischief. However orderly and well-disposed the great bulk of a ship's company may be, there are always a set of *mauvais sujets*, who are continually giving trouble by their insolence and insubordination, and by inciting others to the same conduct. To keep this in check, the "cat" has been regarded by every naval officer as his grand resource; and to all remonstrances on the degrading consequences of its use, he has had but one reply,—“How can you maintain the discipline of the service without it?” Of late, however, the feeling which has been extending among intelligent men of all classes and parties, that the best escape from the difficulties which surround the whole question of *punishment* is to be found in the *prevention* of wrong doing, appears to have taken root in the Admiralty; and a suspicion seems to have arisen,—and to have been, perhaps, augmented by the body of evidence collected from the charges of judges, the statements of police-magistrates, jailors, &c., as to the relation between intemperance and crime, and widely diffused by the teetotal advocates,—that the spirit-ration might have more to do with the difficulty of maintaining discipline, than was commonly supposed. The Report issued by the Committee is founded on the examination of no fewer than fifty witnesses; forty-five of whom hold various ranks in the government service, from that of captain and lieutenant down to that of petty officer and able-seamen (no fewer than seventeen of the latter class having been examined); whilst four hold appointments in the merchant-service; Dr. Carpenter being the only witness not personally familiar with the requirements of the marine life, and even he (as we learn from his evidence) not being altogether ignorant of them, having made a voyage to the West Indies and back in a "temperance-ship." The evidence, therefore, may be regarded as that of a body of *practical* men, having no theory or system to support; all their class-associations, as well as their individual predilections (so far as appears from their statements), being in favour of the spirit-ration; and nothing but the experience of its results leading them to a different conclusion.

I. Two great hygienic movements divide the attention of the public; the one having reference to Public Hygiène, or to the sanitary condition of the people, as injuriously affected by agencies having an endemic or epidemic character (we use the words in their primary signification); the other bearing on Individual Hygiène. The water-supply and sewerage of our towns and villages; the drainage of the soil; the construction and arrangement of our dwellings; the disposal of our dead; these are large and important questions of public hygiène, now in progress of solution. The establishment of public baths and wash-houses is an important measure bearing on private hygiène, although a measure undertaken by the community; but the largest scheme of this kind is that which would influence the dietetics of the people, especially in reference to "alcoholic liquors," the fulfilment of which is sought by the united efforts of individuals, through



the agency of "Temperance Societies," and by an active press and a widely-diffused literature. To this class of hygienic reformers, Mr. Eaton evidently belongs: and we cannot but congratulate him, and the individuals with whom he unites his efforts in this field of hygienic moral culture, on the enlarged spirit displayed in the questions propounded, and on the practical tact by which the most comprehensive of human sciences is enlisted in support of their efforts.

The progress of these questions in public and private estimation deserves the serious consideration of those members of the medical profession, who have a regard for its true dignity, and who look beyond the narrow circle which the professional man of other classes, as well as the medical, is apt to tread. The pernicious abuse of alcoholic liquors has been long well-known to the profession. "Delirium tremens" and insanity, "gin-liver" and dropsy, have long been associated with intemperance; and practitioners have never been wanting with the courage to raise their warning voice against the vice, both in public and private. Nor has the necessity for a better sewerage of our towns, a better drainage of our soil, or a more rational disposal of our dead, been less recognised or less emphatically asserted. A glance at Hogarth's works is sufficient to show that these efforts were not in vain; for he fully comprehended the horrors which accompany a career of drunkenness. His "Gin Lane," published in 1751, is as graphic as Cruikshank's "Bottle;" and although, in the accompanying picture of "Beer Street," the contrast is meant to be favorable, in deference to the public opinion of the age, which connected British beer and British patriotism, the three balls of "Pinch, the Pawnbroker," the basket of books addressed to the trunk-maker (amongst which we recognise the 'Transactions of the Royal Society'), and the bloatedness of the figures introduced, constitute a covert satire on the then popular practice of swilling. That which the pictorial satirist depicted was well-known to the medical practitioners of the day, and lamented by many. The efforts made then and subsequently by the profession, to correct the evil, were almost without result; and it is only since the question has been taken up by intelligent and enthusiastic laymen, that the vice of intemperance has received anything like a check. We think the subjoined passage from Dr. Carpenter's preface conveys opinions, from which few in the profession will dissent:

"The experience of every practitioner must bring the terrible results of Intemperance frequently before his eyes; but, whilst he is thus rendered familiar with its consequences as regards *individuals*, few, save those who have expressly inquired into the subject, have any idea of the *social* evils resulting from it, or of the degree in which they press upon every member of the community. The author believes that he is fully justified in the assertion, that among those who *have* thus inquired, there is but one opinion as to the fact, that, of all the causes which are at present conspiring to degrade the physical, moral, and intellectual condition of the mass of the people, there is not one to be compared in potency with the *abuse of alcoholic liquors*; and that, if this could be done away with, the removal of all the other causes would be immeasurably promoted." (Preface, p. xiii.)

Dr. Spencer Thomson, with equal force, observes, that—

"No class of the community can be so practically alive to the terrible evils inflicted on mankind by the abuse of alcoholic stimuli, as the medical profession; and none ought more sedulously to guard against all that can encourage that abuse or unnecessary employment."

Dr. Thomson, however, guards himself, and warns his readers, against an intemperate disuse of an important class of remedial agents, observing :

“But, as generally happens, from one extreme we are in danger of running into another ; and enthusiastic men, some of them possessing high medical attainments, by advocating the total abandonment of alcohol even in the treatment of disease, and under all circumstances, are incurring a risk, not only of injuring the efficiency of practical medicine, but of damaging a good cause.” (Preface, p. xv.)

We are glad to observe, that both Dr. Carpenter and Dr. Thomson agree in advocating the utility of alcoholic stimulants under given circumstances, and dissent from the intemperate advocacy so justly characterised by Dr. Thomson. The ardent, impetuous, and extravagant, are, however, to be found amongst the opponents of every vice, and the teachers of every virtue ; for men who feel strongly, write and speak strongly and intemperately, unless their zeal be moderated by wisdom. Such conduct is to be regretted, because it renders a good cause less attractive, and by so much diminishes the utility of their services ; but the practitioner who keeps a steady eye upon his duty, and is resolved to fulfil it, will not permit these aberrations from propriety and good sense to turn him from the due advocacy of temperance. The evils of intemperance are too frequently before his eyes, and its terrible results too certain, to allow a maudlin sense of gentility to check his labours. Therefore, although “Teetotallers” be not quite so prudent in their zeal, nor so well taught in their enthusiasm, as to recommend their style of advocacy to his imitation ; and although they may pass the boundaries, not only of propriety, but of truth, in the advocacy of their cause ; the conscientious practitioner holds steadily on *his* way, and would rather suffer somewhat of obloquy, than yield in his “faithful” adherence to what is ethically right. We, therefore, consider that the authors of these Essays are entitled to the cordial thanks of the profession, for taking an enlarged view of their duties as medical practitioners, in reference to the use and abuse of alcoholic liquors ; and, while fully denouncing their abuse, assisting their legitimate use.

Precept and example, are, however, two very different things ; and it must be referred to the general questions of morals, whether medical practitioners shall teach by example ; and whether the temperate use of wine as a luxury, or as a social pleasure, shall be abandoned, or not. We make no profession of asceticism ; we do not “fast often,” or “mortify the flesh” in any way. The genial, cheerful view of Christian morals is, we confess, more to our individual taste ; we are thankful for God’s gifts, and endeavour to enjoy them ; and we, therefore, must plead guilty to the taking of wine, when in our own cases it simply conduces to social enjoyment, whilst even, perhaps, not *absolutely* or *altogether* harmless. Yet it rejoices us to see a man steadily abstinent, while others “quaff the foaming goblet ;” it is a noble victory over the conventionalisms of society, and possibly over temptation ; it is the most effectual style of teaching, provided the virtue be practised silently, or, at least, unostentatiously ; and it will help most powerfully to modify those social customs, which are too apt to be a snare to the joyous convivial companion. In this respect, indeed, the advocates of total abstinence have done essential service to society ; for, although we cannot justly attribute to their efforts the *whole* of that greater moderation at table, in which the present generation excels

the past, we are satisfied, that they can fairly claim *much* of it as their work.

We do not intend to give a full analysis of the several works before us ; but rather to dwell upon certain points of special pathological, hygiènic, or ethical interest. It is obvious, that the scope and arrangement of the English Essays were necessarily determined by the questions propounded ; and consequently, that there is a general similarity in the two. Dr. Thomson, however, differs from Dr. Carpenter in giving an introductory chapter on stimuli generally, and a second on the nature and properties of alcohol and alcoholic fluids ; and in these respects abandons the strict line of inquiry laid down. We are inclined to think that an additional question might have been propounded, and the attention of competitors directed to *the etiology of the desire* for alcoholic stimulation, or rather for artificial stimulation generally. That man is endowed with an instinctive desire of this kind, is a matter of common observation ; and we feel satisfied, that much new and important information on this part of the subject might have been brought together, had it come within the scope of the required conditions. Dr. Thomson, in his brief notice of stimuli generally, omits, we think, the main point, namely, the consideration of this desire. His object was rather to establish a proposition, on which he bases the opinions promulgated in his Essay, to the following effect. He maintains —

“That the healthy functions, both of body and mind, in man, are dependent for their continuance upon a regular series of ordinary stimulant actions ; and, further, that these functions are at intervals acted upon by occasional or extraordinary stimulants, which temporarily occasion their exalted and increased action, not only without actual injury, but with positive benefit. And, lastly, that these extraordinary stimulants are not liable to lose their power of beneficial stimulation, unless exerted in a disorderly and unrestrained manner.” (p. 11.)

Dr. Carpenter opens his inquiry with an investigation of the influence of alcohol upon the physical, chemical, and vital properties of the several components of the animal fabric ; judging that from such knowledge important assistance may be derived in estimating its effects upon the human system as a whole. After detailing various experiments, Dr. Carpenter deduces the following inferences :

“That alcohol, when applied to the living tissues in a sufficiently dilute form *exalts* for a time their vital activity ; but that this exaltation is temporary only, and is followed by a corresponding *depression*. And further, that when the alcohol is in a state of sufficient concentration to act more potentially, its exhausting or depressing effect is manifested, without any previous stage of excitement. This inference we shall hereafter find to be in precise accordance with that to which we shall be conducted by observation of the effects of alcohol upon the system at large ; and we are justified, therefore, in regarding alcohol as belonging to the class of *stimulants*, and as subject to the laws of their operation. It has been affirmed by some, that alcohol in small doses is *tonic* ; but of this there is no adequate proof.” (p. 7.)

In describing the phenomena of drunkenness, alcoholic intoxication, or, in the strictly etymological sense,—of poisoning by alcohol,—Dr. Carpenter points out that the successive phases of the phenomena may be referred to the successive action of the alcohol on the cerebrum, the sensorial ganglia, and the spinal cord ; and in discussing the pathology of alcoholic intoxication, he refers to the elective affinity, if we may be permitted the

phrase, which exists between alcohol and the nervous system,—a view which is also adopted by Dr. Thomson. The former observes on this point :

“The *general* stimulant action, which is for a time exerted by alcohol introduced in small quantities, and diluted by admixture with the general mass of the blood, is easily explained upon the basis of the observations first detailed ; but its special power of exciting the *nervous centres* to augmented activity, can only be accounted for by the idea of some special relation between alcohol and nervous matter. And this idea is fully borne out by the fact, that Dr. Percy found alcohol to exist in the substance of the brains of the dogs poisoned by it, in considerably greater proportion than in an equivalent quantity of blood. This fact is one of fundamental importance, as showing us how directly and immediately the whole nutrition and vital activity of the nervous system must be affected by the presence of alcohol in the blood ; the alcohol being thus specially drawn out of the circulating current by the nervous matter, and incorporated with its substance in such a manner as even to change (when in sufficient amount) its physical as well as its chemical properties. It is important also to observe, that this affinity is obviously such as will occasion the continual presence of alcohol in the blood, even in very minute proportion, to modify the nutrition of the *nervous* substance more than that of any other tissue ; for the alcohol will *seek out* (as it were) the nervous matter, and will fasten itself upon it,—just as we see that other poisons, whose *results* become more obvious to our senses, (although the poisons themselves may exist in such minute amount as not to be detectible by the most refined analysis,) will localise themselves in particular organs, or even in particular spots of the same organ.” \* (p. 20.)

The principle thus enounced by Dr. Carpenter is of considerable practical importance. We have already showed how important a bearing this doctrine of the existence of an elective affinity between various poisons and the vital tissues, has upon the theory of fevers.† A collateral practical inference directly flows from these views ; namely, that the continued action of alcohol on the nervous centres exercises an important influence in determining the predisposition of drunkards, or of habitual imbibers of fermented drinks and distilled liquors, to zymotic diseases. Dr. Carpenter is of opinion, that the selective power of alcohol appears to lead it in the first instance to attack the *cerebrum*, the intellectual powers being affected before any disorder of sensation or motion manifests itself. We are inclined, however, to entertain a different opinion on this point, and think that there are various reasons for supposing that alcohol acts immediately and directly on the sympathetic or organic system, even when our perceptions do not distinguish any cerebral or sensorial change. Thus Dr. Thomson indirectly leads to the conclusion, that the elective affinity referred to is not limited to the cerebral or cerebro-spinal system, when he observes :

“That alcohol enters into intimate union with the gastric parietes, appears probable from an observation of Dr. Percy’s, who found it impossible, by any ordinary means, to destroy the evidences of its presence in the substance of the stomach of an animal to which it had been administered. That the abuse of ardent spirits has some effect in hindering growth and development generally in the animal body, is practically recognised in the case of dogs, to whom it is given for the purpose of stunting their growth ; and, it is also said, to jockeys, for the same purpose.” (p. 55.)

Although the jockey-fact is of equivocal value, we have grounds for

\* See, for illustration of this doctrine, now generally admitted by physiologists, Dr. W. Budd’s paper on Symmetrical Diseases, in ‘Med.-Chir. Trans.’ vol. xxv ; and Mr. Paget’s Lectures on Nutrition, in ‘Medical Gazette,’ 1847.

† Vide our Third Volume, p. 76.

acknowledging the validity of the other statement. The alcohol is, however, given to the dog when very young, otherwise it has no effect in checking the growth of the animal, and not always even then.

The morbid conditions that result from the abuse of alcoholic liquors make up a fearful catalogue of diseases. Delirium, under the two forms of *D. ebriosum* and *D. tremens*, with all their horrors; the more permanent disorder of the cerebral functions, manifesting itself under the various forms of insanity; inflammatory and congestive diseases of the nervous system, comprising meningitis, apoplexy, epilepsy, and palsy; diseases of the gastro-hepatic, intestinal, and renal systems, including gastritis and hepatitis, acute and chronic hemorrhages, albuminuria, anasarca, and ascites, gout, rheumatism, and all their sequelæ and complications;—these are some of the painful and fatal evils inflicted on himself by the drunkard, and which are considered in detail by Dr. Carpenter. The tendency of habitual intemperance to produce idiocy, insanity, or mental debility in the offspring, is also forcibly dwelt on by Dr. Carpenter; and we shall, if space permits, notice this point in considering the etiology of the desire for stimulants generally. Previously, however, to taking up this question, we shall lay before our readers a short analysis of Professor Huss's observations on the pathology of "the chronic alcohol disease;" which, although we do not altogether acquiesce in his views, are of great value.

We learn from Dr. Huss's work, that he has for years directed his attention to the evil consequences of excessive doses of alcoholic fluids, a "malady" which has in Sweden, he says, rather increased than diminished of late years. In the above-named country, Temperance Societies have been long established; but much, he observes, of the benefit they might have afforded has been lost, by the intemperate zeal of many of the partisans of the cause, who have loudly condemned, not only the *abuse*, but even the occasional and moderate *use*, of alcoholic drinks. In the course of the opposition that has naturally been aroused by these sweeping denunciations, it has been maintained, that no diseases can fairly be ascribed to the *abuse* of alcoholic fluids. This our author finds of course little or no difficulty in disproving; and he commences his Essay by maintaining, what few we think will deny, that under almost all circumstances, and in regard to nearly all individuals, an *excess* of alcohol invariably acts most unfavorably upon the system. The present section of his work Dr. Huss devotes chiefly to the consideration of the effects of alcohol on the nervous system and upon the brain, not only on account of the number and variety of the symptoms, but also from the obscurity that has hitherto prevailed regarding their true nature and cause. To the train of various symptoms thus produced, Dr. Huss has given the title of '*Alcoholismus Chronicus*;' and he observes that, though few or none of them are by him detailed for the first time, yet that no special work or monography has been devoted to the diseases arising from the abuse of alcoholic liquors.

During the last three years, cases of "*Alcoholismus chronicus*" have greatly increased in Stockholm; particularly those forms of it characterised by diminution of power in the limbs, and passing on to complete paralysis of motion, with or without decrease of sensation in the parts affected. Our author inquires, whether this melancholy fact is occasioned by some alteration in the character of the spirit that is drunk, or whether a larger



quantity is now consumed than in previous years, when no temperance societies were in existence. Perhaps the real cause is to be found in the altered constituents of the impure *corn*-brandy which is generally drunk by the lower classes in Scandinavia. This fiery spirit, of the most detestable flavour (to our taste), is distilled almost entirely from potatoes; and in its unpurified condition contains an oil, or oleaginous fluid, of a highly deleterious character. The recent increase in the number of cases of the disease, corresponded with the time when large quantities of this spirit were distilled from diseased potatoes. Is it possible, then, asks Dr. Huss, that in spirit thus prepared from a diseased source, certain deleterious principles of a new character are formed, or are set free? Or does the spirit prepared from diseased potatoes contain no other but the principles known to exist in that distilled from the fresh and healthy tubers,—with this difference, however, that certain principles always found therein, greatly vary as to the proportion in which they are present?

“Long before the potato disease appeared, it was known, that when brandy was distilled from damaged grain, or from decayed potatoes, a certain principle was produced, volatile, irritating, and giving forth a suffocating odour, resembling, according to Berzelius, the smell of cyanogen gas in alcohol. The same chemist has remarked, that an impure brandy of this kind intoxicates more rapidly, and causes more excitement, while the patient is under its influence. Of late, since corn-brandy has been so extensively distilled from diseased potatoes, the presence of this deleterious principle has been more generally recognised; nay, it is so well known to the distillers, that it is termed by them “*Brännsnyta*,” or “*Stick*,” for when brandy containing it is warmed, or a person breathes air that has been passed through the spirit, a sharp irritation is felt in the eyes, nose, and fauces.” (p. 4.)

Chemistry, however, has not determined the nature of this product. Dr. Huss rejects the theory, that the narcotic alkaloid *solanine* produces the above effects; he maintains, that it is not sufficiently volatile to pass over with the alcohol at the ordinary temperature of distillation. Some have suspected, that brandy distilled from diseased potatoes contains an excess of potato-oil ( $C^5 H^6 O$ ), the “*finkelolja*” of the Swedish distillers. This oil, however, is not present among the products of distillation, unless the skins of the potatoes have been added. Now it is the skin of the tubers, that is *least* subject to the disease. True it is, that when this oil is given to the lower animals, it produces symptoms similar to those characterising “*Alcoholismus chronicus*,” but Dr. Huss is still disposed to regard alcohol, and not potato-oil, as the chief cause of the maladies. His conclusions are, that the brandy prepared from diseased potatoes, contains some new principle—termed by the distillers “*Brännsnyta*,” which is not to be met with in spirit distilled from fresh potatoes or from sound grain; and that this principle, operating analogously to alcohol, strengthens and confirms the evil effects produced by the latter. Such is a brief sketch of our author’s opinions, prefixed by way of introduction to the volume before us, in which are recorded the various cases of the malady that have been observed by him, divided into classes, according to the prominence of one or other of the symptoms. Many of the evil consequences produced by excessive doses of alcoholic fluids, he remarks, may also be observed to arise from other and very different causes; thus the fatty liver, and chronic inflammation of the stomach and intestines, are to be found in persons of wholly temperate habits. But, he continues,—



“Very different is it in regard to those symptoms which are developed in the nervous system by the abuse of alcohol, and which leave no appreciable sign after death, either in the brain or in the spinal cord, or its nerves. These symptoms are of so marked and peculiar a character, that there is little difficulty in assigning to them a separate and distinct place in our nosologies. It will be seen, that the character of these symptoms approaches nearest to those resulting from chronic poisons; and among these, to the effects of *opium* on the one hand, and of *lead* upon the other. It is to such groups of symptoms that I have given the name of *Alcoholismus chronicus*, by which word are signified those manifestations of disease, which, without pathological changes in the direct sense of the word, are developed in the chronic form, in those individuals, who, for a long period, have been accustomed to take alcoholic liquors in excess. We further eliminate from this arrangement, the delirium that is the immediate consequence of drunkenness, as likewise the well known disorder, *delirium tremens*.” (p. 3.)

Now we think it extremely probable that, under this term *Alcoholismus chronicus*, Professor Huss has described different diseases of the nervous system; all of them of a toxic origin, indeed, but caused by no one poison acting singly. It is well known in Germany, that several vegetable products of a poisonous nature, as well as potatoes, are used indiscriminately in the manufacture of common brandy. The *lolium* often mixed with barley, and diseased rye, or *secale cornutum*, are the most poisonous of this class; and the morbid phenomena they excite are in many respects similar to those described by Professor Huss. The seeds of the *Raphania raphanistrum*, a weed growing in corn-fields in Sweden, and in most parts of Europe, were thought by Linnæus to be the cause of this diseased condition; and he therefore termed it *Raphania*. This term has been also generally used to designate the results of poisoning by spurred rye. In his ‘*Encyclopædia of State Medicine*,’ published in 1838, Most notices the then greater prevalence of tremors, paralysis, and delirium, amongst dram-drinkers, than formerly; and attributes it to the toxic qualities of the spirit obtained from these poisonous cereals, which are largely mixed with the inferior barley and rye of the continent. The police regulations interfere with the sale of such grain for food; but it appears that they do not prevent its use in distilleries. The presence of a poisonous oil in inferior spirits is also well-known, and its effects have been often described;—it excites sickness, vertigo, fainting, and paralysis. It is formed during the fermentation and distillation from bad grain, the husks of grapes, potatoes, &c.; and its production is favoured by the use of copper vessels. It appears to differ in its physical qualities, according to the materials used for the vinous fermentation. If bad corn-brandy be diluted and frozen, the impure “fusel oil” is separated as a tallow-like substance. It crystallises from solution in boiling alcohol in white scales; if sublimed, it takes the form of white colourless needles; if evaporated on paper, it leaves an oily stain; is greasy at ordinary temperatures, and when warmed, melts into a yellow oil. The rectified “fusel oil” from potato brandy is colourless; feels fatty, but leaves no stain on paper. This “fusel oil” collects during the process of distillation, either in the receiving-vessels, or in certain other portions of the apparatus. In one instance, 100 parts of the concrete oil were found to contain 22·5 of oxide of copper, 6·3 oxide of tin, and 3·5 of oxide of lead. This fact points to another source of poisoning in these cases, namely, the poisonous metallic oxides.

It seems not improbable, that the disease termed *Beriberi*, which pecu-

liarily attacks the drunkard among the lower classes of Ceylon and the south-eastern coast of Hindostan, belongs to the same family as the "Alcoholismus chronicus," a family of diseases caused by alcohol, in combination with narcotico-irritant poisons, both vegetable and mineral. Beriberi is characterised by most of the phenomena induced by the use of diseased rye; the difference being principally in this, that in *Raphania* we have convulsions as the most prominent system, in Beriberi paralysis. It usually commences with a feeling of numbness below the middle of the thighs. The limbs are tremulous, the walk unsteady and tottering; spasms occur in the calves and soles of the feet, and even extend to the thorax and larynx, causing dyspnoea. The diminution of motor power in the legs often increases to complete paraplegia; and, in very bad cases, the paralysis extends upwards, so that the hands, arms, and even the neck and lips, become anæsthetic, and nearly paralytic. Concurrently with these phenomena, there are all the symptoms which characterise the advanced stage of *nephria*, or Bright's disease, as general anasarca, effusions into the pleura, pericardium, &c. In 65 cases observed by Mr. Malcolmson,\* 60 had more or less paralysis, 57 had numbness of the feet or hands, 33 spasms; a sense of coldness in the extremities was felt by 5, of biting of ants in 3, of tingling in 10, and in 4 of the feet being covered with clay. The disease is endemic; and by the native practitioner it is considered necessary to the cure, that the patient should be restricted to a diet of *wheat* brought from the interior, that is to say, to a cereal not grown in the locality. From other considerations it seems probable, that it is the deteriorated quality of the rice, and other grains grown in the district, which causes the disease; consequently, we have in the circumstances of the population, with reference to their cereals, and to the alcoholic drinks produced from them, a state of things analogous to what is observed in the northern countries of Europe.

In the convulsio cerealis, or *Raphania*, the symptoms increase in intensity, from below upwards, according to the amount of poisonous rye taken. In the milder cases, there are anæsthesia and spasms of the flexors; then trismus or opisthotonos; in the worse cases, epilepsy and paraplegia; in the worst cases of all, delirium, insanity, and idiocy. The acute stage will continue for three weeks; but the effects of the poison do not cease with that stage, as it is apt to induce all those varied forms of chronic disease of the nervous system, which Professor Huss has grouped together under the term "alcoholismus chronicus."†

Another source of poisonous contamination for alcoholic liquors may be traced to the tribe of fungi. We have already observed, that spurred rye, mildewed grain, bad potatoes, husks of grapes,—in short, any vegetable matter capable of the saccharine fermentation, is used by the distillers of Germany and the north of Europe. Such diseased vegetable products have a most intimate connection with microscopic fungi; and hence the probability, that the known poisonous principles of these mushroom growths are held in solution in these inferior kinds of spirits, and may even communicate those acrid, stupefying properties, which they in especial possess, and which induce the lower classes to prefer them to the

\* A Practical Essay on the History and Treatment of Beriberi, &c. By Assistant-Surgeon S. G. Malcolmson, &c. Madras, 1835. (p. 86.)

† Romberg, Lehrbuch des Nervenkrankheiten des Menschen, B. I.

purier alcoholic drinks. On all these points, we can only speak of probabilities, it is true; but they are sufficiently strong to warrant us in expressing a doubt, whether the *alcoholismus chronicus* of Professor Huss be really dependent even chiefly upon alcohol; and, in entertaining the opinion, that a series of careful observations will show that its phenomena result from the habitual use of alcoholic drinks holding various poisons in solution, in different amounts, and differing in their nature and action; but all having this in common,—that they exercise a highly deleterious influence, especially on the *nervous system*. This much, however, is certain; that the dram-drinker is taking daily doses of other narcotic and acrid narcotic poisons besides alcohol; and that the dram is, in reality, a most villanously poisonous compound, containing, combined with the alcohol and water, a heterogeneous mixture of narcotic and acrid poisons, burnt sugar, oil of vitriol, pepper, and the other matters and things by which the gin-merchant cooks his article to the taste of his customers. How much of the poisonous principle derived from the ergot, or from the lolium, or from the poisonous fungi that constitute the deadly vegetation of the refuse used by distillers, enters into the composition of the “Old Tom” or “Cream of the Valley,” or the other hideous compounds that the wretched drunkard swallows, may be and is beyond our knowledge; nor can we speak as to the exact amount of “Fusel oil,” or “Finkelolja,” or “Stick” contained in them; nor say how much of the poisonous oxides of lead, copper, and tin, are taken daily by the *habitué* of the “gin-shop.” Of one thing, however, we may express a confident assurance,—namely, that small poisonous doses of one or other of these various ingredients *are* taken; and that their morbid effects *must* appear in a derangement of the functions of the nervous system.

Although we thus differ from Dr. Huss, in our mode of looking at the phenomena which he has recorded, we consider the collection of facts which he has formed, from the large field of observation which has been (unfortunately) supplied to him, sufficiently important to justify us in requesting the attention of our readers to the following summary of them.

The effects of alcoholic drinks on the mucous membrane of the stomach and intestinal canal are too well known to require repetition here; Dr. Huss has invariably found the coats of the stomach more or less inflamed, and the mucous membrane of the small intestines in a like condition. The consequence of this condition of the mucous coats of the stomach is, that a gastric juice of abnormal and deleterious quality is secreted, digestion is impeded, and the various symptoms of dyspepsia are developed. The altered gastric juice is rendered still further unfit for its function, by the addition of fresh doses of alcohol, which is well known to diminish the solvent powers of this fluid; and thus the processes of assimilation and sanguification are materially changed.

In his account of the alterations produced by alcohol in the liver, we do not find that Dr. Huss has advanced anything new to science; but he is evidently thoroughly acquainted with the most recent researches upon the functions, anatomy, and pathology of this organ.

Respecting the kidneys, Dr. Huss observes:

“The so-called granular condition of these organs can frequently be directly traced to excessive intemperance, though other causes may have been aiding and abetting to complete the changes thereby produced. The kidneys of drunkards

exhibit an especial tendency to become affected with Bright's disease [we wish for once and all that he would not spell it *Bright*]; and thus other maladies, which rarely exercise any influence on the kidneys, may, in drunkards, lead to Bright's disease. Thus, for example, long-continued agues have appeared to me to be the *starting point*, in many cases, for Bright's disease; and the same, too, is the case in rheumatism. It is easy to explain how alcohol acts in such cases, when we remember how this fluid increases the secretion of urine, and augments the flow of blood to the kidney," &c. (p. 9.)

The evil effects of over-stimulation on the heart and large blood-vessels are well known, in so far, at least, as regards the acknowledged increase of size of the muscular parietes of the organ; but the peculiar results of the deposit of fat around and between the fibres of the heart, have not been sufficiently studied. On first examination, a drunkard's heart, thus loaded with fat, would seem to be hypertrophied; but, on closer inspection, the muscular substance will be found to be *atrophied*, and replaced by adipose tissue. The loss of power thus induced, allows even the left ventricle of the heart to become greatly dilated, and the usual symptoms of dilated heart are then observed. Should the patient survive this period of his malady, we next arrive at the stage, where the deposit of fat begins to be absorbed, and general emaciation takes place; so that the heart is often found considerably atrophied, and particularly in those cases where death has been preceded by paralysis, not resulting from apoplexy, but from general decay of nervous power.

Respecting the origin of the so-called atheromatous degeneration of the coats of the arteries, Dr. Huss admits, that there is considerable difference of opinion. Rokitansky believes it to be originally the result of a peculiar dyscrasia of the blood; while Engel regards it as the direct consequence of inflammation.

"It appears to us," says Dr. Huss, "that, in drunkards, both these causes may be in operation. The blood, by the addition of alcohol, becomes more irritating to the coats of the arteries; and, at the same time, by the over-stimulated heart, it is driven with increased violence against the walls of these vessels, and, in all probability, occasions in the latter a greater tendency to inflammation. This cause, however, would not of itself be sufficient, were it not that the blood of drunkards exhibits so great a tendency to the morbid deposit of fat in various organs. The atheromatous degeneration of the arteries does not consist of the ordinary products of inflammation, as fibrin, &c., but chiefly of cholesterin, fat, and albumen. In the arteries of the brain, another important change is frequently observed, usually, but not always, in company with atheroma of the larger blood-vessels of other parts. This is a dilatation of the arteries, both large and small, which ramify in the cerebral substance; so that, on slicing the brain, they appear of larger size than natural; while the coats of the larger blood-vessels likewise become brittle." (p. 11.)

The question of the influence of alcohol in producing tubercular phthisis is decided by Dr. Huss in the negative:

"It is seldom that we find pulmonary phthisis in the drunkard; but we frequently meet with what we term obsolete tubercles, which indicate that tubercular disease has at one time been in progress, but has been subsequently checked, and has ceased. But such appearances are constantly met with in the lungs of the most temperate people." (p. 13.)

We find nothing novel in the alterations which are described as being met with in the cerebral substance, in the spinal cord, or in the muscular tissue.

At one period or another of the drunkard's life, a large quantity of grayish unctuous fat is deposited, partly under the skin and between the muscles, and partly in the omentum and other portions of the abdominal organs. Subsequently this fat is reabsorbed, emaciation comes on, and not unfrequently, we find the place of the fat in the cellular tissue occupied by a gelatinous-looking deposit, chiefly, to all appearance, composed of albumen. To this succeed serous exudation and anasarca to a greater or less degree.

From what has been already said, it will naturally be expected, that the condition of the blood of drunkards will differ materially from that of healthy individuals. Dr. Huss does not, however, give us under this head much of original observation; indeed, we find that he has drawn his account of the blood under these circumstances, chiefly from B. Frank's *Essay in the Hannov. Annalen* for 1847. The modes in which alcohol operates on sanguification are generally known; it is either taken up directly into the circulation, or it operates unfavorably on the process of digestion and chymification, and thereby alters the character of the blood. The morbid condition of the liver likewise contributes to modify the blood; the excrementitious constituents of the bile not being thrown off by the blood of the vena portæ, but being carried on into the current of the circulation. Moreover, if Bright's disease exist, the blood will be still further disordered by the presence of urea. The alterations which some or all of these pathological changes produce in the blood, are well described by Dr. Huss; but it is evident that this part of the subject has been less illustrated by science, than the symptomatology of the disease we are now considering. The fatty condition of the blood of intemperate persons has been long noticed, but it has received particular attention from B. Frank in the *Essay* above referred to. The milky appearance which it presents, is caused by the blood or serum forming an emulsion with the oily particles which exist in it in excess. But such a condition of the blood may be produced by other causes than by intemperance; and, again, other changes in the constituents of that fluid may occasion a similar appearance.

"In general I have observed," says our author, "that oily particles abound in the blood remaining in the heart and larger arteries in the bodies of drunkards, and this blood, when rubbed between the fingers, gives the same feeling as though it were fat." (p. 19.)

Our author then proceeds to consider the near relation, in point of chemical composition, between fat and alcohol, but does not state anything in addition to the researches of Liebig and others in this respect.

"It is possible that alcohol is, in the bodies of the intemperate, directly converted into fat; but the altered functions of digestion and assimilation contribute still more largely to load the blood with this abnormal product. Moreover, the drunkard prefers animal food to all other, and often consumes this in large quantities, and as fat as possible, while his digestion is too rapid for the perfect assimilation of much of these fatty matters. The deposit of fat is still further increased by the imperfect decarbonization of the alcoholised blood in the lungs." (p. 20.)

We pass over several pages devoted to the subject of intoxication and delirium tremens, as not exactly appertaining to the present *Essay*, and proceed to the consideration of the symptoms of "*alcoholismus chronicus*." Under this appellation, our author designates those groups of nervous symptoms, appertaining to both the motor and sensitive system of nerves,



which are developed gradually in persons, who for a long period have been addicted to taking spirituous liquors in excess. These symptoms do not stand in any immediate relation to morbid changes during life, or to any pathological alterations after death, appreciable at least by the naked eye, in the centres or periphery of the nervous system.

The symptoms described in the succeeding pages have been observed chiefly in persons who have indulged in the unpurified spirit made from potatoes :

"With but few exceptions, the symptoms have been caused by the undistilled potato brandy, (*udestilleradt potatés bränvin*,) which is served out over the counter of the spirit shops to the lower classes of this metropolis. Spirit from grain is not common, and the distilled spirit freed from the volatile oil, (*finkelolja*, *fermentoleum solani*,) does not suit the palled taste of the habitual spirit-drinker. The presence or absence of this oil must be carefully borne in mind in estimating the causes of the disorder; I have already stated, that I have collected a considerable number of cases of this disease, which is in other words to say, that this malady is by no means rare in the capital of Sweden. To the shame of my country I must acknowledge, that such is truly the case; and that, during my travels in various parts of Europe, I have not, in the hospitals of the large towns, met with many cases of this kind, nor yet have I heard complaints of the frequency of this disorder, to the extent that it has fallen to my lot to see in this metropolis.

"It is possible that our climate contributes not a little to this; it may be that the symptoms here are developed with greater intensity than elsewhere; or else, that the abuse of spirituous liquors is more prevalent in Sweden than among the other nations of Europe. The former supposition is possible, the latter it is not in my power to deny." (p. 34.)

Dr. Huss forgets here, that the malady to which he alludes has now, for the first time, been designated and defined by himself. On looking back to the results of our experience in a large English manufacturing town where impure spirits are drunk to great excess, we now, for the first time, see clearly the history of many groups of morbid phenomena, of which the true nature was previously an enigma to us.

Our author divides his cases according as their symptoms chiefly affect (A), the body; (B), the mind; and (C), both of them together.

The first group of cases belonging to class A, are those in which, after long habits of intemperance (*dram-drinking*), tremors and unsteadiness of the voluntary muscles come on, particularly in those of the upper extremities. At a later period the muscles of the lower limbs begin to tremble; and lastly, the muscles of the trunk. The more extreme degrees of tremor are only observed, when the patient attempts to stand for some time perfectly still, when he will be seen first to tremble violently, and then to fall, if not supported. The upper limbs shake in like manner, if the arms are held extended for a short time. These symptoms are most marked when the patient rises in the morning, before he has had access to his customary stimulus; and they are much increased if, from any cause, he be debarred the use of spirits for a time. It would appear, then, that the motor system of nerves is so debilitated, as to require an artificial stimulus, before the usual movements can be accomplished. We fancy there are few of our readers who have not met with cases of this kind; but too often attempts have been made to trace these symptoms to some pathological change in the solids of the body, as disease of the heart, of the brain, &c.



Another group of symptoms is that in which feebleness first shows itself in the voluntary muscles; the loss of power increases, and finally paralysis may take place:

"The patient first remarks, that he cannot hold things so fast as heretofore; soon too he finds, that objects which he has taken up, fall involuntarily from his hands; and as he exerts himself to avoid this inconvenience, the hands begin to shake and tremble, and he calls this nervous debility, and endeavours to correct this by fresh doses of ardent spirits. This "loose-handedness," if I may so term it (*släpphändhet*), passes on sooner or later to incomplete paralysis, wherein, at first, nothing can be retained, and subsequently nothing can be grasped by the hands. Though the hands and fingers are thus weakened, the humeral portion of the arm retains a considerable degree of strength, so that the debility is most marked in those parts which are furthest from the nervous centres." (p. 44.)

The same train of symptoms gradually shows itself in the lower extremities, and the patient may eventually lose entirely the power of locomotion. The nerves of sensation, too, begin now to be affected, a feeling as of something creeping between the flesh and the skin is much complained of, particularly over the *glutæi* muscles and the upper part of the thigh. This sensation is relieved by active movement of the limb. Our author regards this as the first stage of altered and departing sensation in the affected parts, and has frequently observed it to be accompanied by vertigo and dimness of vision. In most of these cases, too, he has found the first sound of the heart altered in tone, so that it can scarcely be said to differ in this respect from the second sound.

In the majority of the cases here recorded, which are given at great length, the blood was found to contain a quantity of fat globules. Our author seems to have benefited his patients greatly, in some of the cases of palsy, by the administration of strychnia in small doses. We suspect this peculiar loss of motor power to have been dependent on lead-contamination.

From the cases characterised by loss of motor power, Dr. Huss next proceeds to describe those in which sensation was chiefly affected. The same order in the progression of the symptoms, from the extremities of the nerves towards the nervous centres, is here observed; but loss of sensation is almost always accompanied with loss of motor power; and, in fact, is generally preceded by it. At first the patient experiences difficulty in determining, from the feel, the nature of the object he has laid hold of, or against which his foot may have struck; but in a more advanced stage, sensation may be so entirely lost, that a pin may be run through the foot or hand without any pain being felt. Dilatation of the pupils is a common symptom in the more advanced stages of "*alcoholismus chronicus*." Dr. Huss has ever observed, that by the time that the above-described loss of feeling was fully developed, important changes had taken place both in the stomach and liver; the former being in the state of chronic inflammation, and the latter having undergone fatty degeneration.

The cases we have hitherto noticed are, in conjunction with more or less of debility and numbness, chiefly characterised by tremors and agitations, indicative of the loss of the due equipoise in the voluntary system of muscles. We next proceed to those in which the operation of the poison is developed in other forms; at one time producing cramps or twitching of the tendons, at another more or less violent convulsions.

We own that we scarcely expected to find so many cases of convulsions

attributable to excessive indulgence in ardent spirits ; but when Dr. Huss states, that the extreme point to which these convulsions may extend, is the true epileptic paroxysm, we fully coincide with his views. Epilepsy, it is well known, is the too frequent termination of the dram-drinker's life. Our author has, however, never observed the convulsive form of alcoholismus chronicus to supervene, without being preceded by the other minor degrees of the malady. This should always be considered, in forming our diagnosis of the cause of these convulsive seizures. Of the cases here detailed at full length by Dr. Huss, No. 10 (p. 56) presents a very characteristic example of the fully developed convulsive form of the disease. Here we have tremor, formication, and loss of power, followed by attacks of convulsive jerking of the majority of the voluntary muscles. The muscles of the face are contorted, the tongue does not respond to the will, the head and the extremities shake constantly, while the muscles of the trunk are only very slightly affected ; in a word, the convulsions, though not severe, may yet be called general. In this, and the following case, tenderness over the first to the fifth lumbar vertebra was observed, when these parts were subjected to pressure. The pain, too, was found to radiate from these vertebræ when pressed ; and our author considers that it is a doubtful question, how far the convulsions may not have been the result of spinal irritation ; as the symptom just referred to, is one of the most frequent and most certain indications of that disease.

From the convulsive forms, there is a natural and easy transition to those characterised by true epileptic seizures. Although the cases here detailed are of much interest, yet they present no peculiar symptoms. At an advanced period of the disease, Dr. Huss has observed that the epileptic paroxysms gradually disappear ; and that their departure corresponded with a marked decrease of the patient's muscular powers. In such cases our author believes, that the brain and the spinal cord have lost that degree of excitability (*spänstighet*) which is necessary for the production of the epileptic seizure. Case No. 15 is an instance of what, from the previous assertions of our author, we did not expect to find : viz., a fully developed case of *epilepsia potatorum* occurring without any preliminary symptoms. Dr. Huss seems to have much confidence in the *Infus. gratiolæ* as a remedy in these forms of alcoholism.

Hitherto we have met with cases only of diminished power or of diminished sensibility ; but a rarer form of this malady is next described, in which the latter function is morbidly increased ; the morbid sensibility of various parts of the body seeming always to have been preceded by formications, which decreased as the sensibility was augmented. This, too, we would remark, is an occasional symptom of *lead-poisoning*.

The fatal cases next are brought before us ; but our reader will anticipate, from what has been said before, that post-mortem examinations have thrown but little light upon this disease. The results, indeed, are rather negative than positive ; they prove, what was already known, that serious alterations may take place in the functions of the nerves, without any change in the nervous centres being discoverable by our eyes. In almost all fatal cases, the liver and the stomach have undergone various pathological changes ; while the heart is less frequently affected, and still less frequently the brain.

We have only room for the *resumé* of the post-mortem appearances ;

though in the details of every case there are points of interest, and observations of great value are added by the author.

The alterations common to all the cases detailed, were :

- a. Dilatation of the smaller blood-vessels of the brain.
- b. Hypertrophy of the Glandulæ Pacchioni.
- c. Chronic inflammation of the mucous membrane of the stomach.
- d. Changes in the liver, of various kinds. In one case, the nutmeg liver appeared; in a second, there was cirrhosis; in a third, fatty liver; and in two others, the granular condition (*granulationstillstand*).

The alterations which were not common to all the cases were :

a. Effusion of clear serum between the membranes of the brain, dipping between the convolutions in two cases; and in one instance the lateral ventricles were filled with serum.

b. Effusion of clear serum between the dura mater and the arachnoid, in the spinal canal, and at the base of the brain.

c. In three cases, the larger and smaller blood-vessels which enter the brain at the base of the skull were dilated; in two of these three cases, the inner membrane of the blood-vessels was tender and brittle, and in one it was opalescent and thickened.

d. In one case, there was dilatation of the smaller blood-vessels in the substance of the spinal cord.

e. In one instance, there was both red and white softening of the spinal cord.

f. The blood was watery in four cases; in two of which fat globules were distinctly seen; in the other two, they could not be discovered.

g. In one case, the blood was coagulated, and contained much fat.

h. In three cases, the ileum and colon were in a state of chronic inflammation.

i. Serum was effused into the lungs, the subcutaneous cellular tissue, the pleura, and the peritoneum, in one instance.

k. Hypertrophy of the left side of the heart occurred once.

The reader will see how carefully the *post-mortem* appearances in these five cases have been noted and analysed. One of these cases alone, with the accompanying observations, occupies six pages of the volume; but Dr. Huss states, that he considers it necessary to be thus prolix, as no complete monography of "chronic alcoholism" has been published. He believes, that the cases he has hitherto described under the first head, viz., that of *corporeal* (*somatiska*), as distinguished from *mental* affections (*psychiska*), may be arranged under the following heads :

1. The premonitory form (*prodromatiska*), where there is only observed a diminished tone in the muscles of locomotion and prehension.

2. The paralytic form.

3. The anæsthetic form.

4. The convulsive form.

5. The epileptic form.

6. The hyperæsthetic form.

Our author next proceeds to describe some anomalous cases; beginning with one (No. 24), in which, in addition to the usual symptoms of anæsthesia, paralysis occurred in the muscles of deglutition. Dr. Huss treated this case with the *Faba Sti. Ignatii*, in small doses, and effected a rapid and complete cure. The symptoms in this case resembled not a little those

of stricture of the œsophagus, or still more of a sacculated pouch in that canal. Nos. 27 and 28 are instances of most remarkable ocular (not mental) delusions; but want of space compels us to pass them by with this brief mention. No. 29 is placed by our author under the class of Anæsthetic forms; we should ourselves, however, be inclined to reckon it among the Hyperæsthetic varieties. The sensation here felt was that of burning hot grains of sand making their way beneath the skin of the back and of the arms and legs. Dr. Huss (perhaps justly) regards the symptom as an exaggerated degree of formication. This singular affection occurred every morning at an early hour, and ceased about noon. It was entirely removed by two full doses of opium; but debility, tremor, and anæsthesia, to a great extent remained, and were not banished until the *Faba Sti. Ignatii* was employed.

Six cases are next very minutely described of *mental* affections consequent on spirit-drinking. In all these cases the corporeal indications of alcoholismus chronicus were not wanting. Dr. Huss believes that suicide more frequently occurs in the melancholia from intemperance, than in that from any other cause, especially among the lower classes of society. We are obliged to pass these cases by likewise, for we have already extended our analysis beyond due limits.

The five cases that follow are instances of mental and corporeal affections in the same subject; the one form sometimes prevailing, and then the other.

Case (No. 36), is perhaps the most interesting that is detailed in the whole volume, as the greater part of its history is given in the patient's own words. We would gladly have extracted it, but its great length (8 pages), including Dr. Huss's annotations thereon, must be our excuse. In this one case we find combined almost every symptom of chronic alcoholism. The paralytic, the anæsthetic, and subsequently the hyperæsthetic form prevailed in the first attack, for which the patient sought relief in the hospital. He left that institution almost completely cured, and retiring to the country, occupied himself with manual labour, and resolutely abstained from spirits during two years. At the end of that period the craving for alcoholic stimulus became so excessive, that he again gave way. During the whole period of abstinence from spirituous liquors, he remained in perfect health. The return to his evil habits brought on convulsive attacks, and at the same time the mental faculties became more or less disordered; an attack of mania supervened, and the patient subsequently thrice attempted suicide. By careful treatment, with the *oleum solani*, *arnica*, and *nux vomica*, he entirely regained for the second time his lost health; but Dr. Huss expresses little or no hope of his patient's being able to resist the morbid tendency to drink, which, as he most justly observes, in itself constitutes a disease. The remaining portion of this volume (pp. 167 to p. 194), is occupied entirely with minute histories of cases, which, though greatly resembling those of Alcoholismus Chronicus, yet differed from that malady in one important symptom or another. Some of these maladies were the results of metallic poisons, as of arsenic, mercury, and lead; while, in a few instances, the disease had arisen without any obvious cause.

While perusing this volume, we have been repeatedly vexed and annoyed to find that it is designedly left incomplete; the cases alone, with

the author's annotations on each special instance, being here given, whilst the general conclusions from these facts, the pathology, and the treatment, are reserved for the ensuing part of the work. But even in its unfinished form, we cannot but regard this work as a most important contribution to medical science, and as one calculated to augment the fame the author has already acquired as a learned pathologist, and a diligent investigator of disease. There is not a page, we repeat, in the volume, which is not replete with information; there is not a case detailed, which might not serve as a model for "case-taking," in our hospitals and dispensaries; and we trust that Dr. Huss's conclusions may be tested in this country where, alas, there are too many opportunities of observing the disease in various degrees of complication and severity.

Some valuable information on the amount of distilled spirit consumed in the British Isles, was recently furnished by Mr. G. R. Porter to the Statistical Section of the British Association, and may be of use to the inquirer in this branch of pathology. It appears from authentic returns, that the average quantity of home-produced spirits and rum annually consumed by each adult male, (the women and children being reckoned as *nil*), is  $2\frac{1}{3}$  gallons for England,  $3\frac{1}{3}$  gallons for Ireland, and no less than  $11\frac{1}{8}$  gallons for Scotland. In this estimate, the *absolute* amounts are rather too great, the quantity of spirits consumed in 1849 being taken as one of the data, whilst the population-returns of 1841 furnish the other; but this inaccuracy will, of course, have little effect on the *relative* proportions. It is to be remembered, that a large consumption of malt liquors takes the place of spirits, in great degree, among the labouring population in England; and that, in the middle and higher classes, brandy is more commonly employed than whisky and rum. The comparison, therefore, is by no means so greatly in favour of England, as it would at first sight appear; but considering that the preference of the Irish population for malt-spirit is nearly the same as that of the Scotch, the comparison tells much more in favour of Ireland than we were prepared to expect,—it being currently reported, that the effects of Father Mathew's labours have evaporated with the excitement which they produced. We believe that it would be wrong, however, to stigmatise the Scotch as an habitually *drunken* nation. From inquiries which we have made on the subject, we are inclined to believe that the consumption of spirits among the peasant-population of the country is extremely small; and that the large amount consumed, is principally to be accounted for by the drinking habits of the inhabitants of the principal towns (especially Glasgow), and of the villages on the coast, as also of individuals scattered over the whole country. Thus we have been informed by the clergyman and chief medical practitioner of Newhaven, a fishing-village close to Edinburgh, containing 1500 inhabitants, that they have ascertained by careful inquiry, that no less than £20,000 is annually spent in whisky by that small population. At the usual retail price, this would represent about 30,000 gallons, or no less than 20 *gallons a year for every man, woman, and child in the place*. On the other hand, the clergyman of a Highland parish informed us, that the great bulk of his people rarely tasted spirits except on some special occasion, but that he could point to some six or eight individuals, who, though seldom or never seen to be drunk,



severally consumed from half a gallon to a gallon of spirits weekly, or from 26 to 52 gallons each in the course of the year.

II. If we turn from the Pathology to the Ethics of this question of the use and abuse of alcoholic drinks, need we, meditating upon the vast perspective of suffering, and of crime, that opens before us,—on the slow lingering descent into all depravity and degradation, from a high eminence of moral worth, that we witness,—on the ruined homes and broken hearts, on the wide-spread social devastation that we know to be caused by the *abuse* of alcoholic drinks,—need we, meditating on all this, hesitate to commend this subject, ethically, to the consideration of the profession? The remedy for much of this evil, it is certain, is in *their* hands; and we are satisfied that the wise and good amongst us, see and feel the necessity that is laid upon them to do their duty in this matter, as the instruments selected by Divine Providence to minister to suffering humanity. We frankly declare, that whatever obloquy we may incur, we will do ours; and we confidently trust to the aid and good-will of the like-minded. On this point we fully adopt the sentiments of a recent writer on medical ethics, (noticed in our last Number), who thus refers to the great moral question of temperance:\*

“It is difficult to conceive that a physician, possessed of the ordinary feelings of humanity, should fail to be decided on this subject, either in his opinions, or his influence. No man has had so varied and extensive opportunities of witnessing the ravages of intemperance. It is not an occasional visit that he has made to the miserable home of the drunkard. It is not occasionally that he has heard from trembling lips the tale of woe, and seen its painful and often hideous signs. It has been with him an almost every-day occurrence. Misery on every hand has made its appeal to him. And if he has allowed his desire for popularity to hinder him from heeding such touching and frequent appeals, it is not too much to say to him, that he has been shamefully recreant to the dictates of humanity, and that he will have to render a large account of neglected opportunities of doing good.”

It is not, however, sufficient to be convinced of the necessity there exists for grappling with this monster evil, or to be willing to labour to its extirpation. Our efforts should be well-directed, as well as sincere and energetic. On this point there has been a failure, and we will endeavour to advance the matter a step.

The use and abuse of alcoholic liquors, as we have already remarked, constitute no new feature in the history of mankind. Our earliest historical record describes the drunkenness of a patriarch; and attributes to it the degradation of a whole race of mankind, descended from that son who mocked the folly of his father. The religion and laws of the nations of every portion of Asia bear traces of enlightened efforts to check the vice of intemperance; and to this day there are numerous tribes who, by religious profession, are total abstainers. From Indus to the Pole, literally, man has provided himself with alcoholic stimulants, using almost every product, whether animal or vegetable, capable of the vinous fermentation—milk, honey, fruits, cereals, roots. Where, however, the religion or laws of the state or people forbid the use of these stimulants, or where per-

\* Physician and Patient. From the Text of William Hooker, M.D. Edited by Edward Bentley, M.D.



chance the manufacture of an alcoholic stimulant has not been discovered, the same unerring instinct to gratify the desire for artificial stimulation, has found in the betel-nut, haschisch, poppy, or other vegetable product, more or less effective substitutes for alcoholic liquors; and against the abuse of these, also, religion and legislation have been diligently directed. We may, we think, assert without fear of contradiction, that in every age of the world there has been a total abstinence movement; and we may also assert, that every such movement, even when it has received the most emphatic sanction of religion and of law, and when its precepts and practice have been enforced with all the energy of Oriental fanaticism and despotism, has failed to make any other than a temporary impression on the intemperate use of nervine stimuli. As the total abstinence movement of the present age is following a similar course to all that have preceded it, we do not hope for better results; nay, we do not hope for equal results, for it is highly improbable, that a total abstinence from alcoholic stimuli (leaving out of consideration the class of narcotic drugs) will ever be made an essential dogma of the national religions of Europe. And if so improbable a result were to follow upon the total abstinence movement, it would finally be found, that we had only substituted one stimulant for another; and opium, or haschisch, would take the place of alcohol. Indeed, it seems not improbable, from the greatly increased consumption of opium in this country, that such a substitution is already in progress.

Now, since the long experience of ages thus teaches us the phases through which a successful temperance movement would pass, it behoves the medical profession to consider this question well, and to hold out the lamp of science as a guide to the attack on the mighty foe of mankind. And, in the first place, science teaches us, that an instinctive desire, such as is that from which the vice of intemperance springs, can never be eradicated. Hence the fundamental principle, that a system of asceticism is as inapplicable to this vice as to any other arising out of instinctive desires. Vows of celibacy have never eradicated lust; of fasting, gluttony; of poverty, avarice; of humility, ambition. The thoughtful, the enlightened, the religious, will not require a vow of total abstinence to keep them temperate; the ignorant, the untaught, the irreligious, will too frequently only take the vow to break it. Such sentiments are felt and acted upon by many, who yet willingly lend their countenance and assistance to temperance societies; and in doing this they are perfectly consistent; for these societies tend to render people thoughtful and enlightened, especially those who have little time for reading and thought. It is true, their asceticism renders their teaching repulsive; but nevertheless they teach truth, they often teach well, and, sometimes, we are bound to add, they teach wisely.

The wine, ale, and spirit-drinkers, may be divided into two great classes. The first comprises those who take them in moderation, with a view to individual and social enjoyment. These are willing to grant to Dr. Carpenter and to Dr. Thomson that man in ordinary health, and surrounded with the usual conditions of health, has stimulus enough in the food he eats, in the atmosphere he breathes, in the continually recurring round of duties which engage, or ought to engage, the bodily powers or mental energies, to preserve him in health and strength, without the aid of alcohol in any shape; and that even the most moderate habitual use of alcoholic liquors

becomes to the "average man" positively injurious, if protracted for a sufficient length of time to allow of the development of its effects. But they will not measure their dietetic conduct in accordance therewith, simply because they take the enjoyment which is certain, as a balance against the evil which is uncertain; or because they deliberately prefer their pleasure, to a prospective and perhaps slight injury to their health. This rule of conduct is, in fact, the rule adopted in all the various pursuits of mankind. At the bar, in the senate, in the field, in the practice of medicine, in commerce, in the chase and other amusements; in short, in every one of the multitudinous employments of his faculties in which man can engage, and in which some or other of his innate desires and sentiments are called into activity, he deliberately continues to pursue his object, although suffering, disease, and death, are all but certain. To this large class, then, of the users of alcoholic drinks,—the class using them (and not abusing them) for the purposes of social or individual enjoyment, the advocate of total abstinence will address his teachings in vain.

The other large class of drinkers of alcoholic liquors comprises those who use them immoderately for the purposes of enjoyment, or of relief from suffering; and it is this class on which the advocate of total abstinence can exercise the best influence. This may be subdivided. It comprises men who, beginning with moderate doses for the purposes of enjoyment, have at last made the pleasure thence arising a special pursuit, or at least *one* of their special pursuits. It also comprises those who take alcoholic drinks to secure a brief period of freedom from painful feelings, arising from numerous causes; but the most common of which is, that state of the body resulting from the moderate or immoderate use, or rather the abuse, of alcoholic drinks. These various classes of drinkers to excess, require appropriate and special treatment for the cure of their infirmity; one method is as inapplicable universally in this as in other forms of diseased action.

There is, however, *one* fundamental principle of treatment; namely, that the mind should be diverted from the pursuit of enjoyment, or of relief from suffering, through this means, by setting before it other and better means. Nothing is so antagonistic to the whole class of morbid instincts, as active or diversified employment of the body or mind. Hence the importance to morals of cheap railway-trips, pedestrian excursions, and those various recreations and amusements which distract the mind from a monotonous routine, and set before it new objects of attention. If the large sum spent in nervine stimuli, such as alcoholic drinks, opium, and tobacco,\* were only *partly* thus diverted from its present channel, a vast amount of social good would result in increased happiness, diminished crime, and improved industrial and commercial pursuits.

But while the advocates of temperance should diligently endeavour to increase and cheapen the innocent amusements of the people, they would also do good service by investigating and denouncing the vile frauds by which a cheap stimulant is supplied. It is to be feared, that the narcotic poisons which are found combined with the spirits of the north of Europe, are not absent from our own. Now, it is the effect of all these narcotic

\* The total expenditure chiefly by the *labouring classes*, in this country, on distilled spirits, beer, and tobacco, is estimated by Mr. G. R. Porter (the highest authority on this subject), at no less than *fifty-seven millions* annually. This sum he significantly denominates "the self-imposed taxation of the working classes."

poisons (in common with alcohol) to cause an ever-increasing desire for them ; so that their presence in alcoholic drinks renders the latter doubly effective in establishing the habit which characterises the worst forms of incurable drunkenness, and which has been termed *oinomania*. (See p. 196). The cause of humanity would gain by any means which, by rendering spirits purer, should arrest this frightful form of the vice ; and the cause of temperance would gain by the increased cost at which the purer form of spirit would necessarily be produced. The excise laws, then, should not only take cognisance of direct adulterations, but forbid the employment of the inferior grain or vegetables (as bad potatoes) in distilleries.

Frequently, however, *oinomania* is directly dependent upon a morbid cerebral change, either functional or structural. There can now be no doubt whatever, that everything which exhausts the sensorial or motor power conduces to excite this irrepressible desire for stimulants. Breathing and labouring in an impure atmosphere, long-continued mental exertion, or depressing emotions, are almost as effectual in exciting this morbid development of the instinctive desire, as the continued use of the nervine stimulants. Hence the necessity for a careful investigation, with a view to removal of general agents of this kind.

Various forms of gastric and intestinal disorder excite the instinct for stimuli into activity, by re-acting on the cerebrum ; especially those forms of *gastrodynia* which are relieved momentarily by alcoholic stimulation. They act, indeed, in two ways, either by creating the habit, or by exciting the desire. The desire for relief from pain, corporeal or mental, is, without doubt, one of the most general and influential of the causes of this form of intemperance.

“ If any care, or pain remain,  
We'll drown it in the bowl,”

is the burden of many a *baechanalian* song.

From all these considerations it is obvious, that a really effective check to the vice of intemperance cannot be found in the one means only, namely, the advocacy of total abstinence ; but that an enlightened pathology and philosophy must supply it, by demonstrating the nature of the instinctive desire on which the vice is based, and pointing out the means by which, under its various morbid aspects, it may be antagonised. In this, as in other questions of morals, the medical practitioner must be eminently catholic.

Education, Sanitary Reform, and Temperance, may be considered as the three leading ideas among the philanthropists of our time. Each has its exclusive advocates, who represent it as the panacea, the one thing needful, from which every other improvement will spontaneously proceed in due course. But the more enlightened promoters of these objects see that they must be pursued in conjunction, to be really efficacious. The Educator, in his attempts to develop the intelligence, to cultivate the moral nature, and to elevate the tastes and aspirations of the rising generation, is continually baffled by the depressing and degrading influences of their physical condition, and by the atmosphere of brutal sensuality which they are forced to breathe ; for his success, therefore, the conjoined labours of the Sanitary Reformer and the Temperance Advocate are essential. So the Sanitary Reformer finds one of his greatest difficulties in the careless

apathy of the class he is striving to benefit ; an apathy which is the obvious and immediate result of ignorance and degrading indulgences, and for the removal of which he must look to the assistance of the Educator and the Temperance Advocate. And the Temperance Advocate, in his turn, finds he can do but little, unless he can give a new direction to the minds he is attempting to reform, by educational means adapted to their condition, and can provide for a higher degree of physical well-being, by the assistance of the Sanitary Reformer. In these general objects, and the various departments of labour which they include, there is "ample room and verge enough" for the exertions of every well-wisher to his race ; and there is no one, however limited his opportunities, however narrow his sphere of influence, who may not do *something*. Upon the members of our Profession, as we have remarked on a former occasion, the especial call is laid, to keep the great subject of Sanitary Reform before the public mind ; but it is not less their duty to encourage, in whatever way they may deem best, the extension of the Temperance Reformation, and in particular to apply themselves to the correction of the many fallacious notions which are prevalent in the public mind, as to the virtues of alcoholic liquors in sustaining the human frame under prolonged and severe exertion, bodily or mental, and exposure to heat, cold, and vicissitudes of temperature. Upon these points, the authors of the two essays before us are fully agreed ; and the body of evidence which they have collected must be satisfactory, we think, to every unprejudiced reader.

Although we have felt called upon thus to express our dissent from the views on which the principle of Total Abstinence is based, and believe that the system which its apostles would build up can never have any firm foundation in the public mind, yet we should be sorry to be understood as desiring to check or discourage them in the gigantic labour they have undertaken. We believe that they have effected, are effecting, and will effect, great good ; especially in collecting and diffusing information, which shall tend to correct certain prevalent errors with regard to the supposed virtues of alcoholic liquors ; and in undermining the various "drinking customs," which almost force many of our artisans into intemperance, in a way but little suspected by those who blame them for their vicious propensities. Every one knows that there was a time, and this not very long gone by, when such compulsion was used among the higher classes ; the door being locked until a certain quantum of wine had been disposed of, and every one at table being forced to take his share. The progress of civilization and refinement has now happily rendered this practice a matter of history ; and in "good society" excess has given place to moderation. There are indications, to which we shall presently refer, that the movement of public opinion on this point is extending downwards ; and every friend to temperance may most legitimately unite with the upholders of the total abstinence principle, in promoting, by every possible means, this most desirable reform. And those members of our profession who belong to the first of the classes just now alluded to, and who are satisfied that the health and strength of the labouring man may be *as well* sustained (to say the least) without the moderate use of alcoholic liquors, as with it, may do great service to the cause of temperance by making known their convictions on this point, notwithstanding that they do not feel called upon to preach, still less to practise, total abstinence.

As far as we can gather the views of the authors of the two Essays before us, from the slight notices which they give of the ethical relations of this question, we cannot think that they differ so much from those which we have expressed, as the circumstances under which these Essays were produced might lead us to suppose. The following are Dr. Spencer Thomson's remarks upon the subject :

"The Total Abstinence system is only adapted, and it is eminently so, for men sunk in a sensual state, incapable of exerting the higher control of religion, or even of rational intelligence ; and it is in this view, that the pledge of total abstinence assumes its most valuable aspect. We find a large mass, a large proportion of men, who cannot use without abusing, who are unable to control their own actions ; with such, the pledge, the vow, is the only means of restraint, and for this purpose it is most valuable, and every man who is not merely habitually intemperate, but who even verges towards the dangerous boundary, ought to become a total abstainer ; we would even say, at the risk of some injury to health. Beyond this, the question becomes one of private judgment : if any man, capable of governing himself as a responsible being, yet feels that, all circumstances considered, he ought to become a pledged total abstainer for the sake of example, and that his physical constitution and external relations admit of it without injury to his efficiency in his individual duties, let him do so by all means : the question is one of duty to his Maker, to his neighbour, and to himself ; but let him beware of judging his brother, who does not think, who is physically unfit to act along with him." (p. 182.)

It is evident, therefore, that Dr. Thomson only regards total abstinence as a *means* of repressing excess, and of promoting habits of temperance ; and that he does not look at it as the ultimate *end* and *aim* of the movement. Although Dr. Carpenter dwells more strongly than Dr. Thomson upon the injury likely to accrue, in the great majority of cases, from the *habitual* use of even moderate quantities of the weaker alcoholic liquors, —pushing his argument upon this point to an extent to which many will doubtless think it incapable of stretching, and to which we ourselves may deem that it can only apply by a little overstraining,—yet he nowhere states, or even hints, any objection, founded on either science or experience, to the *occasional* temperate use of alcoholic stimulants, for the promotion of social enjoyment. On the contrary, he repeats his formerly-expressed conviction, that "if all the world could be really temperate, there would be no need of Total Abstinence Societies ;" for enunciating which doctrine he has been stigmatised by some of the ultra-partisans of the cause, as a traitor to his principles. And he has distinctly placed the question of abstinence from the *occasional* use of alcoholic liquors upon the *moral* ground only (Preface, p. xii) ; leaving it to the consciences of his readers to determine, whether, in their respective spheres of action, they are likely to exert a beneficial influence by embracing that system. It is obvious, therefore, that he, too, desires the wide extension of this principle, for no other reason than that it may contribute to call forth in the masses, who are at present sunk in brutal sensuality, that capacity for being worked-on by higher motives, and that power of exerting self-control, which it is one of the most constant effects of the habitual employment of nervine stimulants of any description to weaken or destroy. We are sure that both these advocates feel with us, that the Total Abstinence movement will *permanently* effect but little *by itself* ; and that it can only be regarded as one among the means which must be earnestly and perseveringly carried forwards, in order to produce any *durable* result in the removal of the social evils



which press so heavily upon the thoughts of every real lover of his kind. In this great work, there is room for the labourer of every kind and degree of capability; each will readily find his place, if he sets himself in earnest to seek for it; each will do his work best, if his department be freely chosen by himself, from a sense of its importance in his estimation, and of his own special fitness for it; but each must rejoice in the co-operation of those who are labouring in other departments, and must be prepared to recognise the value of their efforts.

III. We are not without proofs, indeed, that the results of the labours of the Teetotal Advocates of our day must not be judged of solely by the number of converts they make; but that they exert an influence beyond the sphere of their own societies, which is perhaps the more wholesome, and the more likely to be permanently efficacious, because it does not interfere with that liberty of action, which, by every man of independent mind, is prized as one of his dearest privileges. One of the most interesting examples of this influence is presented in the Report of the Admiralty Committee, the objects of whose appointment we have already referred to, and of whose inquiries and results we shall now give a brief summary.

The daily allowance of spirits formerly issued to every man in the Navy, was half a pint; half of this ration being served out at dinner, and the other half in the evening. By the recommendation of a Committee of Flag Officers, in the year 1824, the Admiralty authorities reduced this allowance one half; the evening serving of grog was discontinued; and tea was issued in its stead. The result of this change is described in the Report as "a sensible improvement in discipline, to the comfort and contentment of all on board." In less than two years, however, the introduction of the imperial measure, augmenting the allowance by one fourth, rendered it too large for a single issue, and again forced into use the custom of the evening serving, which is now proved, by the concurrent testimony of nearly all the witnesses, to be "the source of those evils which render discipline irksome, and give to the naval service a character for harshness which it does not deserve." There seems to be fully as much complaint of drunkenness and insubordination at the present time, as there was before the reduction; and this, strange as it may seem, arises out of the habitual temperance of the great mass of the men.

The existing system works thus.—Half of the spirit-ration, diluted with water, so as to make half a pint of grog, is served out to each man at dinner-time. Generally speaking, every man drinks his allowance, unless he spares a share of it for some unlucky shipmate, who is suffering from "the stoppage of his grog," as a punishment for previous drunkenness or misconduct. The case is far different, however, with the evening allowance. The majority of the men prefer the tea to the grog; and yet it is a point of honour with them not to give up the latter for the "savings price" allowed by the purser. The whole allowance for each "mess" is brought in a vessel called the "monkey;" and, in many ships, it appears that the entire contents become the perquisite of the cook of the mess (which office is filled by each man in rotation), who thus gets the allowance of a dozen or sixteen men. This he drinks by himself, or with the assistance of a friend or two, invited, it may be, from some other mess.



This jollification sometimes takes place immediately before the evening muster at quarters; and the men who have taken part in it manage to keep the appearance of sobriety until they have passed muster, though half an hour afterwards they may be found dead drunk under the guns. If the liquor can be conveniently hidden away, however, it is kept until after the muster. In ships where the cook does not get the monkey-full of grog at the evening serving, he is usually allowed a "plus," both at dinner and at night; the measure in which it is served out to the men being purposely reduced in its dimensions, or not being entirely filled up, or being partly occupied by the thumb of the distributor, which is inserted in it; so that he gets two or three times his own share by the contributions of his messmates, each of whom cheerfully submits to this trifling mulct, in order that he may enjoy the treat in his turn.

Such are the systematic methods in which facilities for drunkenness are obtained, by means over which the officers cannot exert any direct control, and which are kept up by the *esprit du corps* of the Naval service, against the better feeling of the well-disposed men. There are other modes, however, which are practised to a great extent, but are not coercive like the preceding.

That a state of either actual intoxication, or of half-drunkenness and irritability, is the immediate cause of *from three fourths to nine tenths* of the punishments which it is found necessary to inflict on board a ship of war, was the concurrent opinion of all the witnesses of various grades who were examined upon this point; and this opinion, based upon ordinary observation, was found to be justified, not merely by the examination of the lists of offences, but by experiments made for the purpose of testing it.

"That crime (says Captain Michael Seymour, in a letter to the Admiralty) bears a direct proportion to the quantity of ardent spirits consumed, is a truth of which ample proof exists in the statistical reports published, and open to the inquiry of all who may be disposed to inform themselves. To aid, therefore, in removing such a check to the moral advancement of our seamen, will not only give a fair field to the exertions of our naval chaplains, and encourage the anxious endeavours of our best officers; but our naval discipline, which the effects of a long peace, the delusions of meddling philanthropists, and an increase of knowledge, have shaken, will stand on a sounder basis, and 'command' will be rendered easier." (p. 122.)

Of the effect which even the ordinary allowance of spirits has upon some men, who are more than usually susceptible of its influence, the following remarkable example is given in the evidence of Captain Drew;—we cannot but believe that the loss of the power of self-control, here so remarkably evinced, is a much more common result of the use of alcoholic liquors than is usually supposed:

"I had a marine who was a very bad character, and he was constantly complained against for quarrelling and fighting, and disobedience to the orders of his sergeant. At length I began with flogging him; I gave him two dozen lashes, and told him that I would increase his punishment every time I had a complaint against him. In less than a month, I had another complaint against him, and I gave him three dozen; within another month, I had a complaint again, and it appeared to me that the man's reason was affected, as he was constantly excited. I therefore applied to the surgeon of the ship, and asked him to examine him, to see whether he was not a fit subject for invaliding. He was examined, and the surgeon reported that he

was as fine and healthy a young man as there was in the ship. I then did not think myself justified in flogging him again, but took upon myself to do an illegal act, with a good intention; and when we came into harbour (in the West Indies), I hired a cell in the gaol, and kept him there three days upon bread and water. When the man came out of gaol, I told him whenever I had complaint against him, as sure as we came into harbour, I would send him back to gaol. He said, 'Do you mean to say, that I am to be sent to gaol every time we come into harbour?' I said 'No, only in case of my having a complaint against you.' He said, 'Thank you, sir.' I said, 'Now I will start afresh with you; I will forget everything that has happened, if you choose to alter your conduct.' He said that he was very much obliged to me; and he came to me the next day, and asked me if I would stop his allowance of grog, and let him be paid for it. I did so, and never had another complaint against the man while I was in the ship." (p. 13.).

It is also the concurrent opinion of the witnesses examined on this point, that the disadvantages of the spirit-ration in regard to discipline, are not counterbalanced by any benefit as regards the maintenance of the health of the men, or to their power of sustaining heat, cold, or labour. The constant employment of this large quantity of stimulus, also tends to diminish the benefit which may be obtained from an extra-allowance under particular circumstances. And, what is perhaps worse than all, it is the most potent means of keeping alive that love of drink which has been the great bane of the sailor's life, and which (as appears from the evidence of various individuals well acquainted with the habits of the class) has been undergoing a spontaneous abatement during the last ten or twelve years, to an extent which is far from generally known. Thus Lieutenant Brown, the Government Registrar of Seamen, tells us, that there is now no difficulty in obtaining crews for ships in which no grog is allowed, although the men formerly invariably demurred, so that it was difficult to get them to sign the articles on such a condition. "Nearly all the articles signed in the Sailors' Home at Liverpool have the stipulation 'no spirits allowed;' and all the men who go there to be hired, know it very well, and they make not the slightest objection in any sort of way." He says further, "I have caused to be examined within this last week about fifty seamen, men of war's men, and others; and I have here their answers, which were taken down; and three fourths of them are for coffee or stout." And when asked whether he thinks that the general character of seamen has improved during the last ten or fifteen years, he replies, "Yes, in the last ten or twelve years they have become much more intelligent men. You can make them now understand things which you could not before; and in their appearance they are very much improved." The testimony of seventy-eight Shipping Agents in the different ports of the United Kingdom is nearly unanimous to the same effect; a much larger proportion of seamen being now found ready to serve on board "temperance-ships;" the number of such ships being considerably on the increase; and the men when on shore drinking less than formerly, the young (it being almost universally remarked) preferring beer to spirits, and being a better behaved class than the old.

It has been obviously from considerations of this kind, that the Committee have decided that a change in the mode of serving out the allowance will not meet the existing evils, and that a reduction in its quantity is imperatively called for. To what extent that reduction shall be carried, is a question which seems to have been carefully considered by them; and

here, as in many similar cases, there is a difficulty in reconciling what is *desirable* with what is *practicable*. Many of the captains and medical officers who were examined, give it as their decided opinion, that the health and morals of the men would be decidedly improved by an entire withdrawal of the regular spirit-ration, a discretionary allowance being issued when circumstances appear especially to demand it. In support of this it may be urged, that this system has now been in partial operation for many years in the merchant-service of this country, and still more completely in that of the United States, and that it has worked extremely well. The following are among the statements given to the Committee on this subject by Capt. Chadwick, of the American packet-ship, 'Sir Robert Peel:'

"For the last twelve years I have sailed on the strict principle of temperance,\* and have found it work well, and no complaint among the men; they were always ready to do their duty, and do it cheerfully, which I did not always find to be the case when spirits were allowed them. The American merchant-ships nearly all sail upon the temperance principle. Even in our whaling-ships, of which there are nearly 700 vessels, there is not one in twenty in which spirits are allowed; and thus far they have been very fortunate in their voyages, which are from two to four years, generally cruising in all climates where their voyages can best be accomplished." (p. 11.)

In bad weather Capt. Chadwick allows coffee; and we find that he is by no means solitary in the opinion, that, when extra-exertion is called for, warm tea or coffee would be preferable to "splicing the main-brace;" some of the men having given this as their own experience, although they admit that the grog would be generally preferred. The following testimony in favour of the working of this system is given by Capt. C. Hamilton, R.N., who may be regarded as an impartial observer, having had occasion to take his passage, a few years since, from the Cape to Rio, on board a merchant ship in which no spirits were allowed:

"We had severe weather off the Cape, but no spirits were issued. The crew was very cheerful and well-conducted, and the master was an abstemious good man, and a thorough sailor. Some of the crew had been men-of-war's men, and one had deserted from the navy. I spoke much to these men; they said they were well paid, well fed, and had a decent captain, and did not wish for spirits. They said they all liked their glass as well as others, but preferred good wages on the whole. *I felt secure and comfortable on board that vessel, on account of there being no spirits.* It was on this occasion that I was particularly struck with the advantage of a ship sailing without spirits on board; and from that time I have considered it as idle, to say that seamen are necessarily happier or more contented from being supplied with grog." (p. 21.)

An opportunity afterwards occurred for Capt. Hamilton to try the experiment on board his own ship, the 'Frolic;' for being off the coast of California, where no spirits could be purchased, except such as were of very bad quality, he asked the men whether they would object to go without them, and they readily agreed to the arrangement. After the first few days, he says, they seemed to forget all about it. They were nearly three months without spirits; and during that time the men were quite as healthy and happy as usual, or rather more so; and a considerable improvement took place in their conduct. Having been previously, however,

\* The word "temperance" here obviously means "total abstinence;" temperance-ships being those in which there is no spirit-ration, and no other alcoholic liquor being substituted for it.

a remarkably healthy and orderly crew, the positive benefit of the abstinence was not so apparent as it has been in some other instances. Among the reasons given by this officer for the measure he advocates—the entire abandonment of the issue of spirits as a daily ration—is its injurious effect upon the youths who are entering the service, both before the mast and in the midshipmen's mess :

“They considered it,” he says, “a manly thing to drink grog, and the example set before them leads to very injurious effects. I give my opinion chiefly with reference to the ships in which I first went to sea as a youngster, and where grog was drunk a great deal in my own mess, among boys of my own age, and the mischief that I saw it do to friends and messmates of my own age. I think the issuing of spirits in the fleet does more harm in the end to the officers than it does to the men.”

Capt. Hamilton alludes to many instances which have occurred within his own knowledge, of young officers having become habitual drunkards, from having been early induced to take off their tumbler of grog, not because they liked it, but because they thought it manly and sailor-like to do so ; and he affirms, that such is the influence of this example of spirit-drinking on the young officers, and such is too often its influence on them in after-life, and on the whole service, that he considers that the man who shall cause this terrible evil to be swept away will be entitled to the lasting gratitude of his countrymen. With an independence which does him great honour, Capt. Hamilton adds,—

“So confident do I feel, that to abolish spirit-drinking altogether in the navy would be to confer the greatest blessing upon it, tending especially to do away with the necessity of corporal punishment, and from my experience of the sound sense, gratitude, and high feeling of English seamen when properly treated, that I should not be in the least afraid of making myself unpopular with them by making known my opinions.” (p. 21.)

We now come to the medical testimony in regard to the influence of the spirit-ration on the general health ; and shall first cite that of Dr. J. Robertson, who has been in the service twenty-two years, and was surgeon to the ‘Enterprise,’ one of the ships in Sir James Ross’s Arctic Expedition. He gives it as his opinion, derived from his personal observation, that the more the spirit-ration is reduced, the better it will be for the health, strength, and general welfare of the men, if it cannot be done away with altogether ; and he considers that his statements apply equally well to tropical and to arctic voyages. With reference to the latter, he particularly states, that although he considers some “indulgences” necessary, when the men are suffering from cold, wet, or fatigue, yet he would prefer the employment of warm tea or coffee, where they can be had, to that of spirits. He says, too, that the whalers have no regular allowance of spirits ; only having an issue occasionally, when there is heavy work ; and he speaks of it as the regular practice, among the hunters of the North-West Company, and also those of Australia, to take tea instead of spirits on their journeys, from the experience of the more sustaining qualities of the former (pp. 85-7). Surgeon James Donnet, who has been ten years in the service, bears testimony to the frequently injurious influence of the spirit-ration in tropical climates. Several of the men on board the ‘Calypso,’ he says, who were subject to derangement of the digestive organs, caused by a long sojourn beneath a tropical sun,

would drink their allowance under an impression that it would strengthen them, and render them fitter for their work. Whilst the excitement lasted, this proved true; but as soon as passed, it was followed by a lassitude which occurred daily. Two of such cases terminated in liver-complaint. He is very confident that warm tea is preferable to spirits in case of severe labour in cold and wet; and states the following remarkable case, in proof of the preference shown in its favour, under circumstances of a peculiarly trying nature:

“I have heard an officer, who was in the ‘Quail’ cutter when wrecked in the Bay of Biscay in 1836, say, that for ten days (during which the weather was so bad, that the man at the helm was obliged to be lashed, the sea at times making a clean breach over her), although grog was offered the men on deck every hour, they refused it, preferring warm tea; not that they disliked grog, but tea proved in their case the most invigorating beverage.” (p. 88.)

This gentleman has since been sent out as surgeon in one of the recent Arctic Expeditions; and Dr. Carpenter has stated,\* that being in his company just after he had been examining and passing the men, Mr. Donnet mentioned that he had heard no expression of dissatisfaction from them at the prospect of the abstinence from spirits, which the Admiralty had prescribed as one of the conditions of this service,—as regarded the seamen, at least, no restraint being imposed on the officers. Mr. Donnet expressed the intention of himself adopting the abstinence principle, being thoroughly satisfied, that prolonged and severe cold can be better sustained without alcoholic liquors than with them.

Dr. Patrick Martyn, who has been twenty-three years in the service, states it as his opinion, that after great fatigue and exposure to cold and wet, hot tea, coffee, or cocoa are preferable to grog; and mentions that in North America, having more than once pilots on board in snow-storms, who remained at the bowsprit-end during twelve hours at night, they refused grog, though he recommended it to them, and took coffee in preference, as that which best enabled them to sustain this severe exposure. The only case in which he thinks it likely that the allowance of spirits is positively beneficial, is that of men exposed in boats to the malaria of swampy districts, as on the coast of Africa; and even then he thinks that hot coffee, could it be procured, would be just as efficacious.† Dr. Mansell, who has been seventeen years in the service, gives similar testimony.

“I think,” he says, “that a cup of hot tea or coffee would be more efficacious than ‘splicing the main brace;’ and it would enable a man to do more work than the stimulus arising from spirits, which only exists for a certain time. I dare say that a man would be able to go through more physical exertion with tea or coffee, that is, continuous exertion.” (p. 102.)

The recommendation which the Committee has determined on making, is probably the best that it could offer under the circumstances of the case. Their report proposes that the ration of spirits be reduced one half, and that the evening serving be altogether done away with; also, that no

\* Medical Gazette, Sept. 20, 1850.

† We understand that Sir W. Burnett has caused a strong solution of quinine to be prepared, and supplied to ships serving on malarious stations; a dose being served out as a prophylactic to such men as are thus specially exposed to the morbid influences which it may be expected to antagonise. The idea strikes us as a very good one, and as based on a far sounder principle than the administration of alcoholic liquors, which are not asserted by any one to possess any power of directly antagonising the malarious poison.



allowance whatever be issued to boys and cadets, except by the special direction of the captain. In this mode it is hoped that drunkenness and its attendant evils may be greatly reduced or entirely done away with; in the event, however, of its continued prevalence, the captain is to be left at full liberty to order the grog to be drunk at the tub (thereby preventing the transfer of any part of the allowance from one man to another), and also to stop the "compensation allowance," which it is proposed to give as an equivalent for the withdrawal of half of the spirit-ration, and which is more than twice as great as its money-value. It is pretty clear, that the Committee looks forward to the time as not far distant, when the change, which is now in progress in the minds of seamen on this subject, may render it practicable to abolish the spirit-ration altogether (except under peculiar circumstances); and when the Government service may no longer remain the stronghold of the intemperate habits, which are now banished from many of the best-appointed and best-navigated vessels of our commercial marine.

Before quitting the subject, we must briefly notice a statement which has recently been brought into prominence by the opponents of the temperance reformation, respecting the peculiar liability to scurvy, which is affirmed to exist among the crews of "Temperance-Ships." We are not aware that there is any other ground for this assertion, than that furnished by the following letters, which appeared in the 'Times' of April 2, 1850, under the heading "*Scurvy on board Temperance-Vessels.*"

"SIR,—I am desirous of giving publicity to the fact, that during the past twelve months I have had occasion to treat more cases among the crews of English ships visiting this port, than during any of the preceding five years I have conducted the medical duties of the station. Scurvy is almost exclusively confined to vessels coal-laden from England; and, as you are well aware, several ships have almost been disabled by it from both officers and men suffering; the worst cases seem invariably in such vessels as do not allow a ration of spirits. I would, therefore, most earnestly suggest to charterers and owners of vessels the propriety of allowing a portion of spirits daily, and that the quality of the provisions supplied to the ship be strictly attended to; also, that cleanliness of the person and clothes be strictly enforced.—I have the honour to be, &c., &c.,  
J. A. VAUGHAN.  
To Capt. S. B. BAINES, *Political Agent, Aden.*

"*Naval Department, Aden; Feb. 26.*

SIR,—I take the liberty of enclosing a letter from Dr. Vaughan, who has medical charge of the Naval Hospital at Aden. Since the letter was written, two more ships under temperance rules have entered this port, with their crews suffering severely from scurvy.—I have the honour to be, &c., &c.,  
S. B. BAINES."

Captain G. A. HALSTED, R.N., *Secretary, Lloyd's.*

Now we are constrained to say, that Dr. Vaughan's letter betrays a degree of ignorance and careless reasoning, which we are very sorry to meet with in a member of our profession holding an important public appointment. Meeting with numerous cases of scurvy on board coal-ships, and finding the worst cases to occur on board temperance-ships, (we give him the full benefit of the supposed fact, although his use of the strong term "invariably" is curiously qualified by that of the doubtful "seem,") he jumps to the conclusion, that the stoppage of the spirit-ration is the cause of this increased severity, and that its resumption is consequently



the remedy. Is there any one of our readers who would fall into such a transparent fallacy? We trust not. All the experience of those most conversant with the subject leads to the conclusion (as we have demonstrated on a former occasion, Vol. II, p. 439 et seq.), that the essential cause of the disease, without which no other can be effectual, is the want of fresh vegetables. So long as a ship is adequately supplied with fresh vegetables, whatever other morbid influences may prevail, no scurvy makes its appearance; but when the supply of fresh vegetables fails, the disease is sure to appear sooner or later,—the length of time that may elapse, and the severity of the attack, being dependent upon other accessory causes, some of which are known, others merely suspected. Thus, bad provisions, imperfect ventilation, filthiness of the clothes and berths, despondency of spirits, are universally admitted to be favouring or accessory conditions; and the utmost influence that, even upon Dr. Vaughan's own statement of facts, can be referred to the disuse of the spirit-ration, is that which is known to be exerted by the preceding causes. That the temperance system will not *prevent* scurvy on board-ship, when the essential cause is in operation, with the assistance of one or more of the above accessories, may be freely admitted; and consequently, the frequent occurrence of scurvy on board the coal-ships arriving at Aden need not excite the least surprise, when it is known that those ships are, for the most part, very badly provisioned, and when it is remembered that their crews are necessarily exposed to the depressing influence of continual emanations of carburetted hydrogen and carbonic acid gas, which make their way from the hold into the filthy ill-ventilated berths set apart for the men.

In the terrible outbreak of scurvy that occurred in the American squadron which blockaded Mexico, it was observed that, other things being equal, the disease was most severe on board the vessels in which the ventilation was most faulty. Now, in this and all similar attacks which have occurred in our own or the American navy, not the least influence for good can be attributed to the circumstance, that the spirit-ration was regularly issued; its utter valuelessness as a prophylactic against scurvy being shown by the fact, that a hundred years ago, when scurvy made its greatest ravages, the spirit-ration in the navy was nearly double what it is now. Consequently, it would need a very strong body of well-sifted evidence, to show that the crews of "temperance-ships" are, *cæteris paribus*, more subject to scurvy than those which have a spirit-ration, and that any good is to be expected from its resumption by those who have discontinued it. In the mean time, it would surely be much more judicious to attempt the entire *prevention* of scurvy, by the means which experience has shown to be efficacious, than to be resorting to a spirit-ration, with the very doubtful prospect of *mitigation* which alone it can afford. Instead, therefore, of "earnestly suggesting to charterers and owners of vessels the propriety of allowing a portion of spirits daily," we would enforce upon all whom it may concern the necessity of providing a due supply of fresh vegetables, in the full conviction that, with or without the spirit-ration, no scurvy will appear, so long as these last. The American whalers use potatoes for this purpose, and serve them out raw in slices with vinegar. Mr. Busk is of opinion, that the antiscorbutic virtues of the potato are not impaired by boiling; but we are inclined to think, that a smaller quantity will serve the purpose, when eaten raw, than when cooked; and

this, considered as a measure of economy, is of no small importance in a long voyage. Next to fresh vegetables, good lime-juice is undoubtedly the best antiscorbutic; but unfortunately a great adulteration is practised in this important article, and its value is consequently much impaired. To this circumstance is probably to be attributed the frequent appearance of scurvy, of late years, on board emigrant and convict-ships going long voyages, and even in the navy itself.\*

It is a little remarkable, that we should have the power of setting in opposition to Dr. Vaughan's hasty and ill-considered doctrine, the conclusions of so experienced an observer and excellent a reasoner as Dr. Bryson, on the very same subject. In the "Medical Times" for June 15, will be found a very interesting communication by that gentleman, on some of the phenomena attending the outbreak of scurvy in the British and American squadrons engaged in the blockade of the River Plate a few years since, and the comparative immunity of the French squadron, which was placed under the same conditions with the preceding in every other important respect, save this,—that instead of spirit-ration, the men had a daily ration of red wine. That this difference was the real source of the marked exemption which the latter enjoyed, appears not only from the statements of Dr. Bryson as to the impossibility of assigning any other cause, but also from the following circumstance, which may be regarded as almost an *experimentum crucis* :

"During the siege, besides the battalion of Royal Marines, there were two British regiments of the line for some time quartered in the town. To the soldiers, when it could be procured, a daily ration of rum was issued, or, in lieu of rum, a small sum of money, which was generally spent in the purchase of some even more pernicious liquor. On the other hand, to the marines, a daily ration of red wine was issued, and no spirit. Amongst the soldiers many cases of scurvy occurred, while the marines entirely escaped."

"*A more forcible argument than this,*" continues Dr. Bryson, "*in favour of the total abolition of the daily use of spirits in the navy, in the army, and in merchant-vessels, could hardly be adduced.*"—Now here we have the parallel cases of a French and English squadron employed in the same service, and similarly circumstanced in regard to supply of provisions, and yet presenting so marked a contrast in regard to liability to scurvy, that it was obvious that some very essential difference must exist in the conditions operating to produce it. The absence of all other ostensible differences leads to the suspicion, that the cause lies in the use of red wine by the one,—this wine, in virtue of the vegetable acids which it contains, exerting an anti-scorbutic influence, and having (from its very small proportion of alcohol) little or no positive deleterious agency,—whilst the other consumes a spirit-ration, which has no antiscorbutic properties, and is in other respects rather prejudicial than favorable to health. But it might be not unfairly objected to this inference, that the cause lay rather in the general condition of the ships, the difference of race and of general *physique* between

\* We have been informed, on high authority, that such was the lamentable condition of Sir James Ross's crew, probably in great part from this cause, its operation being aggravated by the severe and continued labour to which both officers and men were exposed during the winter, that, if he had attempted to pass another winter in the Polar regions, he would probably not have brought home one third of his crew alive. We believe that the most scrupulous care has been exercised, in furnishing supplies of the best possible kind to all the expeditions which have been sent out during the last year.

the French and English seamen, the difference of discipline between the two services, or some other of those almost indefinable influences which play no inconsiderable part in the etiology of disease. The case of the soldiers and marines, however, seems to afford—when taken in conjunction with the preceding—an indubitable proof, that the difference lay in the liquors consumed by the two classes ; since we have here two sets of men of the same race, and under the same external circumstances in every respect ; those who were served with red wine enjoying the same immunity as did the French sailors, whilst those who drank spirit, showed the same liability to scurvy as that presented by the English seamen.

Dr. Bryson's conclusion, from the whole of his inquiry into the etiology of scurvy, based upon the ample data supplied by the government returns, not merely from the navy, but from convict and emigrant-ships, are the following :

“ First ;—That although lemon-juice and sugar, that is, lemonade, will not prevent scurvy for an indefinite length of time, and in despite of all kinds of privation with respect to vegetable substances, yet it will generally, in vessels provisioned according to the present system, retard its evolution, according to the presence or absence of other predisposing causes, for three, four, or perhaps six months ; and when given in increased doses after the disease has broken out, it will, for a time, lessen the severity of the symptoms, and sometimes effect a cure. Secondly ;—That spirits are totally devoid of anti-scorbutic properties, and injurious to the health of men deprived of vegetable diet. Red wine, on the contrary, is, under similar circumstances, conducive to health, and assists materially in warding off the scorbutic diathesis. Thirdly ;—As the best means of preventing scurvy from breaking out in large masses of men, is a wholesome nutritious diet, consisting of fresh meat, vegetables, and farinaceous substances ; so, when the disease has occurred, these are the best, and, perhaps, the only remedies required to effect a cure.”

We trust that our readers will lend their aid in making these conclusions known among those who are concerned in the provisioning for ships for long voyages. It is extremely important for the health of the crews, that advantage should be taken of the teachings of the widest experience in this matter ; and it is certainly not less important, that the progress of the great reformation which is taking place in the drinking-habits of seamen, should not be checked by the careless reasoning and imperfect information of any individual, however well-meaning.

Since this article has been in type, we have learned with great satisfaction, that the Government of the United States has determined to discontinue the use of the “cat” in its Naval service ; and at the same time to abandon altogether the issue of spirits to the seamen. It will be obvious, from what we have already stated, as to the connection between intemperance and punishment on board-ship, that the first of these changes is so connected with the second, that it could not possibly (in the opinion of most naval men) be made alone, in the present state of our service, without great hazard ; but we trust that such an example of good conduct and sound discipline will be shown by the American navy, as may induce our own Admiralty to take the same steps ere long.

## ART. IV.

*The Principles of Surgery.* By JAMES MILLER, F.R.S.E. F.R.C.S.E., Professor of Surgery in the University of Edinburgh, Senior Surgeon to the Royal Infirmary, &c. &c. Second Edition. Illustrated by 238 Engravings on Wood.—*Edinburgh*, 1850. 8vo, pp. 803.

THE large additions which have been so rapidly made of late years to our knowledge of the pathology and treatment of surgical diseases, have led to the necessity for frequent revision and enlargement of systems of surgery. We thus find each of our schools requiring and being supplied with text-books, in which a condensed and collected view may be obtained of the principles of our science, and in which these principles are illustrated by the most recent discoveries in physiology, pathology, animal chemistry, and microscopic anatomy. The effect has been a most marked improvement in the character of modern systematic works on surgery, and their great superiority over those of the last century. In Edinburgh, the systems of Latta, Benjamin Bell, and Allan were long the text-books of the schools. They have been replaced by those of Liston, Syme, and Miller. The Edinburgh works must also have been the text-books of the English schools; for we find nothing of the sort before the publication of the lectures of Abernethy and Astley Cooper. Hunter's Lectures can never be considered as a system of surgery; and Pearson's 'Principles' remained incomplete. The successive large editions of Samuel Cooper's 'First Lines' and 'Dictionary' show how much such works were required; and their progressive increase in value fully kept pace with the improvement in the works of the Edinburgh School. In one respect, however, we have serious doubts whether the older are not superior to modern works; namely, in the more close and definite practical application of general principles. Professor Miller's works bring this consideration strikingly before us, inasmuch as he has separated into two distinct treatises the principles and the practice of surgery. Liston had done something of the same kind. He wished his 'Elements' to be considered as a surgical text-book for students, and his 'Practical Surgery' as a kind of application of the doctrines of the other work. But the latter was in reality a series of essays on certain special subjects, and the former was by no means confined to general principles. Professor Miller has been more successful; but he has laboured to do what it is impossible perfectly to accomplish. In no department of the profession are principles and practice so closely connected, as in Surgery; and in none should the connection be more jealously maintained. If science and art do not go hand in hand, surgery must degenerate into a handicraft. Had Liston formed one connected system of surgery of his two works, he would have produced the most perfect system of surgery in the language. Mr. Fergusson avoided the error of his great compatriot, and produced the work which has become so deservedly popular; but his space did not allow him to enlarge sufficiently on general principles. Professor Miller had great difficulties to contend with. He wished to go thoroughly through the scientific department, and yet neglect no practical detail. Space was limited; so he makes distinct works on principles and practice. A glance, however, at the table of contents of the work before us, shows how impossible it has been to

carry out the plan. We have long dissertations upon aneurism, wounds, fractures, &c.; but not a word respecting hernia, stone in the bladder, venereal disease, affections of the eye, and many other surgical diseases which certainly ought to be treated on 'principle,' and the principles of their treatment accordingly laid down, in any work purporting to be a systematic treatise on surgery. The fact is, that they are treated in the other work; and thus neither can be said to be really what it professes to be. In the work on practice some general principles must necessarily be discussed, while in that on theory several most important subjects are as necessarily passed over in silence. Were the two works united, their connected size would be far from equal to that of the two separate volumes; as the summary of practice in the one, and of principles in the other, now necessary, would be superfluous. It is only on such a plan, that we can hope to see a perfect text-book for surgical students—a complete systematic treatise on surgery. We know no one more capable than Professor Miller of writing such a book. Both his works are admirable in their way; but the plan on which he has proceeded is, in our opinion, faulty, for the reasons we have just given.—Having stated this, all the unpleasant portion of our critical task is over; and we can go through the volume before us with unmingled satisfaction.

The work is divided into three sections:—1. Elementary Disease. 2. Morbid action in certain tissues. 3. Injuries. It is introduced by an exceedingly well-written "Historical Notice of Surgery;" and concluded by a valuable appendix, entitled "Surgical Experience of Chloroform."

In the First Section we have various Constitutional Affections in Surgery; Fevers, Cachectic Affections, Affections of the Nervous System, and Affections of Internal Organs in connection with Surgical Disease. Next follow Inflammatory Action and Congestion, the Healing Process, Suppuration, Ulcers, Mortification, Hypertrophy, Atrophy, and Absorption, Tumours, and Hæmorrhage.

The Second Section comprises various Affections of the Integument, of the Serous and Mucous Membranes, of the Periosteum and Bones, of the Joints, Arteries, Veins, Lymphatics, and Nerves.

In the Third Section are Wounds, the effects of Heat and Cold, Fracture, Dislocation, Sprain, and Rupture of Muscle or Tendon, Bruise, and Suspended Animation from various causes.

It will be quite plain, after a glance at the above list of subjects, that we cannot be expected to enter into a critical analysis of a work in which they are treated,—since our whole number would not afford space for such a proceeding. Any one subject would afford matter for a long article. We must therefore content ourselves with making a few unconnected extracts in various parts of the work, and leave our readers to form their opinion from them of the value of the remainder. One chapter, however, we may most strongly recommend, that on Inflammation; but we must dismiss it without further notice, as we shall devote a large proportion of our space to this subject in another part of the present Number.

How admirable are the following remarks:—

"Or the water-dressing—simple or medicated, according to circumstances—may be continued until the end of the cure. But then comes the question, not unimportant, how often is such dressing to be renewed, the oiled-silk raised, the lint taken away, the redundant discharge gently removed, a fresh portion of lint laid



on, and the oiled-silk re-adjusted, with a slight retentive bandaging if necessary? The answer to such question is—as seldom as possible; as frequently as cleanliness demands, and no oftener. When discharge is seen soaking through the dressing, and beginning to drain away, renewal is had recourse to. For not only is the condition filthy, and as such affecting injuriously not only the patient but those around; but, besides, the discharge, becoming subject to chemical change, grows irritant, and may induce degeneration in the sore of an inflammatory type. There is then a necessity for change. But, until such necessity occur, let no change be made; inasmuch as it cannot be effected, however delicately and dexterously, without some injury being done to the tender surface by admission of atmospheric influence, as well as by rude mechanical contact. And by the oft repetition of this, again inflammatory degeneration may be induced. The “*nimia diligentia*” of surgery is fraught with manifold injury; and is an error against which the junior practitioner should especially guard. In practical surgery, nothing, however simple in itself it may appear, should be done without a good and substantial reason for its use.

“Another error, at least equally pernicious with too-frequent dressing, is an affected nicety in making the change of application; not only wiping away the redundancy of discharge, but insisting on a perfectly clean abstergence of the surface of the sore itself, till it look pretty and red; washing, sponging, rubbing, irrigating; thwarting Nature in one of her most beneficial acts; taking away, clumsily and rudely, the best protection of the tender surface; and invoking inflammatory action, or tendency thereto, with consequent degeneration of the sore. At each dressing, gently wipe away pus from the surrounding integument, but do not interfere with that which covers and protects the granulations; our dressing is subsidiary to this, and ought not to supersede it.

“The means whereby the cleansing is effected are also a matter of some moment. Usually, it is by a sponge. But this is likely to prove injurious, especially in the wards of an hospital. A sponge is a thing of some value, in the eyes of a patient or nurse, and not to be lightly parted with. It is used not for one patient only, but for many, or all. It becomes soaked with discharge of various kinds; it is hastily and imperfectly cleansed after each employment; and, ere its daily course is run, can hardly fail to have been the means of conveying noxious matter to previously healthy sores; inducing their degeneration, and perhaps exciting the serious complication of erysipelas. Instead of sponge, therefore, especially in hospital practice, let fine tow, lint, or soft linen rag, be used as the abstergent agent, a thing of no value, and which, consequently, may be burnt as soon as used, and have no opportunity of carrying contamination. And, generally speaking, the basin of cold water, usually in attendance during the dressing, may be well dispensed with. Dry and gentle wiping of the surrounding skin, leaving the actual sore untouched, is all that is required. More is not only unnecessary, but tolerably certain to prove injurious; it belongs to the noxious “*nimia diligentia*.” (pp. 238-9.)

Here again the sound practical surgeon is seen :

“Some, taking an abstract view of the subject, entertain a question as to the expediency of operating at all in carcinoma; inclining to regard the affection as wholly constitutional, and not to be eradicated, or even restrained, by removal of only a local portion of it. This view we do not propose to consider; but, with the majority of the profession, granting that the disease is constitutional as well as local, and that in most cases it shows more of the former than of the latter character; granting that very many cases occur—doubtless the majority—in which operation is inexpedient; and granting that in all cases, looking to the constitutional vice, we can never be certain of immunity from return, and must invariably issue a guarded prognosis accordingly:—still, we are of opinion, that there are cases, often presenting themselves to the surgeon in extensive practice, in which it is his bounden duty, by operation, to afford his patient the chance either of a definite and radical cure, or at least of a postponement and palliation of the malady. Such cases are those in which the tumour is yet small, and comparatively circumscribed; the lym-



phatics unchanged, either in the immediate vicinity or at a distance; the integuments and muscles free from incorporation; the patient not far advanced in years; and the cachexy as yet but little indicated, if at all. On the other hand, affections of the lymphatics, already begun, even though to a trifling extent, contraindicates operation; for, according to experience, reproduction is sure to follow, even when the surgeon is certain that not only the tumour itself, but the adjoining changed structure as well, lymphatic or not, has been thoroughly taken away. Incorporated skin and muscle can be removed, by wide and free incision; yet, in such cases, it is often difficult, if not impossible, to say that what is left is sound, free from lodgment of the *materies morbi* already in its texture; and, in these circumstances, experience again speaks loudly in favour of return. In the very old, a carcinoma may exist for years, in a latent or indolent condition, still occult, and still of small size and circumscribed, the seat of little uneasiness, and attended with but little disorder of the system; indeed the patient may die, ultimately, of disease to all appearance totally unconnected with the carcinoma. Under such circumstances, operation is withheld; the tumour is left undisturbed, and guarded carefully from excitement. But while thus, in the patient of seventy, the progress of the tumour is slow, and the indications of cachexy weak or apparently absent—the opposite obtains in regard to the patient of forty. And when, at such age, a tumour is advancing rapidly, with a marked cachexy at the same time consuming the general frame, it is prudent to abstain from the knife, even though the lymphatic system seem as yet wholly uninvolved; for, in such cases, the probability of return is extremely great; the disease being not delayed by the operation, but truly undergoing exacerbation. And thus we see, that extreme activity of the disease in the comparatively young, and extreme indolence of it in the aged, both alike contraindicate operation. It may also be observed, that, *cæteris paribus*, return is more probable in the case of the open tumour, than of the occult." (pp. 326-7.)

In the treatment of Periostitis, the author recommends a practice well worthy of a further trial:

"Sometimes action seems to be partially arrested, yet does not decline; a tense and painful swelling remains, unabated, and on the contrary tending still to increase. It is plain that relief of tension would be a most important indication in such circumstances. For a similar state of matters, unconnected with bone, we would freely practise incision; tension would be at once relieved, and action would speedily decline; the wound would suppurate, and its margins perhaps slough, but granulation and closure would speedily follow. Here, however, similar procedure would be rash and untoward. Tension would doubtless be relieved; but, with suppuration, which is inevitable, would certainly come either ulceration or death of the bone; the very results which we seek to avoid. Direct incision, therefore, is plainly unwarrantable. But, by inserting a fine bistoury or tenotomy needle, at a little distance from the tense part; passing it over, cautiously, beneath the integument; then turning and pressing its edge, so as to divide the tense membrane wholly to the desired extent; cautiously withdrawing the instrument, so as to make a valvular, oblique, and subintegumental wound; and finally closing the single integumental puncture immediately, with isinglass plaster, or collodion—in fact by completing the wound so as effectually to prevent introduction of atmospheric influence, and thereby obviating the chief risk of suppuration—we may obtain diffusion of the swelling, relieve tension, and so facilitate both resolution of the action and discussion of its results. This manœuvre, however, requires skill and caution in its performance; and even with these is not wholly devoid of risk. It is therefore not to be indiscriminately employed, but should be reserved for those cases which otherwise prove obstinate, and in which aggravation and suppuration seem imminent." (pp. 417-18.)

The subcutaneous or valvular mode of removing loose cartilages from joints is here well described:

"In the first place, the patient is to be prepared for the operation. For a day or two, the limb is to be disused, so that previous excitement may have thoroughly subsided. Low diet is enjoined, the primæ viæ are gently yet efficiently cleared, and general secretion is seen to be in a satisfactory state, so that there may be no predisposition to inflammation. Then the foreign body, having been made superficial, is gently pushed to the extreme verge of the synovial pouch; either on the inside or on the outside of the patella, as may be most convenient. The internal position is usually the preferable; and there it is retained fixedly, by the fingers of an attentive and steady assistant. A tenotomy needle, or thin and narrow bistoury, of fine edge, is passed in an oblique direction; and an incision, a little larger than the outline of the cartilage, is made through the tense synovial membrane. The instrument is then withdrawn slowly and cautiously, the finger gently yet firmly following and consolidating its track. A few drops of blood escape, but not a particle of synovia; and no air has obtained admission, even to the areolar tissue. The integumental wound is immediately and carefully occluded, by plaster or collodion.

"The foreign body is then gently pressed through the aperture in the synovial capsule, which aperture, as has just been stated, is made sufficiently free to admit of this being accomplished without force or difficulty. When exterior to the capsule, it is coaxed through the areolar tissue—sufficiently lax, readily to admit of this—by gentle pressure of the fingers; not in the track of the puncture, but in a different direction, probably at nearly a right angle to it. When about an inch and a half, or two inches, from the synovial wound, it is there permitted to remain. Not permanently, however, as has been proposed. Otherwise, acting still more as a foreign body in its recent and raw site, inflammatory action is excited, suppuration is all but inevitable, and extension to the synovial membrane becomes extremely probable; the very result to the avoidance of which all our pains had been directed. For two days, or three at the utmost, it is suffered to remain in its new locality, undisturbed; the most careful prophylactic treatment being meanwhile employed, both generally and locally, so as to avert undue excitement. By that time, the synovial wound will have closed by adhesion; and both tracks—that of puncture as well as that of extrusion—will have been consolidated. Then, the substance having been fixed as before, a direct incision is made upon it; not more free than is sufficient for its ready removal. After it has been lifted out, the superficial and slight wound is brought together by strap; and, in all probability, it unites by adhesion." (pp. 530-31.)

The following extracts from the opinions of the Edinburgh professor on the value of the treatment of Aneurism by pressure, will be read with interest. They are written in the true eclectic spirit.

"No doubt, some surgeons have experienced remarkable success in tying particular arteries. Mr. Syme, for example, has been very fortunate with the femoral. But it is more than probable that, as in similar sequences of success in lithotomy, and other capital operations, the progress of time will include disastrous cases, which will not fail to bring down the result to a more ordinary average.

"In tying an aneurismal artery, no skill in the operation can altogether avert risk.—Of phlebitis; for the vein usually is in immediate apposition with the artery, and must be exposed to some roughness of manipulation.—Of gangrene; for, impression on the limb's circulation is inevitably both great and sudden.—Of hæmorrhage, above all; for the artery tied is necessarily more or less altered in structure and function, and consequently prone to the asthenic results of inflammatory action. Had there been no chronic arteritis, there had been no aneurism. Pressure, unskilfully and rashly applied, is at least equally capable of causing these risks. But pressure, employed according to the principles now enjoined, in its modern revival, escapes them all.

"That pressure is to supersede ligature altogether, in the treatment of aneurism, no one can imagine. Some patients, by idiosyncrasy, may be intolerant even of such

modified pressure as is required; and to some aneurisms the application of it is impracticable. There seems every reason to anticipate that the result of experience will determine, that some aneurisms are unsuitable for either deligation or pressure; absolutely incurable, by reason of universal and extreme arterial degeneration. And that, of the curable cases, some are suited for pressure, others for deligation; just as, in stone, patients are not indiscriminately subjected to lithotomy as in former years, but while some are cut, others have the calculus removed by lithotripsy; neither operation being exclusively followed in the practice of any wise and thoroughly-accomplished surgeon. And further, it will no doubt appear, that there are certain cases of aneurism, in which it were both unwise and unwarrantable to operate by the knife, when experience has shown that pressure will suffice; just as we should be culpable in removing a stone by wound of the bladder, which experience tells us may be safely crushed, or removed by the urethra entire. . . .

"Mr. Cusack also has communicated to me the notice of dissection in the case of a patient who had been cured of aneurism by pressure in October 1843; and who died in December last, of diseased brain. The femoral artery was pervious, and of ordinary size, down to the site of the tumour; there its cavity was obliterated, and its coats flattened out into the form and appearance of a ligamentous band. The obliteration extended to about two inches and a quarter, occupying the last inch of the femoral, and the upper one and a quarter inch of the popliteal artery; beneath this point the vessel again becoming pervious, and nearly of its natural size. The profunda and its branches were of ordinary bulk; and the branches of the popliteal were not above the usual dimensions. The vein accompanying the main artery was full of blood, but quite pervious, from the groin down to the lower extremity of the popliteal space. Its external characters were normal throughout; excepting just opposite the obliterated portion of the artery, where the coats of the vein were much thickened and indurated. The internal saphena vein was large, and filled with a fibrinous clot.

"In this case the points of pressure had been in the upper and middle thirds of the thigh, alternately; and it is most satisfactory to have such ample proof of the curative agent having produced no evil effect whatever, on the vessels directly implicated." (pp. 584-86.)

Professor Miller has adopted a plan of protecting raw granulating surfaces from the influence of the atmosphere, by imitating the incrustation of nature. He uses a thick semifluid aqueous solution of gum tragacanth. This is not open to the objection of pain in application, which may be urged against the use of collodion, and appears to be worthy of further trial. The mucilage is—

"Laid gently and uniformly on the raw surface, so as completely to protect it; and if at any portion the envelope threaten to become imperfect, the attendant is directed to effect an immediate repair. The application is productive of no irritation; and, being translucent, permits a complete surveillance of the part. Atmospheric influence is completely excluded; and the raw surface would seem to be placed in circumstances somewhat analogous to its normal state, as if still invested by the integument. Should inflammation ensue, no harm has been done; on the contrary, action is likely to prove less intense than it otherwise would have been; the gum is loosened and washed away by the purulent secretion; and water-dressing may then be used, as in ordinary circumstances." (p. 643.)

In the treatment of Tetanus, our author speaks strongly in favour of the value of the *Cannabis Indica*, from his own experience.

"I can now record three fortunate cases under its use; all traumatic. A girl, eleven years of age, sustained comminuted fracture of the finger. Tetanus occurred, the finger was amputated; and the treatment consisted of purgatives, cold to the spine, Indian hemp—pushed to narcotism—nourishment, and seclusion. The

amendment was gradual and complete. A boy, about the same age, had simple fracture of the thigh, with compound and comminuted fracture of the great toe. The treatment and result were the same. Another boy, rather older, had compound fracture of the bones of the arm. The treatment again resulted in cure. And in these cases I was and am inclined to award to the cannabis the greater part of the therapeutic agency. In other examples of the disease, I have seen it fail to cure, but never to relieve. It is given in doses of three grains of the extract, or thirty drops of the tincture; repeated every half hour, hour, or two hours; the object being to produce and maintain narcotism. There is a very marked tolerance of the remedy." (pp. 684-85.)

Again in the treatment of false joint after ununited fracture, the experience of the author has led him to propose a method of procedure, which is a great improvement upon former plans.

"When the principle of "subcutaneous incision" came into use, the idea struck me that this important addition to surgery might be made available towards the remedy of ununited fracture; and accordingly I proposed that a strong needle, having been passed obliquely down to the part, should have its edge freely moved about in all directions, so as to cut up the ligamentous bond of union, as well as the dense investment of the ends of the bones; the needle being then carefully withdrawn, and the puncture covered by plaster or collodion. The parts will probably be reduced to a state very similar to what attends on ordinary fracture at the first. A pouch of blood will form; the blood will be absorbed; fibrin will take its place; inflammation being absent, the plasma will become organised, while, at the same time, secretion and organisation may advance from the ends of the bone; and consolidation, as by definitive callus, be completed.

"My experience, as far as it goes, speaks in favour of the practice. Lately, this method succeeded, quite beyond my expectation, in consolidating an ununited fracture of the humerus, which had sustained compound injury about ten months before. The bones overlapped, and could not be adjusted. Altogether the case was so very unpromising as led me to remark, while performing the subcutaneous puncture, that it was an unfair test of the practice; and that, under such circumstances, a successful issue could scarcely be expected. Yet, on the first undoing of the splints, five weeks after the puncture, the parts were found quite firm." (p. 724.)

With the following hints as to an every-day occurrence, the treatment of sprains and bruises, we must close our series of extracts:

"Nothing is more common than to apply leeches immediately after infliction of the injury, in order that they may suck out the extravasated or 'bruised blood,' as it is called. But these little animals drink only from the running stream, drawing for themselves from the blood-vessels; and, therefore, they fail to perform what is expected of them by their employers. At the same time, their bites, admitting the external air to the extravasated blood, are likely to induce suppuration in the areolar tissue. They are of use only at a more remote period, to moderate inflammatory action, occurring as a secondary result of the injury. Friction, in like manner, is often employed from the first, and of a stimulating nature. The result is, to induce and aggravate inflammatory action; an event which it ought to be our main endeavour to avoid. Friction is expedient only after the period of excitement has passed; and, even then, it must be at first employed gently and with caution." (p. 746.)

We shall take another opportunity of referring to the admirable appendix on the "Surgical experience of Chloroform." Like every other section of the work, it is fully, fairly, and ably treated.

Having, in our introductory remarks, so freely criticised the plan of the

present volume, we must in justice express our most unqualified approbation of the manner in which every subject has been handled that finds a place in it. Nothing is slurred over to make room for a more favorite topic. Nothing like prejudice or unfairness can be anywhere detected; nothing like imperfect knowledge of any department, either of scientific or practical surgery. On the contrary, the truth is calmly and deliberately sought for; conflicting opinions and antagonistic facts are fully examined and conscientiously weighed; and the reader is supplied with the grounds upon which the conclusions of the author are founded. He rises from perusal with the conviction that the author is a sound and accomplished surgeon, thoroughly acquainted with the literature of his profession, a man of great personal experience, and a teacher endowed with the merit, somewhat rare in these times, of being able to communicate what he knows to others in a clear, simple, and agreeable manner. The style is exceedingly good. A sort of attempt at fine writing, an exuberance of ornament, was complained of, and not altogether without reason, by reviewers of the first edition. This fault has been carefully corrected, and we should now find it difficult to name a surgical work that is better written. It is beautifully printed on good paper, is copiously illustrated with excellent wood-engravings, and forms a very handsome volume. For all these reasons we recommend it most strongly, and congratulate the author on having produced a work so creditable to himself and to the school of Edinburgh.

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ART. V.

*Iodognosie, ou Monographie Chimique, Médicale et Pharmaceutique, des Iodiques en général, et en particulier de l'Iode et de l'Iodine de Potassium.* Par F. DORVAULT.—Paris, 1850.

*A Chemical, Medical, and Pharmaceutical Monograph upon the Iodine-Compounds in general, and especially upon Iodine and Iodide of Potassium.* By F. DORVAULT.—Paris, 1850. 8vo, pp. 270.

THIS work originated in an Essay submitted in competition to two societies, both of which awarded it the prize. The subject proposed by the Medical Society of Lyons related to the Iodide of Potassium; while the Society of Sciences of Hainault left the choice of a subject to the competitors. Before publishing the Essay, however, its author has made many additions, so as to convert it into a complete monograph upon Iodine and its compounds. Being himself only a *pharmacien*, though an able one, he naturally feels that an apology is due for meddling with the medical portion of the subject; but in respect to this he contents himself with collating the facts which have hitherto been published, and with recording additional testimony in favour of the iodine medicines, imparted to him by Lugol, Velpeau, Ricord, Cullerier, and other authorities. This portion of the work has rather too much the character of a mere eulogium, and would probably have been better executed by the collaboration of a practitioner, who would have tempered zeal by the remembrance of disappointments, and would not have viewed the subject with an eye so entirely chemical as the author has done, as may be judged from his fundamental proposition:—"Iodine is a specific fluidifier, and every morbid affection recognising for its cause or effect a humoral coagulation, more or



less plainly indicates its employment." The other departments of the work, however, leave nothing to be wished for, and constitute it a valuable manual of reference for all that relates to the chemical and pharmaceutical history of iodine.

A little enthusiasm may well be excused in regard to iodine, when we consider what an extensive diffusion it possesses among natural bodies, several of which are in deserved reputation for their instrumentality in maintaining or restoring health; and how largely it administers to our aid, in its separate state, in the management of some of the most intractable forms of disease. Improved means of analysis are every day exhibiting a wideness of range of this substance, that was formerly by no means anticipated. M. Dorvault is enabled now to furnish a list of fifty mineral springs, in which it exists in a greater or less proportion; and Professor Cantù has found a remarkable absence of goitre and cretinism in those alpine regions, wherein the waters contain minute quantities of iodine and bromine. And that it exists in waters in which its minute proportion does not manifest itself to the chemist's reagents, is seen in the fact that Professor Chatin has discovered it in a vast number of aquatic fresh-water plants, which he has recently examined for that purpose. The preparation which M. Dorvault regards as embodying all the excellencies of iodine, exempt from its defects, is the *iodide of potassium*; and he has recently, in a communication to the Académie des Sciences, maintained that it is in this state that the element exists in the marine plants and other organic substances in which it is found. Iodine has little affinity for soda, and the compounds so formed are easily decomposed, and the iodine evaporated. For this reason it was not detected in the Vichy and other waters abounding in soda, until it had been fixed by the addition of potassium.

In the *chemical* portion of the work, however, the different modes of obtaining the iodine and its iodide are critically examined, the physical and chemical characteristics of the two substances are detailed, and the numerous adulterations to which the *iodide of potassium* is liable, are exposed. M. Dorvault, on account of the difficulty of obtaining this substance pure, and its heavy price, recommends all *pharmaciens* to prepare it for themselves, for which purpose he regards Pyper's process, with some modification, as the best: 100 parts of iodine are gently heated with 30 of iron filings, 75 of carbonate of potass, and 144 of water, the mass being rapidly dried. The red powder is treated with water, which is then filtered, and evaporated to dryness. An improvement consists in treating the dried mass with alcohol in place of water, removing this by distillation after filtration.

We cannot enter into the long account which the author gives of the means of detecting iodine for medico-legal purposes; but we may mention the simple and effectual plan followed by Rayer for ascertaining whether a patient's system has become impregnated with it. This consists in the production of a blue colour, by touching with nitric acid a piece of starched paper that has been moistened with saliva or urine, the latter being previously reduced to 1-16th of its volume by evaporation.

The *medical* portion of the work contains little of novelty, in respect to the employment of this substance; but we may notice the author's views of its *modus operandi* a little more fully. These are, in fact, based upon



experiments which have been instituted for ascertaining the effects of the iodide upon the different humors,—as the blood, lymph, serum, and milk; or upon their protein elements (albumen, fibrin, casein, &c.); the results of which showed that it opposes the coagulation of the former, and fluidifies the latter. As no change in composition of either the bodies acted upon, or of that employed, takes place, this singular effect is produced by mere contact through the agency of the power which chemists term catalysis. Those writers who regard iodine as stimulant, hyposthenic, or alterative, the author regards as mistaking mere secondary or consecutive effects for essential and efficient ones; while Dr. Cantù's hypothesis of the iodine becoming converted in the blood into hydriodates by means of the hydrogen it finds there, cannot apply to the most efficient preparation, the iodide of potassium. An important consideration in reference to this fluidifying power of iodine, as compared with that of alkalies and other fluidifiers, is, that while it exerts this effect upon the fibrine, it spares the globules.

The system having once given admission to the iodide, discharges it with remarkable rapidity and completeness. Scharlau, of Stettin, recovered daily from the urine 345 of the 350 of the centigrammes administered. Kramer found that, after employing it for fifty days, six sufficed for its complete elimination. Marchal (de Calvi) found it in the urine in from twenty-five to sixty minutes, after even only a grain had been given. Of 15 grains, 0.883 parts were recovered from the urine in thirty-six hours, leaving only 0.117 to be accounted for. When iodine itself is given, it becomes quickly salified.

We need not follow the author in his account of the utility of this medicine in *goitre*, *scrofula*, and *syphilis*, and some other diseases of less consequence, in which it is less certainly useful. Indeed, he conveys no new information in reference to these diseases, or to the surgical employment of iodine in injections and as a local application; while a tone of exaggeration prevails throughout his statements, as *e.g.* when he attributes the whole medicinal virtue of the cod liver oil to the iodine it contains. All the affections in which iodine is useful, are stated, in conformity with his views of its *modus operandi*, to be characterised by a preternatural disposition to the formation of coagulations. However admissible such a position may be as regards goitre and scrofula, it would seem quite absurd in the case of syphilis; yet M. Dorvault believes that some support of his views is derivable from the analyses of the blood in this disease, which have been made by M. Grassi. According to these, the proportion of the globules becomes much diminished, and that of the albumen relatively increased, in secondary syphilis; but, after the employment of the iodide, exactly the reverse is observed. The author's hypothesis is, that with the progress of syphilis, the globules become resolved into albumen, and, under the iodine treatment, a greater homogeneity of the fluid is established, whilst, by a reparative process, the globules are reformed at the expense of the albumen. In tertiary syphilis, the employment of mercurials has a tendency to diminish the quantity of globules, and, therefore, they are better suited for the primary stage, and for the inflammatory complications of the secondary, during both of which globules are in excess. The iodide, on the contrary, whilst ill-suited for these two conditions, is well adapted to remedy the impoverishment of globules

which occur in the tertiary form. The author wishes it to be understood, that when he says that iodine acts as a fluidifying agent, he does not mean that it renders the blood more fluid than normal, but that it removes any morbid disposition to coagulation that may prevail. Coagulation, too, is not synonymous with plastification, as it may be even due to a deficiency of cohesive power among the constituent elements of the blood.

Iodine itself is a substance of so irritant a character, and one so little susceptible of pharmaceutical combination, that had not a more tractable preparation, in the shape of the *iodide of potassium* been resorted to, this medicinal substance must soon have been abandoned. It was only had recourse to at first, for the purpose of rendering the iodine more soluble; and, although it thus enabled the iodine to enter into a greater variety of formulæ, the irritative qualities of this substance still continued. Happily, however, the iodide itself was soon discovered to be the most valuable of compounds; containing 76 per cent. of iodine, being susceptible of a great variety of pharmaceutical combinations, and operating, even in large doses, with no disagreeable effects. It has, therefore, naturally superseded the iodine, although, in some few cases, a combination of the two seems to be necessary. The author recommends, that the *dose* of the iodide should be 5 grains per diem, divided into two doses to begin with, increasing it by 5 grains every few days, until 15 grains per diem is attained; the last amount may be continued for the rest of the treatment of the case, unless particular reasons present themselves on the one hand for diminishing it, and on the other for augmenting it, when it may be gradually carried as high as 2½ drachms per diem with safety. In syphilis, larger doses may at first be required, and Ricord always tests the patient's susceptibility with a scruple a day, divided into three doses; whilst, if the medicine is found to agree, he gradually increases it to 45 grains per diem.

In the author's opinion, much of what we read of the *injurious effects* of iodine, is due to the want of due adaptation of the dose, and to the incautiousness of practitioners in this particular. When the medicine is given in too-concentrated a state, a dryness of the *buccal mucous membrane*, with detachment of the epithelium, may result. Sometimes iodine induces *ptyalism*, in which, however, the mucous membrane is never inflamed and ulcerated as in mercurial salivation, nor is there any special odour present. In persons who have a susceptible *bronchial membrane*, iodine sometimes induces an irritating cough. The *digestive canal*, too, in some, in place of the usual favorable stimulation, becomes much irritated, so that purging and vomiting result. In other cases there is only severe epigastric pain, relievable by giving the iodine in mucilaginous substances, or combined with opium. Of all the mucous membranes, the *Schneiderian* is most liable to irritation, a great quantity of mucus being secreted, having less viscosity than in ordinary catarrh, and possessed of no tendency to purulence. Iodine is easily detected in it. Iodine usually acts powerfully upon the *kidneys*, often giving rise to an abundant but innocent diuresis, though in a case of M. Ricord's this amounted to diabetes, 50 *litres* of urine being discharged in a day. The *skin* not unfrequently becomes irritated, acute eruptions of different kinds appearing in the face, especially the nose and forehead: In a few rare cases, the enamel of the *teeth* has suffered from the prolonged use of iodine preparations. The gravest charge that has been brought against iodine, has been the induction of

*atrophy* of certain organs, as the mamma or testis, or even of the entire organism. M. Dorvault regards such cases as in the highest degree exceptional; and he believes that the diminution of the doughy tumefactions so often observed in lymphatic subjects, has been mistaken for atrophy of the organs themselves. As M. Richelot observes, a swelled testis often afterwards becomes atrophied where no iodine has been given; and some very obese women mistake the removal of the excess of fat for a diminution of their breasts. At all events, taking the mass of cases, the effect of iodine is found to be favorable to development, rather than to induce emaciation. Lugol observes, that he has often seen young scrofulous girls undergo a development of their breasts, as well as of the other organs, while under the influence of iodine; and Ricord has remarked the same in syphilitic patients. This effect of iodine, too, is inconsistent with the aphrodisiac power attributed to it, and which it seems to exert upon the scrofulous patients at St. Louis. M. Dorvault does not, however, mean to deny these ill effects are sometimes produced, but he maintains that they, as well as the *iodism* or *iodine intoxication*, mentioned by Coindet and others, are of very rare occurrence, being due either to idiosyncrasy or to abuse of the drug.

We have little to say respecting the *pharmaceutical* portion of the work. M. Dorvault has collected more than 100 formulæ from different authors, and has accompanied them with critical comments not always of a very favorable character. Although not approving much of formulæ, he has added a few which he believes are of greater simplicity or efficiency than some of those he has quoted. For the administration of the *iodide of potassium*, when the patient does not object to the taste, the best menstruum is simple distilled water, to which, however, if the patient's taste require it, some of the various aromatic waters or infusions may be added. A good form for children, and in cases where the dose required is not very great, is the biscuit; 10 grammes of the iodine being dissolved in its own weight of distilled water, and added to sufficient biscuit-paste for 100 biscuits, the dose being from 1 to 10. When the digestive organs are in an irritable state, the medicine may be advantageously given in the form of injection (1 gramme to 250 of water). A *syrup* of the iodide may be formed, by dissolving 10 parts in 20 of water, and adding it to 800 of simple syrup, and 200 of orange-flower syrup. Another syrup, which M. Dorvault says is in much demand, is formed by dissolving 1 part of the iodide in 500 of horse-radish syrup (this plant being regarded in France as a tonic suppurative), and is very suitable for children. When the system is apathetic, and in need of stimuli, *iodine* may be added to the iodide, as in the following convenient formula for drops: *Iodine* 1 part, *iod. pot.* 10 parts, *water* 90 parts; every 10 drops thus contain 1 grain of the iodide, and  $\frac{1}{10}$  grain of iodine. Both the iodide of potassium and the iodined iodide may be given in white or red wine. As a local application, M. Dorvault prefers the iodide of potassium made into an ointment with 10 times its weight of lard. As a liniment, which is useful in the resolution of *engorgements* of the joints, after the acute stage has passed away, as also in chilblains, he recommends the following: *Iod. potass.*  $2\frac{1}{2}$  parts, *muriate ammon.*  $2\frac{1}{2}$  parts, *spt. camphor.* 100 parts.

M. Dorvault's observations upon the *Incompatibilities* of Iodine, have been already inserted in his Journal (vol. vi, p. 278).

## ART. VI.

1. *Delle Malattie e delle Operazioni della Ghiandola Tiroidea.* Di LUIGI PORTA, Professore di Clinica Chirurgica in Pavia. Con quattro Tavole in Rame.—*Milano*, 1849.

*On the Diseases of, and the Operations on, the Thyroid Gland.* By LUIGI PORTA, Professor of Clinical Surgery in Pavia. With Four Copper-plates.—*Milan*, 1849. 4to, pp. 164.

2. *The Cyclopædia of Anatomy and Physiology.* Edited by ROBERT B. TODD, M.D. F.R.S., &c. &c.—Art. *Thyroid Gland.* By C. HANDFIELD JONES, M.D.—*London*, 1850.

PROFESSOR PORTA is the successor of the illustrious Scarpa. He occupies the chair of his great predecessor in the University of Pavia, and bids fair to emulate his fame. His great work on the 'Ligature and Torsion of Arteries,' made known to British medical men in Volume XXII of the 'British and Foreign Medical Review,' has already procured for him a European reputation, and the last award of the Monthyon prize. Appreciation and reward have stimulated him to renewed exertion. The result is the work before us, not offered as completely exhausting the subject, but with the hope, modestly expressed in the preface, that it may advance in some degree the science and art we profess and practice.

The work is divided into seven chapters. The first consists of certain anatomical considerations; and in the succeeding ones are treated inflammation of the organ—hygroma—hypertrophy—simple organic productions—scirrhus and fungous degenerations. Lastly, with respect to the treatment of bronchocele, the conditions and effects of resolvent means are established, the different operations are examined, and a new method of operating is proposed, which the author has found by experience to be both useful and safe.

Among the anatomical considerations, the following remarks upon the vascularity of the thyroid gland appear to be worthy of attention:

"The thyroid arteries, as they issue from the carotid and subclavian, form large angles, run in an opposite direction to the trunk in a tortuous manner, and subdivide, forming repeated digitations before they penetrate; the object of these dispositions being doubtless to diminish the impetus of the sanguineous current in the substance of the gland where these arteries terminate. Indeed we can scarcely obtain a successful injection of the thyroid arteries by propelling anything through the aorta, or by the carotid or subclavian, although the thyroids are short, very near, and of large calibre. The arteries in branching off upon the surface, and in the tissue of each lobe, form among themselves a network of external and internal anastomoses, which are so slender and tortuous (as the natural anastomoses of the arterial system in man in general are), that a coarse substance injected into one of the arteries frequently passes with difficulty into the others. But when the sanguineous system is increased by an enlargement of this organ, and in some cases even without any morbid enlargement, we find upon the surface, on the margins of the isthmus and of the lateral lobes, anastomoses increased to half or a whole millimetre between the right and left, the superior and inferior arteries. When one thyroid artery is injected by a very fine substance, as size or glue, the other trunks are always very easily filled, with the whole venous system, and the parenchyma of the gland itself. When, after a fine injection, thin slices are cut from the surface of the fleshy tissue, and are examined, either fresh or dried upon a slip of glass, under a microscope of

low magnifying power of from 30 to 40 diameters illuminated from above, the small branches of the arteries are clearly seen to resolve themselves into a pencil of capillary vessels, and then to reform as a small vein. Or a small vessel, solitary in its flexuous course, or in the act of subdividing, without conversion into capillaries, from artery becomes vein. Laying aside the microscope, with a simple lens magnifying ten or twelve diameters, the same observation may be frequently made on the surface of the gland, the change being effected in those small vessels which are scarcely beyond the power of the eye, and which become visible under the smallest magnifying power of a lens." (pp. 5-6.)

After an account of the normal innervation of the gland, the following observation occurs :

"I have examined the nervous system in different cases of bronchocele; the ramifications upon the surface of the tumour appear larger than usual, sometimes very long indeed, divided, and as if dissolved (*decomposée*), so that their filaments appear to confound themselves with those of the cellular tissue of the external tunic. But with regard to the mass, it has not appeared to me, that the thyroid nerves had followed the development of the other parts, especially that of the sanguineous system. Perhaps in some very large goitres, these nerves admit a suspicion of increase; but if real, it is scarcely calculable when comparison is made with the nerves of the healthy organ." (p. 10.)

The author's microscopical account of the structure of the gland differs materially from that of other anatomists who have recently written on the subject; we, therefore, extract it, premising that the observations were made with the great microscope of Plössel, the degrees of the micrometer being regulated by lines of Vienna :

"The parenchyma of the thyroid gland in the state of health consists of two anatomical elements—the common cellular tissue and the glandular acini. From the internal surface of the common cellular envelope, innumerable partitions are prolonged, which subdivide the body of every lobe into other smaller masses, and then again into smaller still, up to the acini, which are also enveloped. The quantity of the cellular tissue, or as some modern anatomists say, of the *stroma*, is very great in this organ, and is diffused over all its parts, as may be proved by dissection; it forms true strata of a certain density, which pass in every direction, and conduct vessels and nerves; but according to the age and the individual, the cellular tissue is more or less abundant, lax or tense, so that the appearance of the gland is in some degree modified. The second anatomical element of this gland is the aciniform tissue. Its parenchyma consists of numerous fleshy bodies of a yellow or deep red colour, various in form and size, soft, elastic, opaque, vascular, and enveloped, as I already said, in the filamentous network of the stroma. These fleshy bodies are of all forms, and are divided into smaller bodies, which continue decreasing to a size scarcely recognisable with the assistance of a lens. Thus the smallest lobes discernible by the naked eye, are merely agglomerations of those smallest bodies which we call acini, and which appear as germs, or the true elementary tissue, *sui generis*, characteristic of the organ; while the stroma, or the cellular tissue that receives and envelopes the acini, appears as their matrix. Many anatomists have represented the thyroid acini in a natural state as bullæ, and have said that the organ was composed of a mass of small bullæ or vesicles, visible to the naked eye, full of serosity, and connected to each other by the cellular tissue and vessels. It is certainly very easy to make the acini of the gland hydatidigenous or vesicular; but in the healthy state, at every age, commencing from the new-born infant, when examined with a lens or microscope, of small magnifying power, of thirty to forty, they appear as opaque bodies, lustrous and soft, without any vesicular appearance.

"When a coloured fluid is injected, as the chromate of potass and the neutral acetate of lead, by the thyroid arteries or veins, the envelope of the gland, the



interstitial cellular tissue and the acini are tinged with the colouring matter injected, so that beyond the vascular network visible to the naked eye appears what might be thought an extravasation colouring the tissue. If, now, thin slices of this tissue, formed of the entire parenchyma, are cut away by a razor, spread upon a lamina of glass, and exposed to a microscope magnifying from 36 to 100, we find that the stain is not an extravasation, but a net or vortex of small vessels, injected in numerous strata one upon the other, with a clear and uninterrupted outline, so that the stroma and the fleshy substance of the acini appears to consist solely of small blood-vessels.

“To understand the microscopic texture of the thyroid gland, an acinus is found and isolated; or, a lobe of the gland being separated, a very thin slice of parenchyma is cut off by a sharp razor. This generally contains several acini, and it is exposed to a magnifying power of 200 or 300 under a compressor. The acinus thus rendered semitransparent, presents at the periphery a stratum of fibres interlaced in various directions, which appertain to the enveloping stroma or cellular tissue; and when the slice contains several acini, the bundles of fibres or the interlaced filaments are also seen in the interior of the slice. The substance of the acinus is seen to be composed of a stratum of granules, some of which are small simple nuclei, rounded, white and opaque; others are true bullæ or vesicles [or rather cells], .003 to .005 lines in diameter, round or oval, sometimes angular, with one or more central cytoblasts or nuclei. When an acinus has been divided with the forceps, torn or simply agitated in water, a number of perfectly isolated vesicles [cells] are seen floating in it; all round the periphery are small masses of granules, which have been exposed or separated by the manipulation. In the fœtus the vesicles [cells] appear larger, and many are destitute of the central nucleus, which is almost always observed at other ages. Thus the microscopic texture of the thyroid acinus is very simple, and easily made out: it consists of an aggregation of granules and nucleated vesicles enclosed in the interlaced fibres of the interstitial stroma.” (pp. 13-15.)

It is certainly not a little surprising to us, to find that Professor Porta has not succeeded in making out what most recent microscopists have discerned without difficulty; viz., that each acinus is, in fact, a vesicle or closed follicle, and that the nuclei and cells of which he considers it as made up, really constitute the epithelium of this vesicle. The following description, given by Dr. Handfield Jones, the most recent writer on the minute anatomy of the thyroid gland, is quite in accordance with our own observations:

“A thin slice of the thyroid, examined under a low power of the microscope, displays its condition very perfectly and readily. It is seen to be made up of closed vesicles, aggregated together in groups of various sizes by the fibrous expansions just described. The form of these vesicles is primarily spherical; but many, perhaps the majority, are more or less affected by mutual pressure, being triangular, elongated, ovoid, or oblong. They are all perfectly closed, the wall being formed by a homogeneous limiting membrane, which is easily traced all round, and can never be seen passing off into a neck, or blending with the envelope of an adjacent vesicle. Where a number of vesicles lie closely crowded together, the homogeneous envelopes are of course in contact, or separated only by the interjacent vascular plexus; but those forming the surface of a group are invested by a thin expansion of fibrous tissue derived from the general capsule. The diameter of the vesicles of the human thyroid I have found to range from 1-2000th of an inch to 1-85th; in the bullock, from 1-2000th of an inch to 1-48th, the greater number averaging about 1-60th of an inch in this animal as well as in the pig. . . . The vesicles are lined internally by an epithelial stratum, consisting usually of nuclei set together in a scanty basis substance, which is either feebly granular, or of a somewhat oily aspect. The nuclei are at once recognised by the practised



eye, as exactly resembling those of the true glands. Their nucleoli are not always visible, and vary very much in number—from one to four or five. The nuclei are, however, always vesicular, bounded by a strongly marked envelope, and have a mean diameter of 1-3000th of an inch. It has been observed by Mr. Simon, and I have occasionally had the opportunity of confirming the remark, that the nuclei, instead of remaining in their primitive condition, proceed to the further stage of cell-development; this he has noticed both in man and the lower animals.” (p. 1105.)

Dr. Handfield Jones informs us, further on, that he has seen in a human thyroid some large oval or circular corpuscles, about 1-1000th of an inch in diameter, consisting of coarse granular matter not surrounded by any distinct envelope, and of an opaque dead white colour. These were, perhaps, abnormal formations; yet in a tortoise, where the gland was quite healthy, similar corpuscles, and more numerous, were observed. We can scarcely imagine, however, that these could be ever sufficiently numerous to have misled Professor Porta into the belief, that the ordinary collections of cells, known as the acini, are destitute of the vesicular envelope; and can only suppose, that although he used an excellent instrument, and is a most dexterous “naked-eye anatomist,” he is not sufficiently practised in the use of the microscope, to be able to recognise what others more familiar with the appearances seen through it find no difficulty in satisfactorily discerning. The large size of the objects, and the low magnifying power under which their vesicular envelope can be seen, should be quite sufficient to negative any suspicion that its presence is rather hypothetical than real.

To complete this notice of the normal anatomy of the thyroid gland, we shall add Dr. Handfield Jones’s account, derived from the analytical observations of Mr. Beale, of the composition of its fluid contents. It appears that about 28 or 29 per cent. of solid matter exists in the secretion; and that nearly the whole of this is an albuminous compound. It is not, however, in the condition of ordinary albumen; for it is not coagulated by the re-agents which throw down that substance, neither acids nor alkalies producing any obvious effect upon the transparency of the fluid, save that strong nitric acid, after a time, colours this material bright green, or yellowish-green, and disengages a great many bubbles of gas. A small quantity of gelatine was found in the thyroid secretion of the ox, but not in that of the human subject. Crystals of triple phosphate and of oxalate of lime occur in the cavities; but no urea nor lithic acid, nor, in fact, any special organic compound can be detected.

*Thyroiditis.* The most important part of the chapter on inflammation of the gland is the account of the appearances observed in fatal cases:—Tumidity of the lobes, injection of the capillaries, turgescence of the veins, thickening of the envelope, and a copious exudation of plastic lymph upon and under the envelope and amid the interstitial cellular tissue. In more advanced cases, the surface of the organ is covered with purulent matter, the sterno-hyoid and sterno-thyroid muscles being also inflamed:

“I have more than once found the veins issuing from the gland, and passing its inferior margin, implicated in the inflammation and full of pus, or obstructed by a clot. In one case the coagulum completely obliterated all the principal venous trunks; even the lymphatic vessels, in cases of thyroiditis, often appear inflamed, varicose, tinged with rosy lymph, or puriform matter, and with opaque parietes. The glands to which they pass are swollen, red, sanguineous, and buried in masses

of plastic lymph. The ramifications of the thyroid nerves are interlaced on the surface of the injected capillary vessels." (p. 22.)

After showing how hypertrophy may be produced by lymph effused during a mere chronic attack of inflammation, the process of suffocation is described, both in a simple case of thyroiditis, and in cases of large bronchocele which suppurate. Professor Porta does not believe that, when symptoms of suffocation are produced by an enlarged thyroid gland, these are the result of mere mechanical pressure; but affirms that they proceed from an extension of inflammation to the air passages. He says:

"I have dissected some of these bronchoceles, in which the glandular tumour was full of pus, with a coating several lines in thickness, which rendered fluctuation null, or uncertain. The air passages, which are believed to be mechanically compressed from the symptoms of suffocation during life, offered all the appearances of chronic inflammation. The lining membrane of the larynx and trachea appeared much thickened; the epiglottis gigantic; the vocal chords thickened and hard; the *rima glottidis* contracted, and exudations of plastic lymph obstructed the canal." (p. 25.)

In the treatment the author places his principal reliance on blood-letting from the veins of the neck; either the varicose veins which extend over the surface of the tumour, or the jugular.

*Hygroma* or *Hydroma*, called by some Hydrocele of the neck, is a new cyst in the gland filled with fluid. Though less frequent than bronchocele, it is not a rare disease. Sometimes it follows an attack of thyroiditis. It is most common in youth, and in females. It varies in size from that of an egg to that of the head of an adult, and in every respect resembles the serous cysts of the liver, spleen, kidneys, &c. In the thyroid gland it commences as a small cyst, perfectly closed, consisting of two coats. The external is cellular and vascular; the internal very fine, and full of limpid serum. Such cysts may be formed in the interstitial cellular tissues of the gland, or arise from hydatid or vesicular degeneration of the acini. They are so common, that many anatomists have considered that the parenchyma of the gland consisted naturally of vesicles filled with the peculiar secretion of this organ. Still it is comparatively rare for them to increase to such a size as to form an evident tumour in the neck.

Palliative treatment by puncture is of little use, as the liquid reforms very quickly. Some very large tumours of this nature have been emptied, and found as large as before after a very few days. Iodine, even in the first stages, is generally inefficacious; but as it sometimes succeeds, though very rarely, it may be tried by inunction. Compression after puncture is generally useless. Caustic is dangerous, and produces deforming cicatrices. Subcutaneous incision is as useless as simple puncture. It might be worth while to try iodine injection in very small tumours of this kind; but in large ones one could scarcely hope for success, and reaction would probably occur to a dangerous extent. An incision, from half an inch to an inch in length, made in the lower part of the tumour, empties the sac, suppuration follows, the sac gradually closes, and the fistulous opening heals, leaving but a very small cicatrix. When the tumour is of moderate size, this is the safest and best treatment; but in larger cases the seton must be used, in the largest tumours two setons being applied crossways. This may be combined with a dependent incision, to give free issue to the pus which forms. In some cases the

author has made an incision through the skin, fascia, and envelope of the gland, down to the cyst, which he has then carefully dissected out. As the principal vessels run along the surface of the gland, they may be avoided, and, with care, no hæmorrhage of importance results. Any of these operations may be followed by thyroiditis, which may extend to the air passages. Indeed, this may occur after simple puncture. But, as a general rule, incision, the seton, or excision of the sac, when cautiously performed, and when inflammation is promptly treated, are safe, and in a large proportion of cases lead to a perfect cure.

*Hypertrophy of the thyroid gland* is not so common as is generally believed. In the bodies of 100 adults that presented evident signs of bronchocele, the author only found fourteen in which the enlargement was owing to true hypertrophy; less than a sixth of the whole number. But in some cases of very large bronchocele, in which the tumour equalled in size the head of a boy, it consisted of simple hypertrophy of the gland.

“In the natural state the thyroid gland is composed of lobes subdivided and ramified into other smaller ones as far as the acini, which are held together by an intermediate cellular tissue, soft, elastic, and transparent. Now, in advanced cases of hypertrophy, this tissue disappears, and, by an aberration of the nutritive process, the parenchyma of the organ is converted into rosy, soft, fragile, homogenous, vascular texture, which, under a magnifying power of 200 to 300, appears as an aggregate of simple nucleated vesicles and of granules. The cohesive force of this texture is so small, that on scarcely dividing it by a probe, or agitating it in a capsule of water, the vesicles separate in great numbers, and are seen freely floating in the liquid. These vesicles are generally oval in form, from  $\cdot 004$  to  $\cdot 006$  lines, with one to three or four central nuclei. Mixed with the vesicles are a number of white opaque granules, from  $\cdot 001$  to  $\cdot 002$  lines.” (p. 57.)

It is not stated whether their measurements refer to diameter or circumference.

In some cases, masses of plastic lymph are found in different parts of the tumour. In others, a new fibro-cellular tissue has replaced the primitive parenchyma. It is only in infancy, in the first stages, or in some exceptional case, that an hypertrophied thyroid gland retains the natural texture of the organ. The thyroid arteries follow the increase of the gland, in some cases to an exorbitant degree, pulsating with great force; and the superior produce a murmur which resembles that of a varicose aneurism, and leads to the suspicion of the existence of aneurism. Professor Porta has dissected such cases, and found that the arteries and their branches were really enlarged and tortuous, but that the gland in no way resembled an aneurism. He believes that what was called aneurismatic bronchocele by older writers, was merely an example of this enlarged condition of the arteries.

*Simple organic productions* of the thyroid gland are present in at least 80 out of every 100 cases of bronchocele. They are not, in general, simple aberrations of the nutritive process, but are new bodies or products formed in the interstitial cellular tissue. The principal are cysts and parenchymatous tumours of various characters. Besides the cysts which form hygroma, atheromatous, steatomatous, hematoid and melanoid cysts are found in the gland. The cysts are formed of two membranes,—an outer, cellular, of various density; an inner, resembling the

serous membranes. Pure cholesterine is often found among the contents. Gelatinous matter, with small calcareous concretions, is also common. Hematoid and melanoid cysts are almost peculiar to the thyroid gland. They differ from the serous cysts in colour and contents. Externally they appear red, purple, or black. The outer coat is very vascular. The inner sometimes very smooth and delicate ; at others dense, opaque, resistant, and of irregular surface. The contents are a red serous fluid, a fluid more dense, resembling venous blood, or soft black grumous masses. At times, also, a dense atheromatous matter, tinged with blood, is found. The blood in these cases may be secreted, but more probably it arises from a rupture of small vessels during difficult respiration, or under some mechanical pressure or friction in the tumour. When the thyroid arteries are injected by any moderately fine substance, under a very slight pressure it returns by the veins and fills them, but in its passage it becomes almost always extravasated in the tumours or cysts the gland may contain, so great is the delicacy of the minute vessels. The black, or brown, dense, glutinous, inodorous, secretion of the melanoid cysts consists of water, fatty matter, cholesterine crystallized, and a brown resinous colouring matter. Under the microscope it appears to consist of nucleated vesicles, and small irregular membranaceous débris. The brown vesicles have a large whitish nucleus in the centre. In addition to the ordinary contents of atheromatous and sebaceous cysts, a fatty unctuous substance, soluble in ether, and resembling the cerumen of the ears, is sometimes found ; and cartilaginous, osseous, and calcareous degenerations are far from uncommon.

Parenchymatous tumours of the thyroid gland have a peculiar texture of new formation. They have a common cellular envelope, sometimes very delicate and closely adherent, at others slightly adherent, thick, opaque, and laminated. The texture may be fleshy, cartilaginous, or osseous. The anatomical and microscopical characters of each of these classes of tumours are described at great length, but our limits do not allow us to follow the author.

*Scirrhus and fungous degenerations* of the thyroid gland are exceedingly rare ; but the author has met with cases in which the tumour presented all the characters of scirrhus of the mammary or salivary glands. He has also observed true hæmatoid and fungous degeneration. The chief difficulty in diagnosis is to distinguish enchondroma and osteoma from scirrhus, and a soft sarcoma from fungus. The history of the case, implication of other organs, and rapidity of progress, will, however, generally suffice. Fungous tumours, arising from the deep cellular tissue in the front of the neck, are not rare, and may be mistaken for fungoid disease of the thyroid gland, which is so very rare. Professor Porta has notes of 400 cases of fungus that he has observed in different parts of the body, and in only 4 of these cases did he observe fungoid degeneration of the thyroid gland. True scirrhus is also rare ; and it is a curious fact, that many patients who die of malignant diseases of other organs, present at the same time evident traces of bronchocele, and yet this tumour is formed by some of the simple organic alterations before described, the gland seldom or never participating on the malignant disease.

The TREATMENT of *simple organic productions* of the thyroid gland, is considered under two heads, medical and surgical.

1. *Medical treatment.*—The first indication is to ascertain the cause, and, if possible, to avoid or remove it by change of air, and other means. The second is to attempt to resolve the tumour; and the attempt, being innocent, may be made in almost all cases. As a local application, the author prefers the protiodide of mercury to other preparations of iodine. Four or five grains, with a scruple of lard, are rubbed in at each friction; or a saturated solution of iodide of potassium in water mixed with lard may be used. From ten to twenty of these frictions will be quite sufficient to judge of the effect of the application. The absorption of this ointment by the cuticle is perfect. The author, by simply rubbing a small quantity on the fingers, has found traces of iodine in his urine from fifteen to eighteen days afterwards. Iodine is also easily detected in the urine of patients taking burnt sponge. Professor Porta formerly used this internally, but latterly he has given the iodide of potassium, commencing with eight grains daily, and increasing the dose to a scruple, half a drachm, and even a drachm daily. It is borne with great ease, excites diuresis, and may be continued for several weeks. The tincture of iodine may be often harmless; but as it irritates the stomach and causes emaciation in many persons, the iodide of potassium is to be preferred, especially as iodine, when given internally, is converted into an iodide of sodium. On inhaling the fumes of iodine from a bladder, evident traces of iodine are found in the urine passed from six to twelve minutes afterwards, and in half an hour the precipitate is copious.

“After inhaling the vapour of iodine for one minute, not so much as a grain of the metal being lost; my urine has given forth the odour of iodine for four or five days. After a single dose taken into the stomach, every appearance is lost after five or six hours. When a large dose is given at once, from 5 grains to a drachm of the iodide, or 50 or 60 drops of the tincture, or when the medicine is continued for several days, the urine generally, after suspension of the remedy, continues to give a violet precipitate, with starch and nitric acid, from four or five to eight or ten days. The minimum duration is three or four hours; the maximum ten, and in some cases twelve days. But sometimes even a small dose of some grains of the iodide continues to show itself in the urine for several days. I have several times taken a single grain of iodide in one or two ounces of water, and in a quarter of an hour I could procure a copious violet deposit from the urine, which was the case for five or six hours, as if I had taken a large dose. By the endermic method, or by inunction, the iodine does not appear in the urine for fifteen or twenty hours, or until the next day, and sometimes is not found. Indeed, we never find a precipitate so copious and so deeply coloured, as when the remedy, even in small doses, is introduced into the stomach, or into the cavities of serous membranes. But its presence in the urine is for a longer period. By inhalation the passage to the urine is made more quickly, but the precipitate obtained from the urine is slight. We may say, then, that the passage from the skin to the urine is slower and more sparing, but the resolvent effect on the tumour is more prompt and decided, probably because the iodine remains longer in the body of the patient.” (pp. 116-17.)

The author also made a number of experiments in order to determine if remedies applied by the natural channels were found in diseased parts of the body. He used soluble salts of various metals, as iodide of potassium, chloride of barium, sulphate of iron, acetate of lead, and tartrate of antimony, upon a series of patients affected with ascites, hydrocele, hygroma, wounds, fistulæ, abscesses, &c., administering the salts externally and internally, and then collecting the various fluids for analysis by Dr. de



Cattanei, Professor of Chemistry in the University. The result was indisputable proof of the material presence of the remedy administered, in the diseased parts.

The result of the administration of iodine in the treatment of bronchocele, may be total resolution of the tumour, partial resolution, or absolute inutility. Of 100 cases thus treated by the author, in one half, the tumour remained totally or in part, and in several of the other cases, the good effect was but temporary. This may be partly owing to the habits of the patient, his profession, or residence, being the same that led to the first increase of the gland. Thus, the resolvent treatment with the removal of the causes of the disease, is both useful and safe, but only effects a radical cure in a limited number of cases, and these the slightest.

*Surgical treatment*, or operations to reduce or remove a bronchocele, are only required when the tumour, by its volume or relations, endangers the life of the patient. Professor Porta has frequently found large bronchocele in subjects in the dissecting-room, the inferior part entering the mediastinum, and hermetically closing the superior aperture of the thorax, pressing the bronchi and œsophagus against the vertebral column, and exciting severe dyspnœa and dysphagia. In a tumour of this nature which he recently examined, he found it pressing on the deep jugular vein under the first rib, closing this vein and the vena cava even to the right auricle. The patient had extraordinary dilatation of the veins of the neck, with great dyspnœa and continual somnolence. Even small tumours often produce serious symptoms, as dyspnœa, dysphagia, cough, aphonia, and threatened suffocation.

The principal operative measures are cauterization, the seton, ligature of the thyroid arteries, and extirpation.

*Cauterization*, either with the iron or caustics, is the worst of these methods. It is uncertain and dangerous, very harassing to the patient, and may leave deforming cicatrices. The *seton* is, also, unsatisfactory. Even when two are inserted crucially, and covered with escharotic powders, the suppuration is limited to the course of the threads, and after a discharge of weeks, the volume of the tumour is but little diminished. The cicatrices are deep and adherent,—the danger of hæmorrhage is also considerable. *Ligature of the thyroid arteries* has been performed in twenty-five recorded cases. Some of the patients died of gangrene, hæmorrhage, or thoracic inflammation. Of the remainder, the failures and success are about equal, but in the records of success, it is difficult to discover to what degree the tumour was reduced. In some, the reduction was only temporary. Indeed, the anastomosis between the thyroid arteries is so free, that it can only be in some exceptional case that the ligature of one could materially affect the nutrition of the gland. The author has twice tied the right inferior thyroid, on account of very large hypertrophy of the corresponding lobe of the gland, but the tumour was in no degree affected by the ligature of the artery, and its condition remained perfectly unchanged. One of the patients died three months after the operation, when the trunk of the tied artery was found pervious but a few lines from the ligature, on account of the very free anastomosis with the inferior thyroid of the same size, which was considerably enlarged. Having seen the preparation of these arteries in the museum of Professor Porta, we may add our testimony to the accuracy of the statement. The right inferior thyroid was injected,



and from this the whole of the branches of the superior were as perfectly filled, as if the trunk had been opened and filled from its origin. The left lobe of the gland was atrophied, and its two arteries very small, not appearing to have contributed to the nutrition of the right side of the gland. In another case, the author tied both superior thyroids. The operation was followed by notable diminution in the size of the tumour after a few days. In a month it was reduced by a third of its original volume. It then remained stationary for four months, but when last seen appeared disposed to increase. It is quite evident, then, that it would be necessary to tie both arteries on the side corresponding to the tumour. But in very large tumours descending beneath the sternum, the inferior thyroid is so deep, that no one would think of attempting to tie it. These are not the cases, however, to which ligature of the arteries is adapted. It is in cases where the tumour is of medium size, but still produces injurious effects and deformity, that this operation is indicated. The superior should be tied near its origin, between the external carotid and the summit of the enlarged lobe of the gland. But in order to find the inferior easily, it is not advisable to search for its trunk at the root of the neck, but to tie it close to its insertion at the base of the tumour. It generally enters at the posterior and inferior part of the tumour, and is easily discovered by raising the outer part of the base of the lobe. Professor Porta has not yet tried the effect of ligature of this artery. It must be remembered that the recurrent nerve runs very close to the inferior thyroid, sometimes before, generally beneath. An interesting case is related, in which the relation of the arteries was so much altered by the size of the tumour, that the operator, in endeavouring to tie the superior thyroid, tied the internal carotid instead. The patient died the same day.

*Extirpation* of bronchocele is frequently spoken of by the older writers on surgery; but they confounded all sorts of cervical tumours with enlargement of the thyroid gland, and only appear to have removed such as are external to it, or on its surface. Still, since the last century, eighteen or twenty cases have been recorded of undoubted extirpation of this gland, the result in about half being successful. But it must be remarked, that unsuccessful cases are often kept in the dark, and, therefore, that the chances of failure must be very great. Even the extirpation of one lobe of the gland is a severe and disastrous operation. It has been performed four times by Professor Porta, in every case with a fatal result. Without regard to hæmorrhage, the danger of the operation arises from the great size of the wound, laceration of surrounding cellular tissue, denudation of the vessels and nerves of the neck, and the larynx and trachea, and diffusive suppuration. Or acute inflammation of the air passages with the lungs and pleura, apoplexy, or nervous exhaustion, destroy the patient even more suddenly. Extirpation of the whole, or of a large portion of the thyroid gland, is an operation which ought to be discarded from surgical practice; but small isolated tumours of the gland may be easily and safely removed. It is not uncommon to find small accessory lobes, or isolated appendices, which are united to the gland by a narrow neck, or by the cellular envelope only. These separated lobes may increase and form tumours, while the gland itself retains its normal size. The tumour might be considered as external to

the gland, but it has the same texture, and the same envelope, is united to it by a peduncle, and receives branches from one or both the thyroid arteries. Professor Porta has repeatedly excised such tumours with perfect safety and success, a simple linear cicatrix remaining. In some cases no ill effect whatever followed, in others inflammation of the air passages came on, but all eventually recovered.

*Method of the author.*—Convinced of the difficulty and danger of isolating the thyroid gland from the parts surrounding it, Professor Porta has followed a plan which he strongly recommends as safe and efficacious. But we must now let him speak for himself:—

“In studying the thyroid gland, my attention has been directed to two facts which have especially interested me. First, that the arteries are inserted at the extremities of each lobe, and that their trunks run toward the superior, inferior, and external margins of the lobes; also that the large branches before penetrating the parenchyma, generally subdivide into a digitation of smaller branches. Thus, although the thyroids are *externally* four considerable arteries, *within* the tissue of the gland, they are merely branches and anastomoses of small caliber, and the embarrassment and danger of severe hæmorrhage from injury to large vessels, are scarcely met with when we isolate the external portion of the tumour. In the second place, that the greater number of bronchoceles, as I have fully explained before, do not arise from degeneration of the proper glandular tissue, but from the generation of one or more non-malignant sarcomatous or encysted tumours in the midst of the parenchyma of the gland itself; that these tumours increasing, at last invade the whole space, and reduce the organ to a simple envelope or matrix, on dividing which the new products are exposed, and may be extracted with the greatest ease, injuring only small vessels, and leaving behind a fleshy sac, which, when so evacuated, falls together, and no trace of the tumour remains. . . . . The result of these observations was the following reflection:—if the extirpation of the thyroid gland, and even of one lobe of it, is so difficult and dangerous, would it not be better to invert the plan of operation, and evacuate the contents of the tumour, without touching its external portion, without lacerating the surrounding cellular tissue, without injuring the arterial trunks, and without dividing the principal organs of the neck? On these grounds the new plan of operation consists in the simple incision of the external envelopes, and of the anterior surface of the tumour, at some distance from the direction of the arterial trunks; in the evacuation of the tumour by means of separation and extraction, or by excision of the cysts and nodules contained in it, without separation of the outer surface.” (pp. 147-8.)

We are not told in how many cases the author has performed his operation, but he has selected six as illustrations, stating that in these, and in all his other cases, it succeeded perfectly in removing the tumour, and curing the patient. In only two cases did inflammatory symptoms require prompt antiphlogistic treatment. In one case a coriaceous cyst, and two sarcomatous tumours were removed from the substance of the gland. In another, the gland did not contain new products, but the enlargement was owing to simple hypertrophy. In this patient, the whole fleshy internal part of the tumour was cut away, leaving a sort of surrounding bark, about two lines in thickness. The bleeding was very slight. Cysts and sarcomatous tumours were extracted in all the other cases recorded.

The instruments for this operation are merely scalpels, blunt-hooks, dissection and torsion forceps, scissors, &c. There are three steps in the proceeding. First: division of the integuments of the neck and of the second belly of the omo-hyoid muscle, which is almost always necessary.

Secondly: incision of the tumour parallel to the external incision, avoiding the branches of the thyroid arteries, or on division at once applying the ligature or torsion, which is very easy, because the branches are mere secondary ones, running along and adhering for some distance to the surface of the tumour. Thirdly, by means of the forceps, and the back or handle of the knife, cysts or tumours are removed. When very deep and adherent, they are excised at the base. In case of simple hypertrophy, the whole internal texture of the gland may be removed. Should any arteries be injured in this last step of the operation, torsion should be employed.

Sometimes the wound heals by the first intention, but more frequently it suppurates, and cicatrizes in a few weeks. Occasionally it becomes sacculated, and requires dilatation, or becomes converted into a fistula, which does not close for a considerable time. The great object of after-treatment is to moderate inflammation which may arise, adopting active antiphlogistic measures, should its extension to the chest or head be threatened.

We think this operation well worthy of adoption by British surgeons. Not only is the plan a legitimate deduction from observation of the pathological anatomy of the thyroid gland, and the anatomical disposition of its vessels, but it is accompanied by abundant proof of its safety and success. The author has, as we believe, conferred a great benefit upon the profession and mankind by his labours, and we think no one who has read the foregoing pages with any attention, will deny that he has honorably fulfilled the hope expressed in his Preface—that he has advanced the science and art of surgery.

#### ART. VII.

1. *Grundlage der Literatur der Pädiatrik, enthaltend die Monographien über die Kinderkrankheiten.* Von FRIEDRICH LUDWIG MEISSNER, Doctor der Medecin, &c. &c.—Leipzig, 1850.

*A Basis for the Literature of Pædiatrics, embracing the Monographs on the Diseases of Children.* By FREDERICK LEWIS MEISSNER, M.D., &c.—Leipsic, 1850. Royal 8vo, pp. 246.

2. *Die Krankheiten der Neugeborenen und Säuglinge vom clinischen und pathologisch-anatomischen Standpunkte bearbeitet.* Von ALOIS BEDNAR, Doctor der Medecin prov. Primararzte des k. k. Findelhauses in Wien, &c. &c. *Krankheiten des Ernährungs-Kanals bei Neugeborenen und Säuglinge.* Erster Theil.

*The Diseases of Newborn Children, and of Children at the Breast, viewed in relation to Clinical Medicine and Pathological Anatomy.* By ALOIS BEDNAR, M.D., &c.—*Diseases of the Alimentary Canal.* Part I.—8vo, pp. 131.

To all of our brethren who take any interest in the science and art of medicine, beyond that which is immediately connected with the daily visitation of a certain number of patients, and which is felt to increase or diminish, *pari passu*, with the number visited, or its remunerativeness, a ready means of ascertaining the nature and extent of the literary treasures of our profession would be a great gift. To such, it would become highly

important, as pointing out sources through which they might avail themselves of the experience of others, and thus support or correct their own ; and by which they might derive a knowledge of the labour already expended in elucidation of any particular subject, towards which they may deem it necessary that further inquiry should be directed, and upon which they themselves may be about to be engaged. We cannot help, therefore, looking forward with many anxious hopes to the promised presentation of a 'General Medical Bibliography,' by the Sydenham Society, to its members. Of the labour and difficulties attendant upon such a work, undertaken on a scale that shall meet the requisitions of the scientific practitioner of the present day, few will have any doubts ; whether the collection of material for its formation, or the methodical arrangement and careful production of such material, when collected, be considered. The latter will require the utmost judgment and care ; for, be the material ever so valuable and ably amassed, unless it can be easily referred to and made use of, it will be next to useless. Upon the best *arrangement* of the matter of a Medical Bibliography, much difference of opinion, no doubt, will exist ; and those of our readers who are acquainted with the discussions which took place, a short time ago, in the 'Athenæum,' upon a cognate subject,—viz., the New Catalogue of the Library of the British Museum,—will be able to perceive how many points must be considered in the endeavour to render useful a catalogue of books.

Dr. Meissner had in view, when he published the last edition of his work on the 'Diseases of Children,' the propriety of appending to it a concise retrospect of the literature of its subject. Second thoughts, however, led him to adopt his present plan of publishing a separate treatise upon it ; and, now that it has appeared, we feel much pleasure in bestowing our commendations, so far as the collection of materials goes. It is most rich, as might be expected, in the account of works published in Germany and France ; but our own literature, upon the *spécialité* with which the author is engaged, is well illustrated ; and that of Holland and of Italy is by no means forgotten. Many inaugural dissertations are included in the list of Monographs. The earliest work (at least which has struck our attention) alluded to, is that of Paul. de Flumine Bagellardes, 'De ægritudinibus et remediis infantum,' 1472 ; and books bearing the date of 1849 are also included in Dr. Meissner's 'Basis,' which embraces the subjects of Lactation, the Physical Education and Hygiène of Children, and Diseases of the Fœtus, as well as the diseases of extra-uterine life. It is particularly full in reference to works on Smallpox, Cowpox, Chickenpox, and the Varioloides generally, no less than seventy-eight pages being taken up with the account of them. To all engaged in the practice and study of the diseases of children, Dr. Meissner's work will be very acceptable ; but we are of opinion, that a better one might have been constructed out of the same material, if the author had adopted a different arrangement and method of printing, or had given a better index. The dates, we admit, are in a bold striking type, and the author's name is easily recognisable ; but yet we find drawbacks regarding the work as a whole. Limited in its scope as it is, these drawbacks are felt in a slight degree in comparison with what they would be if occurring in a general Bibliography on Medicine. With the latter, they would terribly diminish its facility of reference, and, therefore, its chief utility. Dr. Meissner first classes

together systematic works or general treatises. With this arrangement we coincide. Then follow, in a sort of nosological order, different diseases, with the books relating to them. Measles, Rubeola, and Scarlet Fever, follow each other very well; but why the latter should be followed by Chorea, whilst Acute and Chronic Hydrocephalus, and Inflammation of the Spinal Cord, stand between Gastro-enteritis and Croup, and why Rheumatism is placed between Fever and Worms, we cannot divine. This arrangement, then, we disapprove of. Still, we should have thought less of the inconvenience—we will not add inconsistency, for, after all, *convenience* here is the grand thing—if, by a ready index, we could have readily discovered where, in the body of the work, we were to look for the literature (say) of Rheumatism. The only index is a list, occupying barely two pages, of the subjects, or diseases, &c., under which the monographs are arranged; and these subjects follow the same order in the index, as in the body of the work. There is no separate list of authors, nor are there headings to the pages. As the subject we are now touching upon may be of considerable interest to some at the present moment, we will add a few words in illustration of the method which we think would offer considerable advantages as regards facility of reference. There can be no doubt but that it would entail more labour, expense, and space, than that of Dr. Meissner; but in the formation of a ‘Bibliography,’ it would be unwise to sacrifice any reasonable demands to that which must be inevitably felt as a great imperfection in regard to convenience.

Let our readers suppose that in the following they are looking upon the commencement of one of Dr. Meissner’s pages :

—134—

F. Baron, *An Inquiry, illustrating the Nature of Tuberculated Accretions of Serous Membranes, and the Origin of Tubercles and Tumours in different Textures of the Body.* London, 1819. 8vo. With Engravings.

J. Baron, *Illustrations of the Inquiry respecting Tuberculous Diseases.* London, 1822. 8vo.

The only reference to these works, or the authors of them, is “On Tuberculous Disease,” which stands between “Tinea Capitis” and “Rachitis, or the English Disease,” on the second page of the index. Our own method would be this : In a Bibliography there should be two great departments—1st, Of subjects; 2d, Of authors; and a good index, referring to both: for reference to such a work will generally be made for one or other, or both, of these purposes, viz., to know *what* has been written upon, or *who* has written upon, a particular disorder, &c., and to discover whether a particular person has written upon a particular disease, or upon what he has written. All detailed information should be given in the first department, and this information should follow in order of relative importance to the inquirer, and be printed in such a way as readily to attract the eye. The subjects should follow in alphabetical sequence, and the works in the order of their date of publication. Works on general medicine, or systematic treatises, should form the commencement, and should constitute a separate department of themselves. Our page of the first department will be thus illustrated, in opposition to that of Dr. Meissner :



Page 134.                      — *Tuberculosis (Blood, Scrofula)* —

No. 25	<i>An Inquiry, illustrating the Nature of Tuberculated Accretions of Serous Membranes, and the Origin of Tubercles and Tumours in different Textures of the Body.</i>	Baron, F.	1819	Lond.	8vo.	Illustrated.
„ 26	<i>Illustrations of the Inquiry respecting Tuberculous Diseases.</i>	Baron, J.	1822	Lond.	8vo.	—

The second department should consist of an alphabetical list of authors, giving the general name of the subject they have written upon, with reference to the place in which an account of their works is to be found in the first department, viz., in reference to the above.

Page 200.                      — *Baron to Broussais.* —

		Page.	No.
Baron, F. . . . .	<i>Tuberculosis</i>	134	25
Baron, J. . . . .	<i>Ditto</i>	134	26

There should follow an index, with a synoptical table of the words or terms employed, to indicate, in the alphabetical arrangement, the position of subjects or diseases, many of which have many synonymes, and which, therefore, must be given. Then should follow, alphabetically, these words or terms, and the authors' names ; with references to the former in the first department, and to the latter in the second.

Since the commencement of the year 1846, Dr. Bednar has been engaged in turning to account the experience to be derived from a connection with the large Foundling Hospital at Vienna ;—a sphere for clinical observation of no slight extent and importance, seeing that in the years 1846-47, no fewer than 14,477 children (7323 boys, 7154 girls), chiefly within the first fourteen days after birth, were admitted, of whom a considerable proportion may be considered as having demanded medical interference. Dr. Bednar's Treatise now before us, is intended to form the commencement of a 'Complete Pathology of the Infantile Organism;' the completion of which is promised, if the present portion meet with approbation from the public. We are willing to bestow our own praise upon it, in reference to all in it that relates to morbid anatomy ; but with regard to other matters, we find nothing calling for commendation ; on the contrary, we have now and then felt very great disappointment, the more so, as from the reference to clinical medicine on the title-page, we were led to expect much more upon what we are accustomed to include under this subject. The work does not deal with the views or experience of others, but is entirely based on the author's own observation. As many as 850 cases, extending over a period of two years, form the basis of the facts detailed in it ; and the author had at the beginning of his necroscopic researches, the valuable supervisional assistance of Drs. Lautner and Hampl. The following are the more important subjects touched upon by Dr. Bednar : Dyspepsia — Diarrhoea — Cholera — Softening, Anæmia, and Hyperæmia of the Intestinal Coats — and Hæmorrhage from the Alimentary Canal ; together with



Enlargement of the Glands and Suppuration of the Follicles of the Large Intestines—Croup and Aphthæ of the Alimentary Canal—Typhus—Tuberculosis of the Intestines—Lesions of their Continuity—Contraction and Closures of their Canal—Alterations of Position—and Formative Deficiencies. In a short introduction to the above, Dr. Meissner thus writes :

“The diseases of the alimentary canal include all abnormalities of the oral cavity, pharynx, œsophagus, stomach, and intestines. They in general constitute the primary disorders of the infantile organism ; being very rarely the consequences of another malady, as, *e.g.*, erythematous stomatitis a consequence of facial erysipelas, hæmorrhage from the stomach and bowels one of congestion of blood, &c. &c. On the contrary, they are the copious sources of the secondary affections of other organs, *e.g.*, of the lungs, of the brain and its coverings, as also of the blood. It is not always that an anatomically appreciable textural alteration, or diseased condition of the mucous membrane, are at their base ; but they are very frequently induced by a primary abnormal process of decomposition of the contents of the stomach and intestines, the probable existence of which we are led by analogy to believe in, and of which closer investigation assures to us the certainty. If by the absorption of purulent and other noxious fluids, the quality of the blood becomes changed without the vessels becoming in the slightest degree involved, why should not the contents of the alimentary canal, in consequence of the numerous prejudicial circumstances influencing them, undergo abnormal decomposition without the texture of that canal being primitively diseased ?” (p. 5.)

Accordingly, our author regards intestinal affections under two heads, viz., as dependent upon an unhealthy process of decomposition of the contents, and as based upon an anatomically appreciable abnormal condition of the structure of the intestines.

“Amongst the latter will be missed the catarrhal and dysenteric processes. Relative to the first, I have attained the conviction, that the diarrhœa of infants at the breast has not its origin in catarrh of the intestinal mucous membrane ; since such cannot be demonstrated, either anatomically, or by the microscope. Chronic catarrh of the mucous membrane of the intestine, described by writers under the terms of *muco-enteritis chronica*, and *marasmus*, may be more amply treated of under the tabes of children. Dysentery I have not as yet observed amongst the children of the Foundling Institution.” (p. 6.)

Some discussion has at various times taken place as to the cause of the colour of the green or “chopped spinach” stools of young children. Dr. Meissner draws our attention to the action of the *ley* employed in washing the napkins, in causing the acid evacuations of dyspepsia, which are often yellow when first passed, to become green in a short space of time. Before noticing the various complications of diarrhœa, the author alludes to the formation of fungi, which is combined with diarrhœa, attacking children within eight weeks old, especially when the cleansing of the cavity of the mouth is neglected, or when fluids capable of undergoing the fermentative action, like solutions of sugar, milk, &c., remain in contact with the mucous membrane. The layer of fungi—often pretty thick—either covers the mucous membrane of the cavity of the mouth only, or that of the pharynx or of the œsophagus, or extends in various degrees over all three at the same time. (p. 25.)

The chapter on the “Pathological Anatomy” of Diarrhœa, is an able epitome of the results of the author’s *post-mortem* investigations. It is impossible for us to compress it into smaller space than it occupies in the

original (16 pages); so we shall rest satisfied with the following extract from the author's *résumé*, premising, however, that we have wished Dr. Meissner could have persuaded himself to have written much shorter sentences than he now and then indulges in, since twenty-three lines in Viennese German, before one attains the luxury of a full stop, are occasionally perplexing; the more so when such lines are destitute of all parenthetical marks.

"If, following the indications of the preceding table, we place together the abnormal states which remain after rejecting general anæmia and tabes (consequences of diarrhoea), the textural diseases of the lungs, brain, meninges, and serous membranes (constituting the secondary diseases), enlargement of the solitary follicles, Peyer's patches and mesenteric glands (as frequently found in connection with other diseases), and the existence of pus in the umbilical vessels (the presence of which depends upon the age at which the child died), we shall find that hyperæmia of the brain and of its membranes, anæmia of the lungs and of the liver, and the viscid exudations of the serous membranes, constitute the anatomical results found as proper to diarrhoea. To these we must add the thickened tar-like viscid blood, the glutinous, colourless, or greenish or yellow coloured (often mixed with dark-brown flakes,) layer lining the stomach, scarcely to be distinguished from the underlying mucous coat of the latter, and extending to a less degree over the cavity of the mouth, the œsophagus, and the intestinal canal, which latter generally contains thin watery, yellow, or green coloured contents, and the dryness and firmness of the subcutaneous cellular and adipose tissues. The *muguet* of the œsophagus and of the cavity of the mouth (in one case of great neglect and uncleanness of a nurse-child, it was found in the nostrils and on the gastric mucous membrane), bears a like import to the ordinary fungous-formation taking place on the surface of a fermenting fluid exposed to the atmosphere. The softening of the mucous membrane is the result of the fermentation of the fluid in contact with the membrane, and which will be afterwards referred to." (p. 46.)

We pass over the subjects of Constipation, Vomiting, Colic, and *Muguet*, with the mere expression of our regret that their therapeutic relations are so meagerly attended to. The litigated question of *softening* (see our review of Dr. West's Work, vol. III, p. 429), is well treated. A table is given of 117 cases, in which softening either of the stomach, œsophagus, or intestines, or of the lungs, or hæmorrhagic erosion of the gastric mucous membrane, existed. The author arrives at the following conclusions :

"The so-called softening of the alimentary canal and lungs is a result of the fermentative action which occurs primarily in cholera, and secondarily in the other affections of children."

"Softening can invade the mucous membrane during life; perforation of all the coats occurs first after death."

"It is rarely an object of diagnosis, never one of therapeutics." (p. 81.)

Anæmia and hyperæmia of, and hæmorrhage from, the coats of the alimentary canal, next follow under consideration. The two former morbid states, Dr. Meissner considers, cannot be regarded as independent affections. Of hæmorrhage there occur two varieties, a primary form arising from hyperæmia of the gastric and intestinal mucous membrane, and a secondary induced either by some alteration in the quality of the blood, by its impeded circulation, or by loss of substance of the mucous lining. The author's investigations lead him to the opinion, that enlargement of the glands of the intestinal coats has no intimate relationship with any particular disease. But whether such a condition should constitute in

itself a substantive disease, or be regarded as a transitional state yet within the limits of a normal function, is a question not hitherto determinable. Suppuration of the solitary follicles of the larger bowel is a result of inflammation and consequent formation of pus; but whether this inflammatory action be of a catarrhal character, or not, is doubtful.—Dr. Meissner next treats of *croup of the alimentary canal*. Stomatitis, gastritis, and entero-colitis are included under this head.

“The croupose exudation, which is deposited on the surface of the mucous membrane, forms grayish-yellow (sometimes reddened by intermingled blood) membranaceous layers, occupying a portion of the alimentary canal in the form of long stripes, large flakes, or tubular pieces. It has the property of soon dissipating and regenerating itself many times upon the mucous surface, and at length of corroding the underlying tissue; to which action of corrosion the frequent discharges of blood accompanying croup are found to be due.

“Croup may occupy the mucous membrane either of the cavity of the mouth, of the stomach, or of the small intestines; or, as is most frequently the case in new-born children, that of the larger bowel.

“The surface of such an exudation is frequently the seat of an extensive fungous formation.” (p. 94.)

Two forms of stomatitis are described; viz., erythematous and croupose. With respect to gastritis, it is remarked, that it consists in a croupose exudation of the gastric mucous membrane, and in most cases is the consequence of pyæmia.

“In one instance it accompanied a deposition of pus in the capsules of almost all the joints; in another, its deposit in the Malpighian vesicles of the spleen; and in a third, purulent pleuritis, preceded by suppurative inflammation of the cellular tissue of the right cheek and adjacent portion of the neck, with descent of the purulent matter through the anterior mediastinum into the right pleural sac. In no instance was pneumonia absent.” (p. 96.)

#### In entero-colitis—

“The inflammation of the intestinal mucous membrane, which accompanies the exudation of a croupose matter, invades the large bowel or the small intestine, or both together, in different amounts of extension; most frequently it is limited to the larger bowel and to the lower portion of the smaller one.” (p. 97.)

Two forms are alluded to by the author; viz., a primary form, and one originating in pyæmia or sepsis of the blood. This pyæmia may be the effect of phlebitis of the umbilical vessels, or of the axillary ones accompanying gangrene in the armpit. Sepsis of the blood of new-born children is now of very great rarity in Vienna; and it appears that this city is indebted to Dr. Semelweis of the lying-in clinical department for such result, one for which he deserves no less amount of thanks, than for his able researches into the causes and prevention of the once very fatal puerperal fever there. We extract the following account of the morbid anatomy of the bowels in entero-colitis, as in some particulars it differs from the descriptions of other writers:

“The diseased portion of the intestinal tube is in general contracted, its contents are fecal matters, a gray turbid fluid, or the blood effused into its cavity. The mucous membrane, both of the small and large intestine, or of the latter alone, is mostly folded on itself longitudinally; it appears swollen and injected, and often in

the larger bowel is marked with a great number of dirty dark-red spots, from the size of groats to millet-seed. The mucous membrane of the latter is frequently coated with a thin, gray, reddish exudation, which occupies several large spots. In other cases a thick stratum of a yellowish, fibrous, firmly adherent exudation, whose surface is occupied by an abundant formation of fungi, covers the mucous membrane of the whole of the colon, sometimes also of the lower ileum, very rarely the latter alone. Besides this, the mucous membrane and the submucous cellular matter are abundantly corroded in star-like spots.

"In one of the secondary forms was observed, in many parts of the small intestine, a yellowish-gray firm infiltration, like an eschar, which occupied a quarter of an inch of the breadth of the intestine, and, in the oblique direction, the whole mucous membrane. At the same time extensive gangrene invaded the axilla." (p. 102.)

Two forms of aphthæ of the mouth and of the large intestine are alluded to. The author met with one decided case of typhus in a child at the breast, within fourteen days after birth. Its history is given in detail, along with two problematical typhus cases. All the three mothers of the children died of "metritis puerperalis." Tuberculosis of the intestinal canal is difficult to be treated of independent of tubercle in other organs. Only once has Dr. Meissner observed, in an unweaned child, tuberculosis of the mesenteric glands alone, along with general anæmia; when the intestinal canal was affected, it was so always in connection with the deposit elsewhere.

Under the head of "Interruptions of the Continuity of the Alimentary Canal," the following are alluded to as *acquired* in their origin:—1. Opening of the bowel in consequence of penetrating gangrene of the umbilicus. 2. Spontaneous rupture from softening of the intestinal coats. 3. Perforation of a portion of the alimentary canal as a result of the formation of pus: *a*, of the œsophagus, from suppurative inflammation of the cellular tissue of the left side of the neck; *b*, of the rectum, from the results of atresis. The following are *congenital*;—hare-lip, fissures of the palate and uvula, indentation of the tongue, fistula of the neck, of which latter peculiar malformation Dr. Meissner gives an interesting account.

Under "Contraction and Closure" are mentioned atresis of the rectum, narrowing of the opening of the valvula cæci, and closure of the ascending colon by an oblique muco-membranous partition.

Under "Deviations of Position," are included, as *acquired*, umbilical hernia, inguinal hernia, invagination of the intestine, internal hernia and prolapsus of the rectum; the following as *congenital*,—departure from the normal position of the ascending colon; irregular opening of the anus.

The "Formative deficiencies" include the absence of the ascending portion of the meso-colon, the spleen, &c.

We trust that it will not be long before we receive the remaining portions of Dr. Bednar's promised treatise, which we have no doubt will add materially to our knowledge of the morbid anatomy of the diseases of children.

## ART. VIII.

*The Anatomy, Physiology, and Pathology of the Eye.* By HENRY HOWARD, M.R.C.S.L., Surgeon to the Montreal Eye and Ear Institution. *Montreal and London*, 1850. 8vo, pp. 514.

MONTREAL is certainly not the place from which we should have looked for a comprehensive treatise on Ophthalmology, such as is indicated by the title of the work before us. To produce such a treatise successfully, we should have thought that an author, however large his field of practical experience might be, would feel the need of that familiarity with the general advance of knowledge on his subject, which cannot be derived from the mere reading of books and periodicals, and which can scarcely be acquired by any but those who live in the midst, or on the borders, of its current. We do not believe that an intelligent surgeon, in any part of the world, can be cut off from the power of making new and valuable observations; and of contributing, if so disposed, to the advance of his science and art, by the publication of them. But we are as far from thinking that a systematic treatise, which must necessarily be produced under circumstances of great disadvantage, is the appropriate vehicle for them. We had prepared ourselves to find our author offering some reasons for so undesirable a course, and stating the grounds upon which he had come forwards to instruct the public upon the science as well as the practice of Ophthalmology, with some apology for the probable imperfections and errors of his treatise, such as might fairly be attributable to his isolated position. Scarcely anything of this kind, however, do we meet with; the following extract from the preface, being the only passage in which he sets up any claim to a lenient judgment; and the conclusion of this, as our readers will see, being couched in a tone of pretension, which at once removes all ground for the exercise of any peculiar tenderness in the estimation of Mr. Howard's claims as a public instructor on Ophthalmology:

"On an inquiry," says our author, "which possesses so many attractions to the general Student, some feelings of diffidence might be fairly expressed by a writer when committing his thoughts to the criticism of the public; but the fears of the author rise into anxiety when he invites the attention of the Faculty to a work, which not only treats generally of the Organ of Vision, but professes to inquire critically into its Anatomy, Physiology, and Pathology." (p. iii, Preface.)

We shall presently show how little claim Mr. Howard's work has to being put together *critically*, by which is meant, we presume, in a manner exact, nicely judicious, and accurate;—a thing, on so complicated and important a subject as the eye, very much to be desired, but somewhat difficult of attainment.

That in regard to words, Mr. Howard is not particularly critical, will appear from his using *ciliæ* for *cilia* (p. 52), *malar duct* for *malar foramen* (p. 3), *casserian ganglion* for *Gasserian ganglion* (p. 15), *metallic acids* for *mineral acids* (p. 225), &c.

Nor is he more accurate in respect to authorities. Thus, he tells us, that "Mr. C. Bell recommended, that the cataract should be extracted through the sclerotica." (p. 441.) C. Bell never recommended anything

of the kind ; but Benjamin Bell suggested such an operation, and tried it on brutes, but never on the human subject.

Mr. Howard remarks, " that Sir C. Bell should suppose this nerve [the motor oculi] to be altogether under the influence of the will, is not much to be wondered at, as it does not appear that he was even aware that it gave a twig to the lenticular ganglion." (p. 45.) In John and Charles Bell's '*Anatomy of the Human Body*,' vol. iii, published in 1803, Charles says, " The nasal branch of the ophthalmic nerve sends off a slip, or twig, to form, with a branch of the third pair, the lenticular or ophthalmic ganglion." To accuse Sir C. Bell of such ignorance as not to know the anatomy of the lenticular ganglion, is about as far, we think, as impertinence could go.\*

Our author tells us, that " When a wound of the frontal nerve is the cause [of amaurosis], Mr. Guthrie recommended making an incision down to the bone in the direction of the wound, so as to divide the nerve." (p. 474.) To our view, what Mr. Guthrie says is more dissuasive than commendatory. " Of the efficacy of this," says he, " I am sorry I cannot offer testimony from my own practice, having failed in every case in which I tried it."†

That in respect to facts, Mr. Howard is not particularly critical, will appear from the following blunders :

" Through the foramen opticum," says he, " passes the optic nerve with its neurilemma, and the central artery of the retina." (p. 2.) No notice is taken of the ophthalmic artery, although a much more conspicuous object than its branch, the central artery of the retina.

" The malar duct," by which Mr. Howard means the malar foramen, " sometimes transmits small vessels and nerves into the orbit." (p. 3.) The malar foramen uniformly transmits the nervus subcutaneus malse, and a branch of the internal maxillary artery, out of the orbit.

From the lachrymal gland, Mr. Howard states, that there " proceed five or six small ducts ;" whereas, the number of the lachrymal ducts is known to be at least twelve.‡

Mr. Howard says, " The tendo oculi, or tendo palpebrarum, is a small muscle of about a quarter of an inch in length, inserted internally into the upper end of the nasal process of the upper maxillary bone ; it crosses the lachrymal sac a little above its centre." (p. 7.) The structure here described is no muscle, but merely the tendon of the orbicularis palpebrarum of some anatomists, or the internal palpebral ligament of others. It has, in itself, no title to be considered as a muscle.

At page 19, Mr. Howard states, that " The globe of the eye is nearly spherical, its antero-posterior axis being about two lines greater than the transverse axis ;" and at page 38, he says, " The globe of the eye is composed of the segments of two spheres of different diameters, of which the cornea is the segment of the less sphere ; in consequence of which, the antero-posterior diameter of the globe is much its larger diameter." The cornea being a segment of a smaller sphere than that of which the

\* Although Willis mentions the lenticular ganglion as a small round plexus, whence arise delicate filaments, which surround the optic nerve, Schacher, according to Valentin, was the first (*De Cataracta*, 1701, § 9) who gave a true description of the ganglion, and of its short and long root.

† Lectures on the Operative Surgery of the Eye, p. 114. London, 1830.

‡ Cyclopædia of Anatomy and Physiology, vol. III, p. 89. London, 1840.



sclerotica is a segment, is no reason why the antero-posterior diameter of the eye should be longer than the transverse. It merely causes that portion of the eyeball by which the two segments are connected, to be in curvature coincident with neither. Were the antero-posterior diameter much longer than the transverse, or even two lines longer, the figure of the eye would depart very considerably from the spherical. The fact is, that the two diameters in question are, during life, or soon after death, exactly equal.\* If the eye is taken from the body, and placed in water, this fluid, being imbibed by the open vessels, changes the figure of the eye in a remarkable manner, pushing forward the lens and the cornea, and rendering the antero-posterior diameter longer than the transverse.

Mr. Howard states (p. 21), that the conjunctiva of the cornea is separable from it only by maceration. The merest tyro in anatomy knows the facility with which the epithelium of the cornea (for epithelium is all that it possesses of conjunctiva), is raised by a touch of the scalpel, and detached by plunging the eye, for a few seconds, into boiling water.

Pappenheim has mentioned† three facts, which show, that what he describes as nerves of the cornea, really belong to that structure, and run between its proper laminæ. According to Mr. Howard (p. 21), the first of the three facts is, that "If the corneal conjunctiva be removed, the nervous filaments are on the inner, and not on the outer, surface of the corneal epithelium;" whereas, what Pappenheim says is, that "If the epithelium which covers the external surface of the cornea be removed, we see immediately that the nerves are on the inner, not on the outer, surface," viz., of the cornea proper.

"The second coat of the choroid," says Mr. Howard, "is called after its discoverer, the *membrana Ruyschiana*. It can be separated from the outer layer by dissection." (p. 23.) This statement is incorrect. No dissection can separate the proper vascular choroid coat into laminæ. We may tear away shreds of the lamina fusca from the external surface of the choroid, while the pigmentary epithelium separates easily from its internal surface; but the proper choroid cannot be split into layers. When it is fully injected, we see, no doubt, that its outer surface is composed of arterial and venous branches, and its inner surface of capillaries. The close-meshed plexus of capillaries constitutes what is called the tunica Ruyschiana, but does not admit of separation by dissection.

Speaking of the iris, Mr. Howard (p. 26) states, that its muscular fibres may be "demonstrated, on its posterior surface, large enough to be raised with a needle or bristle." We believe this to be entirely a mistake. To reach the fibrous or muscular substance of the iris on its posterior surface, the uvea requires to be removed. The fibres found in the proper substance of the iris, and upon which its motions probably depend, appear, even under high magnifying powers, like very fine hairs, destitute of the transverse markings which characterise striped muscular fibres, and yet by no means identical in appearance with ordinary unstriped muscular

\* "The diameters of the eye do not always bear the same proportion; sometimes the transverse diameter is the longest, in other eyes it is of the same length as the axis of vision; but when the coats are distended, the transverse diameter is diminished, and the axis of vision is lengthened. This change, however, does not take place at all ages, for at fifty it was not met with." (Phil. Trans. for 1796, vol. lxxxvi, p. 7.)

† Ammon's Monatschrift für Medicin, Augenhellkunde und Chirurgie, vol. ii, p. 282. Leipzig, 1839.

fibre. On the front of the iris there are radiating fibres, so coarse as to be capable of being sometimes raised in the way mentioned by Mr. Howard ; but, although certainly contractile, they appear very different in structure from muscular fibres.

Passing on to the retina, Mr. Howard describes it as consisting of three layers, the external of which, or membrane of Jacob, he calls the *serous layer*. Not a word does he say of the curious structure of this *stratum bacillatum* ; certainly, as Huschke styles it, “ the most remarkable of all the membranes of the eye.”

So much for Mr. Howard’s critical Anatomy of the eye.

It is universally admitted by physiologists, that the ordinary motions of the iris are of the kind called reflex ; and that they follow automatically from stimulation of the retina, which transmits the impressions made on it by light through the optic nerve to the brain, whence they are communicated to the third nerve, which supplies motor branches to the iris. The experiments of Fontana, Mayo, Magendie, and Flourens, by which this has been demonstrated, are well known. Those of Mayo, in particular, show, beyond the shadow of a doubt, that the photometric movements of the iris are regulated by the third nerve, through the medium of the short root of the lenticular ganglion, and that they are in no way influenced by the long root of this ganglion, derived from the nasal branch of the fifth.

Mr. Howard has adopted a different view of the matter, viz., that the motions of the iris depend, not upon impressions made upon the retina, but upon its own sensibility to light ; a notion which was maintained by the late Mr. Walker, of Manchester, in an ‘ Essay on the Physiology of the Iris,’ published in 1833, and which derives some slight semblance of support from certain experiments of Valentin\* and Brown-Sequard.† To none of these authorities, however, does Mr. Howard refer, but builds his hypothesis, in a great measure, on a pathological fact, which has long ago been satisfactorily explained ; namely, that in certain rare cases of total amaurosis, the pupil moves briskly, according to the degrees of light to which the eyes are exposed. In cases of this kind it has been ascertained, by dissection, that the causes which produced blindness resided far back in the encephalon, sometimes in the cerebellum, sometimes in the fourth ventricle, so as to affect the origin of the optic nerves, and thus to prevent the process of vision from being accomplished, while they left the tract, through which the influence of the optic nerves is communicated to the *motores oculorum*, altogether free from interference.‡

Such an untenable position as the one that he maintains respecting the motions of the iris, shows that Mr. Howard is not more critically correct in his physiology than in his anatomy. He asserts that, “ to short-sighted persons, on their bringing an object sufficiently near to see it distinctly, the object appears as large as it would to persons whose sight is not defective, if they looked at it with a magnifying glass” (p. 67) ; that the *pigmentum nigrum* is a fluid secreted by the choroid (p. 74) ; that the use of the ciliary processes is “ for the purpose of conducting vessels to the lens” (ib.) ; that the cornea “ is freely permeated with

\* De Functionibus Nervorum Cerebrallum, pp. 13, 109. Bernæ, 1839.

† Annales d’Oculistique, tome xviii, p. 164. Bruxelles, 1847.

‡ Mackenzie’s Physiology of Vision, p. 198. London, 1841.

vessels" (p. 76) ; and that the lens is supplied with nerves from the iris. (p. 78.) These, besides numerous other mis-statements, prove Mr. Howard to have very unsuccessfully bestowed his attention on the subjects which he treats.

Turning to the pathological and practical part of his work, we must confess that Mr. Howard sometimes astonishes us by the extraordinary success which attends his practice, and at other times by its boldness. Witness the following case of wound of the eye :

" John S—, æt. 35, by trade a cooper, was corking a bottle of beer, when the bottle burst, and two pieces of the glass struck him : one piece wounded the upper lid of the left eye, the second piece the lower lid of the right. On examination, I found a perpendicular wound, of about half an inch long, through the upper lid of the left eye ; it extended into the sclerotic coat, through which protruded the greater part of the iris, and a part of the choroid coat ; the edge of the lid was not separated. The lower lid of the right eye was similarly wounded, but the eyeball was not injured. Four days after the accident, the man was at his work, as well as ever.

" *Treatment*.—Having ascertained that there were no bits of glass in the wound, I first made pressure on the prolapsed iris, then suddenly exposed the pupil to a strong light, when it contracted, and the iris and choroid were restored to their natural position. I then drew together the edges of the wound in the lid, with strips of adhesive plaster, and covered the whole with a compress and bandage ; I dressed the right lid in the same way, and then ordered him to bed, with directions to lie as much as possible upon his back. I gave him a good purge of calomel and jalap, which was followed up by nauseating doses of tartarised antimony. There was no more inflammation than was necessary for producing adhesion." (p. 172.)

We have seen not a few wounds of the eye, in which the iris partially protruded through the sclerotica ; but we do not recollect a single case in which the greater part of that membrane did so, much less, one in which the iris was reduced to its natural place, and a perfect cure accomplished. The pupil contracting as soon as it was exposed to a strong light, even when protruding through the wounded sclerotica, the iris and choroid then assuming their natural place, and the patient being as well as ever, and at his work, in four days, are very surprising circumstances.

Cases of ptosis affecting the upper eyelid of both eyes sometimes occur, which resist all external and internal medications. The patient in such circumstances requires to apply some mechanical means, to raise the upper eyelid of one eye, that he may see to go about. The simplest is to tie a piece of pretty thick and soft worsted tape round the head, so as to embrace the eyelid to be elevated. We have seen this perfectly effectual. It is much better than strips of adhesive plaster, or an elevator of silver wire, applied by means of the frame of a pair of spectacles. For such cases, a Mr. Gerrold, referred to by our author, proposes cutting out a button-hole in the depressed eyelid, while Mr. Howard himself "would prefer taking a semicircular piece out of the edge of the lid, making the notch sufficiently deep to come opposite the pupil." (p. 201.) Both plans are bold, but it is not easy to say which would render the patient's appearance the more hideous.

Mr. Howard notices shortly the cure of bad cases of ectropium by the transposition of a slip of skin, or what is termed a blepharoplastic operation ; and mentions the names of some of the surgeons who have had recourse to it. "The only difference of opinion," says he, "that seems

to have existed between these surgeons, was as to where the new part should be taken from, the shape the piece should be of, and whether it should be altogether detached, previously to its becoming adherent to the part into which it is engrafted." (p. 211.) Surely, as to the last particular, there must be some misapprehension on the part of our author. Till the transplanted piece of skin becomes vitally adherent to the part which it is employed to cover, it should still remain connected by one of its edges to its original place; and only after the adhesion is complete should that edge be divided, if it requires to be divided at all, which is not always the case.

It is a matter of everyday observation, that *porrigo capitis*, sore ears, eruptions about the nose and mouth, and *ophthalmia tarsi*, are more or less associated, in many children, with scrofulous or phlyctenular ophthalmia. All this is usually attributed to a general disorder of nutrition, especially of the parts subservient to the cutaneous secretions. Mr. Howard adopts the following hypothesis on the subject:

"Phlyctenular ophthalmia is generally accompanied with eruptions on both the face and head; I have never found any author who gave a reason for these eruptions occurring in children of a strumous diathesis; all have contented themselves with simply stating that such is the case; but the nature of their origin I consider an important question. I conceive that the cause may be traced to some peculiar state of the fifth pair of nerves; the integuments covering the face and head, as well as the mucous membrane lining the eyes, nose, &c., receive sensation from the fifth pair of nerves; now it is an acknowledged fact, that if the ophthalmic branch of the fifth pair of nerves be paralysed, the result will be ulceration of the conjunctiva and Schneiderian membrane. I therefore consider that, reasoning from analogy, we may suppose, that if the whole of the fifth was paralysed, the integuments it supplies will also ulcerate, although, perhaps, not so soon as the mucous membranes, which are supplied by this nerve. I do not say this must necessarily follow paralysis of the fifth, but when we remember the similarity there is between the integuments and mucous membrane, and that both are supplied by branches from the same nerve, I repeat, that I consider the above conclusion to be reasonable, and certainly deserving of inquiry by those pathologists who devote much of their time to skin diseases." (p. 293.)

This extract affords a pretty fair specimen of Mr. Howard's style, and of his kind of reasoning, when he trusts himself on any subject to his own resources. The reader will observe, that though he talks of tracing the disorders of which he speaks to a peculiar state of the fifth nerve, he does not really adduce the slightest proof of any such connection; and that the fact of there being no approach to any anæsthesia of the face, in the cases referred to, deprives his hypothesis of all probability.

Granular conjunctiva, the result of purulent ophthalmia, is one of the opprobria of eye-surgery. Its intractability is owing to several causes, but chiefly to the improper habits of the patients, who are most of them discharged soldiers, and inveterately given to smoking and drinking.

"In vain have I sought," says Mr. Howard, "to cure this disease by any treatment recommended by authors or lecturers on ophthalmology. I have tried them all with the same result,—perfect failure. I have asked many men in Montreal, and elsewhere, whether they had ever seen a case of granular conjunctiva cured, and invariably the answer has been, No; but several of them, with many medical students, can now testify, that they have seen many cases cured by me; and I do not think I am guilty of exaggeration in saying, that within the last four years I have cured upwards of two hundred of such cases.

"My treatment is both constitutional and local. Believing, as I do, that the granulations form in consequence of the diminished power of the absorbents, and that this is produced by the disordered state of the ophthalmic branch of the fifth pair of nerves, my object is to restore these parts to their healthy action, and thus get the granulations absorbed, and not destroyed, as is the usual practice. To accomplish this, the local application that I use is the acetate of lead, in the following manner: when I first see the case, I evert the lids, and then wipe the granulations quite dry with a soft sponge, after which I dust them over with the acetate of lead, it being previously pulverised in the finest possible manner.\* I keep the lids thus everted with the lead on them for four or five minutes, then let a little water pass over them, after which I restore the lids to their natural position. If much irritation is produced, I remove it, by brushing the parts over with a little olive oil. Every day after the first, till the eyes are perfectly well, I drop on the eye a large drop of the saturated solution of the acetate of lead, and order the patient to smear the edges of the lids, every night, with a small portion of the red, or Janin's ophthalmic ointment. If the case is not a bad one, I do not use the acetate of lead in powder, but depend altogether upon the saturated solution, which cures the disease equally well, but is a little more tedious than when the powder is used. To this solution there is not the general objection that there is to the other modes of using lead, that is, the formation of a deposit upon the abraded conjunctiva, and an incurable opacity; for it may be used daily for months without any such evil consequences. The mode of preparing it, is to make a saturated solution of it in pure water, and then to filter the solution.

"Every morning I brush the lids and round the upper part of the orbit, with the eight grain solution of veratria, and if the patient is an adult, order him to take one table-spoonful of the following mixture three times a day:

"R Sulph. quinine, gr. viij;  
Tart. Ant., gr. j;  
Acid. sulph. aromatic. gut., x;  
Aquæ fontan., ʒviij. M.

"I never confine my patient to the house, nor keep him on antiphlogistic diet; on the contrary, I make him take plenty of exercise in the open air, if the weather is fine, and order him to use good, wholesome food, both animal and vegetable, and even to take a glass or two of wine at dinner.

"This treatment I have now adopted for four years, and I can safely say, I have not met with one case of granular conjunctiva in that time that I have not cured, excepting, in some instances, where the conjunctiva had been destroyed by the use of escharotics, such as the nitrate of silver, and the everlasting sulphate of copper, which is used so generally in this disease." (p. 378.)

Some part of this treatment we admit to be highly judicious, such as the good diet, air, and exercise, together with the solution of quinine. Even Mr. Howard's favorite, the eight-grain solution of veratria, applied to the skin, may act beneficially as a counter-irritant to the diseased conjunctiva. The powdered acetate of lead, which is not a new application† in granular conjunctiva, we have seen tried, without any advantage, but, on the contrary, with manifest increase of the pain, and a deposition of lead on the excoriated spots on the inside of the lids, which was tedious and troublesome of removal.

Mr. Howard's pathology of Cataract appears to us essentially wrong. He asserts the lens and its capsule to be "as well supplied with vessels, nerves,

\* "I put the fine powder into a phial, then cover the mouth of the phial with a piece of fine muslin. This bottle I use as an ordinary pepper-castor."

† See the account of the practice of MM. Thiry and Buys, in the *Ann. d'Oculistique*, tom. xxi; *Gaz. Méd. de Paris*, 1849, tom. iv, pp. 564, 578; and in Vol. IV, p. 556, of this Review.



Sinus, Frontal Sinus, Sphenoidal Sinus, Cranium; and so on, with almost every succeeding chapter in the book. But not only is the arrangement of subjects borrowed, but the substance of what is said on most of them is, in a large proportion, borrowed also. A half, or perhaps two thirds of the work, is an abridgment of Dr. Mackenzie's Treatise, such as a medical student might be supposed to make for his own improvement, but such as no man, alive to the *meum* and *tuum* of literary property, would ever venture to publish as an original work.

The reader requires only to open Mr. Howard's book at almost any page, and compare what he finds there set down with the substance of Dr. Mackenzie's section on the same subject, to be convinced of the truth of what we have now stated. The two following extracts, selected in this way, will show the reality and the general style of the abridgment. The first relates to Entropion.

"Entropion is of three varieties, viz.:—*acute*, *chronic*, and *traumatic*, all of which, although producing the same effects, depend upon different causes. The first is generally found in old persons; the second in young and scrofulous children; and the third may be in either, as it is the result of injury.

"In the acute form of entropion, the eyelid is simply inverted, without any shortening, and is caused by the integuments becoming relaxed and swollen, as the conjunctiva does in the ectropium; I am, however, of opinion that there is also an irregular spasmodic contraction of some of the fibres of the orbicularis muscle; indeed, the pain produced by the cilia rubbing against the eyeball must necessarily produce blepharospasmus. In this form of entropion the eyelashes can be brought to their natural position, by pinching up the swollen integuments of the lid, so as to take the weight off the eyelid, but, upon letting them go again, the lid will roll itself in as before.

"In the chronic form of entropion, the edges of the lids are thickened and irregular, the cartilage is shortened and contracted, and the lid being inverted, presses hard against the eyeball; the integuments are tense and vascular, the ciliæ small and irregular.

"The frequent attacks of inflammation alter the cartilage and conjunctiva, sometimes producing an ulcer and cicatrix, which shortens as well as inverts the lid. The cause of this form of entropion is, generally speaking, the neglect of long-continued ophthalmia tarsi, or catarrhal conjunctivitis. Some are of opinion, that in this form, also, there is irregular contraction of the fibres of the orbicularis; for my own part, I am of opinion, that every time the eye is irritated, there is contraction of the fibres of the orbicularis muscle.

"The traumatic form of entropion is generally caused by scalds or burns of the conjunctiva, lining the lids, or its being injured by escharotics, such as quick-lime, or some strong metallic acid; I have never seen such a case that was not accompanied, in some degree, with symblepharon; and symblepharon can hardly exist without, in some degree, producing entropion." (p. 216.)

Our second quotation is descriptive of the operation for Displacement of Cataract:

"The reader will have perceived, that the operations of displacement divide themselves into four periods, viz.:—First, the introduction of the needle through the tunics into the vitreous humour. Second, the dividing of the posterior capsule of the lens. Third, the breaking up of the anterior hemisphere of the capsule. And fourth, the displacement of the lens. He must also have perceived, that there is but one instrument used, viz., a needle, the point of which is curved and flat with cutting edges. There are some minute directions given, which it is necessary to explain. The object of having the ring and little finger supported by the cheek-



bone, is to guard against the needle entering too suddenly, and probably too deeply, into the eye.

"The needle is to pierce the tunics with its concave surface downwards, to avoid wounding either of the branches of the iridal artery with its cutting edges, which it would be very likely to do if it were entered with its cutting edges upwards and downwards. It is to be entered a little below the equator of the eye, that the tunics may act as a fulcrum for the needle, so that when the handle is brought to the horizontal position, the point of the needle with the cataract will be sufficiently below the pupil, not to require any direct pressure on the point of the needle to push the cataract down. If the needle is entered too far back from the cornea, it is likely to wound the retina; if too near the cornea, it will wound the ciliary processes. The reason for directing it, in the first instance, towards the vitreous humour, is to prevent it from sticking into the lens." (p. 445.)

Mr. Howard has great faith in veratria; so much so, that he recommends it for almost every kind of eye-disease. If there be too much action in the disordered part, he quiets it with a touch of veratria; if there be too little, he brushes it up with veratria. For example, in twitching of the eyelids, he says, "The local treatment that I have found to answer most effectually, is brushing the part with a solution of veratria, counter-irritation between the articulation of the lower jaw and mastoid process, together with dry cupping on the temple and nape of the neck." (p. 196.) In blepharospasmus, "I believe," says he, "that brushing the lids once a day with the solution of veratria constitutes the most efficacious local application." (p. 197.) In paralysis of the orbicularis palpebrarum, a disease just the reverse of the last mentioned, he brushes the course of the portio dura "with the eight grain solution of veratria every morning." (p. 199.) In ptosis, he "found the best local treatment to be, brushing the lid every morning and evening with the eight grain solution of veratria,—in conjunction with electricity." (p. 201.) In scrofulous ophthalmia, the eyelids, eyebrows, and temples are brushed with the eight grain solution of veratria every morning. "Its immediate effects," says Mr. Howard, "in strumous ophthalmia, are to remove the morbid irritability of the extreme filaments of the fifth, and thus get rid of the painful sensibility to light, profuse lachrymation, and blepharospasmus; and this is doing much towards accomplishing the object in view. I believe it even does more than this, for, by giving the nerve its healthy action, it restores the nutritive properties necessary for a healthy eye." (p. 298.) In conjunctival xeroma, we again find the veratria set forth as one of the chief remedies. (p. 382.) In amaurosis depending on congestion of the retina, the local treatment from which Mr. Howard has found most benefit, "is fumigating the eye every day with hydrocyanic acid, brushing round the orbit with the eight grain solution of veratria, passing a stream of electric fluid along the course of the nerves, and dry cupping the nape of the neck." (p. 470.) Tremulous iris is a disease which is rarely amenable to treatment. "I met with one gentleman, however," says our author, "who had received a blow on the eye, which produced tremulous iris and mydriasis, and also rendered him quite blind: under the treatment I adopted, the pupil contracted, and the motion of the iris ceased; this treatment consisted in giving calomel and quinine until the system was affected by the former; at the same time, applying electricity to the eyelids, and brushing round the orbit every day with the eight grain solution of veratria." (p. 500.) In night-blindness, "the

only local treatment," says Mr. Howard, "is brushing the eyelids, and round the orbit, with the eight grain solution of veratria." (p. 503.)

It is well known, that as the experience of numerous physicians who have made trial of veratria has not justified the extravagant praises bestowed on it when first introduced as an external application for neuralgia, it has fallen into disrepute. That it may have some effect, as a stimulant, in some of the diseases in which it has been used by Mr. Howard, is not improbable; its action, however, is not understood; and in all likelihood the same results would have followed in many of his cases, had the brushing been performed with some much less expensive and less poisonous article of the materia medica.

The same remark may be applied, we believe with justice, to his fumigations with the vapour of prussic acid. In dissipating the nebulous state of the cornea, left by corneitis, the application of the prussic acid vapour is certainly useful. But in chronic retinitis (p. 329), and in congestive and other varieties of amaurosis (pp. 470, 474), in which Mr. Howard employs it, its influence must be very equivocal, while in cataract (p. 354), we believe it to be null. Mr. Howard represents it as acting "both as a stimulant and as a sedative to the eye" (p. 329); but though it is easy to explain how the same breath may blow hot or cold alternately, the ascription of such opposite properties to the same drug appears to be preposterous, unless the depressing effects are supposed to be a result of the excitement previously produced; or the states of the eye are such, that the same influence may at one time stimulate that organ, and at another depress it, neither of which positions is advanced by our author.

In the following passage of his preface, Mr. Howard plainly hints, that he is the original inventor of some new treatment for disorders of the eye. He tells us, that he wishes "to express to those learned and respected friends who have contributed many valuable notes and suggestions, his sense of the service they have rendered to his inquiry; a service by which, in some instances, he has been enabled, not only to enrich his work, but to confirm his own experience of the propriety of the treatment which he has successfully pursued; a treatment, which the author may be pardoned for stating, has not been suggested in any published treatise on the Pathology of the Eye, which has come under his notice." (p. v.) In our faithful perusal of Mr. Howard's work, we have carefully watched for the new treatment thus announced; but we have not been able to detect anything that was not perfectly familiar, we think, to practitioners, except one little particular. Mr. Howard is very fond of fumigating the eyes with prussic acid vapour, but that is not new; he brushes the lids and brow with a solution of veratria, but that is not new. The only novelty, in the way of treatment, which we have noted, is mentioned at p. 337, under the head of Iritis, in the following terms: "When using the extract of belladonna, I always apply it round the back and front of the ears, and find it has just as good an effect as if applied round the orbit."

## ART. IX.

1. *Lectures on Inflammation, delivered in the Theatre of the Royal College of Surgeons of England.* By JAMES PAGET, Professor of Anatomy and Surgery to the College. (From the 'London Medical Gazette.')—London, 1850. 8vo, pp. 57.
2. *On the State of the Blood and the Blood-Vessels in Inflammation, ascertained by Experiments, Injections, and Observations by the Microscope; being the "Astley Cooper Prize Essay" for 1850.* By T. WHARTON JONES, F.R.S. (From the 'Guy's Hospital Reports.')—London, 1850. 8vo, pp. 100. With Nine Plates, and Wood-engravings.
3. *General Pathology, as conducive to the Establishment of Rational Principles for the Diagnosis and Treatment of Disease; a Course of Lectures, delivered at St. Thomas's Hospital during the Summer Session of 1850.* By JOHN SIMON, F.R.S., one of the Surgical Staff of that Hospital, and Officer of Health to the City of London.—London, 1850. 12mo, pp. 288.
4. *The Principles of Surgery.* By JAMES MILLER, F.R.S.E. F.R.C.S.E., &c. &c. Second Edition.—Edinburgh, 1850. Chapter II.—*Inflammatory Action, and Congestion.*

It happens by a somewhat singular coincidence, that three of those individuals, from whom, in this country, we have the most reason for looking for an application of sound physiological knowledge to the advancement of scientific pathology, should have given to the public their doctrines and researches upon the subject of Inflammation at almost exactly the same time. For Mr. Paget's course of Lectures we have now come to look as one of the regular enjoyments of our London "season;" going to listen to them when delivered, and reading them when published, with the feeling that we shall not merely receive instruction in whatever topic the lecturer may have selected for special illustration, both from the stores of information which he collects from all who can impart it, and from that which he adds to these by his original investigations; but that we shall also have an excellent lesson in philosophy, taught in that best of all modes, an attractive example. For we know of no medical writings that more strikingly evince the philosophic habit of thought, which seems to pervade the whole mind of the writer, and to govern his method of treating every subject which he takes in hand; nor do we know any in which the ideas, however recondite, are more lucidly expressed, or in which the style is at once so elegant, and yet so vigorous, so correct, and yet so free from tameness.

Mr. Wharton Jones's Essay comes before us with the *prestige* of successful competition for the "Astley Cooper Prize" of Three Hundred Pounds, and evidently contains the results of patient and attentive study of the subject by actual observation. The attention of the author, however, was limited by the terms on which the prize was offered, to the examination of those phenomena alone which are presented by the blood and the blood-vessels; phenomena which, in our apprehension, cannot be regarded as essential or fundamental constituents of the inflammatory state. How far he himself so regards them, we cannot feel quite clear; but from the

absence of other considerations, we cannot but apprehend, that his essay will most assuredly mislead any one who studies it with the idea that it will teach him in what inflammation really consists. We believe Mr. Wharton Jones to have most patiently observed, and most conscientiously recorded; but in his interpretation of the phenomena he has witnessed, we find deficiencies and inconsistencies, for which we are somewhat at a loss to account.

Mr. Simon's lectures constitute a short course of General Pathology, in which Inflammation figures, of course, as one of the prominent subjects. We have, on a former occasion (vol. ii, p. 300), noticed with satisfaction, the creation of the chair of surgical pathology at St. Thomas's Hospital, and the nomination of a gentleman so well fitted as Mr. Simon to discharge its duties. The small volume before us is a fair sample, we presume, of the instructions which are given under this arrangement; and we welcome it cordially as an addition to our professional literature, which is calculated to be of much service in enlarging the ideas of its readers as to the intimate connection between the highest scientific doctrines in pathology, and the most eminently practical views of the treatment of disease. We do not accord with Mr. Simon in all that he teaches, and shall presently have occasion to express our most decided dissent from one of his doctrines; but, on the whole, we feel assured that such teaching cannot be presented to a set of advanced and intelligent students, without giving a most beneficial direction to their thoughts, and elevation to their aims.

Of the general character of Mr. Miller's 'Principles of Surgery,' we have elsewhere expressed ourselves in the present Number; and of the chapter on Inflammation, we may say, that it comes up in every particular to the high character which we have given of the work as a whole. Mr. Miller, however, does not treat the Theory of inflammation, to which alone we must restrict ourselves for the present, with such elaboration as need induce us to dwell specially upon his views of it. His account of its essential phenomena is clear, concise, and (in our apprehension) accurate in almost every particular; but, as his work is intended to bring forward those general principles which are especially adapted for the guidance of the practitioner, rather than to investigate questions which have as yet comparatively little practical bearing, he devotes his chief attention to the account of the causes of inflammation, the varieties which present themselves in its course and terminations, and the principles on which its treatment must be conducted. These are topics which will occupy our attention in a future article, wherein we shall have occasion to make frequent reference to Mr. Miller's work. Of his theory of inflammation, however, we must remark that he uses the term "active congestion" in a wider sense than that in which it is generally employed; since he employs it to designate that stage in what is usually termed the "inflammatory" process, in which exudation of plastic liquor sanguinis takes place; "true inflammation" being, with him, "denoted by more or less extravasation and destruction of texture, and the formation of pus in progress." Now upon this we must remark, that if Mr. Miller's phraseology be adopted, we shall have to make very considerable alterations in our nomenclature. His idea of inflammation is obviously formed rather upon the *surgical* than the *medical* aspect of the process; and the very diagram which he gives

in illustration of it, consisting of three concentric circles, with "inflammation" inscribed in the innermost, "active congestion" in the second, and "simple vascular excitement" in the outermost, is obviously suggested by the formation of a phlegmonous abscess. But are pneumonia, rheumatism, and a host of other diseases which never go on to suppuration, and some of which do not even tend to do so, to be excluded from the category of inflammations? Surely Mr. Miller cannot mean this.

We shall offer no apology to our readers for requesting their attention somewhat at length to this topic. It is one, we need hardly remind them, which must occupy the most prominent place in every system of scientific pathology. And if the researches of scientific investigators have hitherto contributed but little to the improvement of practice, it is, we believe, because they have fixed their attention too exclusively upon certain classes of phenomena, and have taken little or no account of changes which did not immediately declare themselves in an easily recognisable manner. At present we feel persuaded, that little or nothing more can be expected from researches carried on after the old model, and that we must take (as our German neighbours phrase it), a new "stand-point," from which to survey the phenomena of inflammation. This we shall endeavour to show, in the course of the critical digest which we shall now attempt to make of the doctrines of Messrs. Paget, Wharton Jones, and Simon, touching the essential nature of the inflammatory state.

We quite accord with Mr. Paget in thinking that it is better, in the present state of our pathological knowledge, to abstain from any formal definition of the term *Inflammation*. The attempts which have been hitherto made to characterise the process in set terms, have either been mere enumerations of symptoms, or assertions of what is supposed to constitute its "proximate cause." The former, even if they could be made precise and complete, would tell us nothing of the essential nature of the departure from the normal condition; thus, the time-honoured formula, *rubor et tumor cum calore et dolore*, simply expresses certain objective manifestations (which, however, are far from being universally present), and no more informs us of what inflammation is, than we should learn what normal nutrition is, by being told that it consists in the absence of all these phenomena. And in regard to the latter, it is sufficient to mention, that two of the definitions which have gained the greatest currency, are in direct opposition to each other; one party maintaining that inflammation is "an increased action of the vessels," with acceleration of the flow of blood in the affected part; whilst the other asserts, on the contrary, that inflammation is "a diminished action of the vessels," with retardation of the flow of blood, or actual stagnation of it in the part. In quoting these opposite definitions, however, and in commenting upon them (pp. 2, 3), Mr. Wharton Jones leaves altogether out of sight a third view of the nature of inflammation, which has gained currency of late years among intelligent pathologists in this country, and which regards the changes in the condition of vessels, and in the rate of movement of the blood, as secondarily, rather than essentially, concerned in the production of the other phenomena of inflammation; looking for the primary departure from the normal state, in the disturbance of those reactions between the blood and the tissues, which constitute the proper function of nutrition. With those who survey the question from this point of view, Mr



Paget has allied himself; and believing as we do, that this is the only source from which much new light on the subject can be expected,—experiment and observation having been well-nigh exhausted in attempts to determine the condition of the blood-vessels, and the alterations in the rate of the blood's movement,—we shall take his mode of discussing it as our guide, and shall follow him through the chief heads of his admirable discourses, the leading idea of which is contained in the following extract from his introductory remarks :

“The very difficulty of exactly defining the process of Inflammation may be our guide to the most hopeful method of investigating it. When we see such gradual transitions, from the normal process of nutrition to the disease of inflammation, that we cannot draw a definition line between them, we may be sure that the main laws of physiology are the laws alike of the disease and of the healthy process; that the same forces are engaged in both; and that, though interfered with by the conditions of the disease, they are not supplanted or annulled.

“Now, such transitions from the normal processes to that of inflammation are not rare. We may trace them, for example, in the gradual passage from the active exercise of the brain, or of the retina, to its ‘irritation’ when overworked, and thence, to its complete inflammation and impairment of structure, after long exposure to what had been a natural stimulus, or to what, in a less degree, might be so. Or, on the introduction of medicines, such as certain diuretics, into the blood, we may trace gradations from the normal increase of the functions of the kidneys, under what is regarded as no morbid stimulus, to their intensest inflammations. Or, again, in the application of an abnormal stimulus, such as that of a heat greater than the natural temperature of the body, where shall we mark the line at which inflammation begins to supervene on health? We may, indeed, say that stagnation of blood, or effusion of liquor sanguinis, shall be the condition *sine qua non* of inflammation; we may call whatever falls short of these, ‘active congestion,’ ‘irritation,’ or by any other name; but in practice, such distinctions are often impossible, and sometimes untrue; and in study, the terms are convenient for the sake of brevity rather than of clearness.

“Evading, then, the question of the precise definition of inflammation, I shall endeavour, first, to describe the state of an inflamed part, giving to the description such a plan and direction as may best help the chief design of contrasting the inflammatory with the normal method of nutrition, and of showing that the immediate causes, and the chief constituents, of the inflammatory state are to be found in alterations of those things which are necessary conditions of the healthy nutrition of a part.” (pp. 3-4.)

In our review of Mr. Paget's Lectures on Nutrition, &c. (Vol. I, p. 404), we followed him through his examination of the *Conditions necessary to Healthy Nutrition*; and he here adopts the same method of distributing these conditions under four heads, viz. :—1st. A regular and not far distant supply of blood;—2d. A right state and composition of that blood;—3d. A certain influence (at least in most cases), of the nervous force;—and, 4th. A normal state of the parts in which nutrition is to be effected. *All these* are usually altered in Inflammation.

I. The *Supply of Blood* to an inflamed part is usually altered in two modes, which it will be necessary to consider separately;—a change in the caliber of the vessels, and in the resisting power of their walls;—and a change in the rate of the blood's motion through them. By some, the second of these changes is considered as a mere sequence of the first; but we shall endeavour to show, in the proper place, that such is not the case.



a. We shall in the first place inquire, then, *what changes present themselves in the condition of the walls of the blood-vessels*, in a part where the inflammatory state has developed itself. On this point there is now but little difference of opinion; for although some maintain that a contraction of the vessels takes place in the first instance on the application of an irritant, all admit that this contraction soon gives rise to dilatation; the result of which, and of the increased crowding of the vessels with red-corpuscles, manifests itself in the redness of the inflamed part. In parts which have no blood-vessels of their own, and yet which manifest certain of the signs or effects of inflammation, these generally, if not always, involve (as Mr. Paget remarks) enlargement of the adjacent vessels, and especially of those from which the diseased structure derives its natural supply of nutritive material. Thus, in inflammation of the cornea, the vessels of the sclerotica and conjunctiva are enlarged; and in ulceration of the articular cartilages, the same enlargement is seen in the vessels of the surrounding synovial membrane, or subjacent bone. From Mr. Paget's observations upon the vessels in the wings of bats, and from the general appearances of inflamed parts, he concludes, that the enlargement takes place more readily in warm-blooded animals than in cold. The redness is not at first attributable in any degree to the formation of new blood-vessels, for no such process takes place until after the subsidence of the inflammation; it is then only that the lymph exuded from the vessels begins to acquire a higher organisation, and that vessels pass into it from the neighbouring textures with a view to the maintenance of its increase or development. It is true that many more vessels become distinguishable by the naked eye in an inflamed part, than exist in the same part in its natural state; and when a membrane in a state of inflammation, such as the bat's wing or the frog's foot, is examined under the microscope, many vessels are found in it, which cannot be seen in the corresponding part on the opposite side of the body.

But the shortness of the time after stimulation, within which we find vessels that were previously invisible, presenting a considerable size, and becoming crowded with blood-corpuscles, is a sufficient proof that they must have previously existed, although they may have been so minute as not to admit even a single file of blood-disks; since the formation of entirely new blood-vessels is not a process that can take place within a few minutes, or half an hour. And, moreover, if the natural circulation be watched for a sufficient length of time, it will be perceived, that some of the smaller vessels at first seen gradually diminish in caliber, and become apparently obliterated, whilst others come newly into view, obviously because they have been previously in a similarly contracted condition.

The effects of various artificial stimuli of kinds upon the caliber of the blood-vessels, has been the subject of examination by a great number of experimentalists; and there is a very general accordance in their results. Mr. Paget has made much use of slight mechanical stimuli.

"If, as one is watching the movement of blood in a companion artery and vein, the point of a fine needle be drawn across them three or four times, without apparently injuring them or the membrane over them, they will both presently gradually contract and close. Then, after holding themselves in the contracted state for a few minutes, they will begin again to open, and, gradually dilating, will acquire a larger size than they had before the stimulus was applied.

"Simple as this observation is, it involves some cardinal facts in our pathology. It illustrates, first, the contractile power of both arteries and veins; and, by the manner of their contraction, which follows at some interval after the application of the stimulus, and is slowly accomplished, it shows that their power of contraction is like that of parts with simple or organic muscular fibres. And one may notice here the illustration of the stoppage of hemorrhage from small vessels. In one cut we may divide a hundred such vessels as these on the surface of a stump, and they may cease to bleed in a few seconds: doubtless, the very stimulus of the knife, while dividing them, has made their walls contract and close.

"But, again, the experiment shows the vessels reopening and becoming wider than they were before, either yielding more to the pressure of the blood which previously they resisted with more strength, or else dilating, as of their own force, with that which Mr. Hunter called active dilatation, and compared with the act of dilatation of the os uteri. In whichever way the dilatation is effected, whether it be active or passive, the vessels will not at once contract again under the same stimulus as before affected them. The needle may be now drawn across them much oftener and more forcibly, but no contraction ensues, or only a trivial one, which is quickly succeeded by dilatation. Yet, with a stronger stimulus, such as that of great heat, they will again contract and close. And such a contraction excited by a cautery may last more than a day, before the vessels again open and permit the flow of blood through them. So that in this we have an illustration of the secondary hemorrhages from vessels, which, after their first closure, have not been sealed by the coagulating blood, or the exudation of lymph,—as well as an illustration of the effect of the cautery or of hot water in again checking such hemorrhages, and more permanently closing up the vessels."\* (pp. 6-7.)

On applying a drop of acetic acid, or of tincture of capsicum, or turpentine, or of the ethereal solution of cantharides, to a portion of the wing, Mr. Paget found that dilatation of the vessels quickly ensued, without any appreciable contraction; and this dilatation was found to extend from the spot to which the stimulating liquid was applied, so that the whole of the blood-vessels in the corresponding metacarpal space, and even those in the adjacent spaces, sometimes partook of the enlargement.—The influence of various agents upon the arteries in the web of the frog's foot has been carefully studied by Mr. Wharton Jones; and the following are his general results:

"1. Constriction may slowly take place, and be slowly succeeded by the normal width. This I have found to be the effect of the action of the sulphate of atropia. A solution of this substance, in the proportion of three or four grains to the ounce of water, applied to the web, causes constriction of the arteries, in about the same time that it causes dilatation of the pupil when dropped into the eye. In other words, it causes contraction of the circular fibres of the muscular coat of the arteries in about the same time that it causes contraction of the radiating fibres of the iris. As in the case of the iris, the arteries slowly recover from the action of the atropia.

"2. Constriction may quickly take place, and be soon succeeded by the normal width, or a width not much exceeding the normal. This has been found to be the effect of the moderate application of cold, mechanical irritation, and galvanic irritation. When a drop of cold water is applied to the web, the arteries are seen to become constricted, but they soon resume their previous state. Slight pressure on the web, with a blunt point, for example, in the situation of an artery, excites constriction of it, but by and by the vessel becomes dilated, as before. When the web is subjected to the influence of a single pair of plates, constriction of the arteries is induced, succeeded by dilatation, as in the preceding cases.

\* "For the control of hemorrhages, something more than the contraction of the vessels is required, that their orifices may be sealed before they can again dilate; and it is probably to the absence of this something, more than to any condition of the vessels, that we must look for the explanation of secondary hemorrhages from small vessels."

"3. Constriction either does not take place at all, or, when it does, it very rapidly gives place to great dilatation. This is the effect of such agents as the following:—a solution of sulphate of copper (gr. xvj—3j) with wine of opium (3j) dropped on the web, I have generally found to cause primary dilatation of the arteries. A strong solution of common salt I have found to have a similar effect, only, perhaps, momentary constriction has more frequently preceded the dilatation. Battley's *liquor opii sedativus* usually first excites constriction of the arteries, but dilatation quickly supervenes. A drop of spirit of wine immediately causes constriction, but this is soon followed by dilatation, of the arteries.

"4. Dilatation, preceded or not by momentary constriction, may slowly yield to constriction, which remains permanent. This is the effect of sulphate of copper in a concentrated solution. When a point of blue-stone is rubbed on the web, in the neighbourhood of, or over an artery, the vessel slowly but steadily becomes completely constricted, and remains so." (pp. 8-9.)

Both the authors just cited remark, that the dilatation is frequently by no means uniform, but is of a varicose or aneurismal character; the enlargements having sometimes a fusiform shape; sometimes being shorter and nearly spherical dilatations of the vessels; sometimes being round, or oval, or elongated pouches projecting from one side of the wall; in short, presenting all the varieties of form which we are accustomed to recognise in aneurisms or aneurismal dilatations of great arteries. By Kölliker and Hasse, who published the first observations upon this change, which they had observed in a case of inflammatory red softening of the brain, it was supposed that the vessels implicated were capillaries; but subsequent observations have shown that it is in the smaller arteries, that the chief, if not the only, liability to this change exists. It does not appear, however, that such varicose dilatations have any necessary or important connection with the other phenomena of inflammation, although they may be frequently, or even constantly found in inflamed parts. They merely indicate a local relaxation of the walls of the vessels at those points, allowing their dilatation by the pressure of the blood-current; and it is possible that, as Mr. Quekett has suggested, they may "represent a disturbed effort for the production of new blood-vessels by dilatation, or outgrowth from the walls of those already extant." From the manner in which they are mentioned by Mr. Wharton Jones (p. 7), we infer that he has found such dilatations presenting themselves occasionally, without the application of any stimulus, or the presence of any sign of inflammation.

Besides their increase in caliber, we find that the vessels of inflamed parts are dilated in length; they consequently become more tortuous, and their pulsations are stronger than natural, each impulse increasing their tortuosity. This change may be well seen in the vessels of the conjunctiva; in which part, according to Mr. Wharton Jones, the small arteries may be recognised by their pulsations, these being manifested by increased bendings as the pulse-waves pass through them. It is felt in the violent throbbing, which is frequently one of the most painful sensations of an inflamed part.

On the whole, then, we may conclude, that the *dilatation of the vessels*, especially of the arteries, with a weakening of the resisting power of their walls, is one of the most constant phenomena of the inflammatory state. Whether this shall be called an increased or a diminished "action of the vessels," depends upon the meaning we may attach to that term; but as modern physiology altogether disallows the notion that the *vessels*

can contribute anything to the *action* of a part, otherwise than by serving as conduits for the conveyance of the blood which that action requires, we think it much better that the term should be discarded from pathology also, and that the simple fact of the undue distensibility should be taken for what it is worth. That it does not, of itself, go far to explain the phenomena of inflammation, will be sufficiently evident, when it is considered that the same state of dilatation may exist in cases of mere *congestion*; both those in which the vital action of the part (whatever it may be) is increased or exalted, and which may be called "active" congestion, or determination of blood; and those in which it is depressed, as happens in what is termed "passive" congestion. Both these states may pass into inflammation; but they do not of themselves constitute the inflammatory state.

b. The supply of blood to the tissues of the inflamed parts is affected, not merely by the alteration in the caliber of its vessels, but by *a change in the rate of movement of the blood*. Upon this point, again, there is no difference of opinion as to the fact, that a retardation, and even complete stagnation, of the current is one of the most constant phenomena of the inflammatory state, when this is fully developed; although there is a great want of agreement, both as to the cause and the consequences of this retardation. In order fully to appreciate these, it is necessary to take into account the *whole sequence* of the phenomena.—On the first application of the stimuli already mentioned to the bat's wing, Mr. Paget found the streams of blood in the dilated vessels to be *more rapid* than in the unaffected vessels of the same or of the other wing. They were also, he says, more steady; "for, in what appears to be the natural state of the circulation in the bat's wing, it is not unfrequent to see an occasional oscillation in the venous stream, an occasional stoppage or back-current, and then a more forcible rush forward; but these are rarely seen when the stimulus has produced the effect just described." Now, this condition is certainly *not* inflammation; for it is never seen in parts in which the unequivocal indications of that state are present. There is every reason, however, to believe it to be the condition of parts in which there is exalted functional activity, and to which there is an unusual "determination of blood;" a condition which is very often the preliminary to true inflammation, but which may subside without the supervention of any further change.

"It is, probably, just such a state as this that is felt by suckling women in what they term the "flow of milk," which is, no doubt, an increased flow of blood into the mammary gland just before a quicker secretion of the milk. Less normally, it is such a state as this that we observe in the skin after the application of mustard, or sharp friction, or a heat from 20° to 50° above its own, or in the conjunctiva when stimulated by dust that is soon dislodged; and such we may believe to be the condition of many internal organs when we might doubt whether they are inflamed, or are only very actively discharging their natural functions. Herein, indeed, in what I have described, is one of the pieces of neutral ground between health and disease: a step in one direction may effect the return to health, in another the transit to what all might admit to be the disease of inflammation." (pp. 7-8.)

The passage of this state of "active congestion" into the normal condition, is indicated by the retardation of the current of blood, with a gradual contraction of the vessels. On the other hand, the transition to

the inflammatory state is marked by a further retardation of the current, without any diminution, but (it may be) with even an increase, in the diameter of the vessels. This may be observed as the consecutive result of the application of stimuli to the bat's wing; but for the sake of bringing all the phenomena into view at once, Mr. Paget recommends the infliction of such an injury as passing a fine red-hot needle through the membrane.

"The first effect of such an injury (in addition to the charring and searing of the membrane, the obliteration of its blood-vessels, and the puckering of the portion of it adjacent to the burn) is to produce contraction of the immediately adjacent arteries and veins. They may remain closed, or, as I have already described, after being long closed, may again open, and become wider than they were before. This dilatation follows more certainly, and perhaps without any previous contraction, in the arteries and veins at a little distance from the burn. In these there speedily ensues such a state of 'determination of blood' as I have already described: in arteries and veins alike the stream is full and rapid; and the greater accumulation, as well as the closer crowding of the red corpuscles, makes the vessels appear very deep coloured. The contrast of two diagrams, showing the natural and the stimulated conditions, illustrates this difference sufficiently well. The vessels of the one, nearly twice as large as those of the other, darker, and more turgid with blood; and, in the one, numerous capillaries which are not visible in the other. But diagrams cannot show the changes in the mode of movement. Close by the burn, the blood which has been flowing rapidly begins to move more slowly, or with an uncertain stream,—stopping, or sometimes ebbing, and then again flowing on, but, on the whole, becoming gradually slower. Thus it may, at length, become completely stagnant; and then, in the vessels in which it is at rest, it seems to diffuse and change its colour, so that its crowded corpuscles give the vessels a brilliant carmine appearance, by which, just as well as by the stillness of the blood, they may always be distinguished. As one surveys an area surrounding this part in which the blood is stagnant, or moves slowly, one sees the streams increasing gradually in rapidity. And often, when there is stagnation in a considerable artery, one may see the blood above or behind it pulsating with every action of the heart, driven up to the seat of stagnation, and thence carried off by the collateral branches; while in the corresponding vein it may oscillate less regularly, delaying till an accumulated force propels it forward, and, as it were, flushes the channel.

"Again, in the same area as that in which the movement is pulsatile or oscillating, and in the area beyond it, one sees the full and rapid and more numerous streams of 'active congestion;' and these may extend over a space altogether uncertain.

"Such is the general condition of the circulation round a part thus inflamed; but the varieties in lesser points that may be presented cannot be described. These must be seen; and, indeed, the whole sight should be viewed by every one who would have in his mind's eye a distinct image of what in practice he must often too obscurely contemplate." (p. 8.)

The observations of Mr. Wharton Jones on the frog's foot are to the same effect. The stimulating agents, whose application causes dilatation of the vessels, generally, in the first place, cause an acceleration in the movement of the blood; which acceleration, according to Mr. Jones, is the result of a diminution to the resistance offered to its motion by friction against the walls of the vessels,—an explanation whose validity we shall presently consider. But this acceleration soon gives place to retardation, if the stimulating agent be applied in sufficient strength; and eventually, the circulation may be altogether arrested in the part. Mr. Wharton Jones has made many experiments on the effects of division of the



arteries, veins, and capillaries, separately and together ; and the following is his general statement of results :

“ We have thus seen, that when an artery is cut across, there take place congestion and stagnation in the capillaries to which it leads ; that when a vein is cut across, congestion takes place in the capillaries from which certain of the radicles of the vein below the wound arise, but stagnation only here and there ; that when capillaries only are cut across, stagnation of blood takes place in those, on the side of the wound next the artery, which are implicated, only to where they are in connection with the other capillaries, whereas the stagnation is more extensive in those on the opposite side of the wound ; we have seen, lastly, that when the accompanying nerve of an artery is cut, the flow of blood does not appear to be particularly influenced.

“ When a wound involving arteries, veins, capillaries, and nerves, is inflicted on the web, we have a combination of these effects ; but, perhaps, to a greater degree, in consequence of the reciprocal influence of the impediments to the flow of blood in the artery and vein below the wound. And it is thus, that the congestion and stagnation are brought about, which occasion the redness by which the inflammation arising from the injury is manifested.” (pp. 33-4.)

Although Mr. Wharton Jones does not seem to have noticed the more rapid movement of blood in the part surrounding that which is actually inflamed, yet we cannot but believe that this is the ordinary condition of the circulation in such parts, among warm-blooded animals at least. For, around the proper focus of inflammation, in which we see the dusky lividity that is characteristic of stagnation, and in which we know that other characteristic changes are taking place, we find a border, whose florid redness testifies to the fulness of its vessels ; and the readiness with which these are disposed to empty themselves when their veins are opened, and the unusually large quantity of blood which is then discharged (as in Mr. Lawrence's well-known and instructive experiment), show that there is anything but stagnation of the circulating current in their tubes. Mr. Paget notices the remarkable continuance of the fluidity of the blood in the vessels, notwithstanding its retardation, or the entire cessation of its movement. “ I have found it fluid,” he says, “ after at least three days' complete stagnation, and so I believe it would remain till it is cleared away, unless the part sloughs. In the latter case, it would coagulate, as it does in carbuncles and the like, which hardly bleed when we cut them through ; but, so long as the blood is fluid, though stagnant, it may be driven from the vessels with full force, as soon as an easy exit for it is made by cutting into the inflamed part, or opening one of its veins.” This fact is obviously one of the same order with that of the continued fluidity of the blood included in a part of an artery cut off from the current of the circulation between two ligatures ; and it leads, with several others of the same order, to the belief, that the continued fluidity of the blood is dependent upon the vital condition of the parts with which it is in contact. It is from this property of the blood that the inflamed part is enabled to return to its natural state, provided the disturbing cause has not acted with too much violence ; which would not be the case if its vessels had become choked up with coagula.—The mode in which the stagnation, and the subsequent resolution, take place, is minutely described by Mr. Wharton Jones ; and we shall quote so much of his account of both, as can be made intelligible without the illustrative figures :



“ Stagnation commences in the capillaries, and extends from them to the veins on the one hand and to the arteries on the other. The mode in which it is seen to take place is by red corpuscles, more collapsed and darker-looking than natural, first adhering to the walls of the vessels, and then other red corpuscles adhering to them. The first adhesion of red corpuscles to the wall of a vessel usually takes place at a bifurcation; and in those capillaries which are least in the direct course from the artery to the vein. In those capillaries which lead most directly from the artery to the vein, and in which, consequently, *vis à tergo* operates most advantageously, the blood is latest in stagnating. It has been supposed, that the red corpuscles first agglomerate in the arteries leading to the capillaries, and that the resulting masses, on entering the capillaries, act like plugs. To these plugs more corpuscles adhere, and thus the capillaries become entirely blocked up. It will be seen below, that grayish granulous-looking plugs, composed, apparently, of colourless corpuscles and coagulated fibrin, are sometimes seen to block up an artery; and on being forced on by the stream from behind, to block up capillaries. Such plugs, however, have no share in producing the stagnation; for this takes place in the manner above described, when no plugs are to be detected. Besides, the plugs in question continue in the capillaries only for a short time, being soon disintegrated and carried away into the veins.

“ The capillaries to which a given artery leads, and also the terminal branches of that artery, having become obstructed by the stagnant red corpuscles, the blood ceases to enter the part of the trunk of the artery immediately above the place where the stagnation is, but passes off by lateral branches higher up, as in the case of section of an artery. Down to the place where the blood is stagnant in the extremities of the artery, the dilated trunk contains but a small quantity of blood-corpuscles, and these are seen collected to one side of the vessel, and oscillating up and down.

“ In the veins, the blood also becomes stagnant up to the entrance of a large branch, conveying blood from a part of the web in which the flow is still free.

“ Not many colourless corpuscles are seen in the vessels in which the blood is stagnant; but they are seen in considerable numbers in those veins in the neighbourhood in which the flow of blood is still free, though, perhaps, retarded.

“ When, by adhesion to the walls of the vessel and agglomeration with each other, the red corpuscles are arrested in their course, the plasma of the blood passes on until the vessel is completely stopped up by the stagnant red corpuscles. This goes on in one vessel after another, the plasma draining away from the vessels which have become obstructed into such as still remain pervious, and with which there is communication. In some vessels, however, all the outlets from which have become blocked up with stagnant red corpuscles, plasma is retained. Such vessels appear empty, or almost so, of blood from the absence of the corpuscles.

“ Thus it is also, that the red injection may be seen interrupted here and there, and vessels with stagnant red corpuscles in them may appear isolated.

“ The stagnant red corpuscles within the vessels are so closely agglomerated together, that they present the appearance of a uniform red mass similar to that which they present out of the body, when the blood is mixed with albumen.” (pp. 34-8.)

On the other hand, when the stagnation which is occasioned by the application of a strong solution of salt to the web becomes dissipated, which it usually is, after no long interval, in young and vigorous frogs,—

“ The flow of blood is observed to be re-established in the following manner: the agglomerated mass of red corpuscles in a given vessel, exposed to the force from behind in the artery leading to that vessel, is seen to oscillate, yielding a little at each stroke of the heart, and again recoiling. If the vessel in question has an outlet into another in which the flow of blood is free, it is seen that the agglomerated mass of red corpuscles, being at last moved onwards, protrudes into the free vessel, the stream in which detaches and carries away the corpuscles composing the

protruding part. In this manner, successive portions of the agglomerated mass are carried away, until at last, reduced to a small size, it is all driven onward by the stream from behind into the stream in front, where it is broken up into its constituent corpuscles. The blood has now free course in the obstructed vessel. In this way, one vessel after another is cleared, until the circulation is fully re-established in the web.

"The process just described may be promoted by applying to the web any of the agents above mentioned, as possessing the property of causing dilatation of the arteries, and acceleration of the flow of blood—even a solution of salt. It is to be remembered, however, that the solution of salt, for instance, if strong, may exert its property of causing dilatation of the arteries, with acceleration of the flow of blood in vain, in consequence of its tending to keep up the stagnation by virtue of its other property of abstracting water from the blood by endosmose, and thus inspissating the plasma.

"The re-establishment of the flow of blood, in the manner described, is to be attributed to the force from behind operating on the agglomerated mass of red corpuscles, now become loosened by having imbibed the thinner plasma of the blood flowing against it. The promotion of the process, by the applications referred to, is an interesting illustration of the *modus operandi* of stimulating collyria, applied to the eye, for the cure of inflammation of the conjunctiva." (pp. 49-50.)

The question now arises,—to what cause are the retardation and final stagnation of the blood to be attributed? Can it be accounted for by the condition of the vessels, by that of the blood, by that of the tissues, or by all combined?

Now, it would seem to be a sufficient reply to the first part of the question, that the rate of the blood's movement does not seem to have any relation whatever to the size of the vessel through which it moves. We have seen that the first effect of stimulating applications has been to produce contraction of the arteries; and it is commonly said, that with this contraction there is acceleration of the current of blood. Both Mr. Paget and Mr. Wharton Jones, however, agree in stating that this is not, or, at least, not usually, the case. The former says: "The stream often becomes slower as the artery or vein becomes narrower by contraction;" and the latter states that, "On the very first application of salt to the web, there is sometimes a momentary retardation: this is coincident with constriction of the arteries." But soon the constriction of the vessels gives place to dilatation, and the retarded to an accelerated movement of the blood. Now, if this were all, we should be justified in suspecting, with Mr. Wharton Jones, that a relation of cause and effect might subsist between the two phenomena; and in surmising that primary retardation in the rate of the blood's movement (where it occurs) might be set down to the coincident contraction of the vessels, and the secondary acceleration to the dilatation which succeeds, both being looked upon as purely physical phenomena. It is somewhat difficult to speak positively as to the admissibility of this hypothesis, owing to the complexity of the physical conditions of the problem. If a liquid be propelled, at a certain measured rate, into a pipe from which it has a free exit, its rate of movement through that pipe will be inversely as the sectional area of the tube. Thus it is, that in a force-pump or fire-engine, the velocity of the stream through the jet will be inversely as its size; thus it is, as every tyro knows, that the venous current is slower than the arterial, in proportion to the excess of the caliber of the veins over that of the arteries; and thus it is, as Mr. Wharton Jones has observed, that the movement of

blood is retarded in those spots in which *partial* dilatations of the vessels present themselves. So, again, if the *entire system* of small arteries were contracted at once, as in Cullen's hypothetical "spasm of the extreme vessels," we apprehend that the first effect would be a retardation in the flow of blood, caused by the increased friction of the current against the walls of the tubes; but if the heart's action were augmented in force, so as to overcome this resistance, and to send forth the same number of ounces of blood per minute that it did before, the rate of the circulation in the contracted vessels must then be augmented in proportion to the diminution of their sectional area. But the condition of the vessels of a *single part*, to which a stimulus may have been applied, is different in this,—that the fluid sent forth from the centre of impulsion is not necessitated to pass through them, as in the preceding cases, but may discharge itself through numerous other channels, with very little addition to the amount that passes through each; and, consequently, the alteration in the caliber of any one small portion of the circulating system will not have the same effect upon the rate of movement of the blood which passes through it, as it would do if a fixed measure of fluid were compelled to pass through it in a certain time. We should apprehend that, in such a case, the rate of the flow would, *ceteris paribus*, be governed in great degree by the facility of its passage; so that a constriction of the vessels would, by increasing the friction, produce a retardation in the current, and that a dilatation, within certain limits, would involve an acceleration of it. We say—within certain limits;—because the influence of friction diminishes in proportion to the increase in the caliber of the tube; and also, because the amount of blood which passes through the vessels of a part, if dependent entirely on its *vis à tergo*, must depend upon the supply which it receives through the larger vessels.

We should be willing, therefore, to admit (with Mr. Wharton Jones) that the phenomena in question might be attributable to physical causes, if they constituted the whole which we have to explain. But they are far from being so. We are satisfied that there are variations in the movement of blood in the different parts of the same capillary system, in the state of health, and still more in the state of disease, which cannot be accounted for on these hydraulic principles. No one can watch the capillary circulation, for any length of time, in the frog's foot, without seeing that the rate of the stream, *in different parts of the same plexus*, bears no proportion whatever to the caliber of the tubes. We have frequently noticed the current moving with most extraordinary velocity through a very narrow capillary, and moving very leisurely through a tube of much larger size in immediate connection with it; and after a little time, the circulation shall be seen to be retarded in the one, and quickened in the other, without any perceptible alteration in their relative diameters. So, again, after the application of stimuli to the membrane, as the state of active congestion passes into inflammation, we see the flow of blood becoming retarded, without any return of the constricted condition of the vessels, but with a tendency, on the other hand, to a further dilatation; and this retardation may proceed to absolute stagnation, without any mechanical impediment whatever being presented by a narrowing of the diameter of the tubes. The long and patient examination which Mr. Paget has made into the subject, has fully satisfied him that "the

*velocity of the stream, in any vessel of an inflamed part, is not determined by the diminution or enlargement of the channel.* Without change of size, the stream may be seen decreasing, from extreme velocity to complete stagnation." On this point we have ourselves been long satisfied, and we are glad to find Mr. Paget coming to the same conclusion. Mr. Wharton Jones attempts to explain the retardation, by attributing it to that change in the condition of the blood, which we shall presently have to notice; whereby, he affirms, the circulating fluid becomes inspissated by the loss of its serum, and the corpuscles tend to aggregate together, and to block up the vessels. But, independently of other objections to this doctrine, which we shall presently mention, how is it that the diminution in the rate of the previously-accelerated current takes place *before* that accumulation of red corpuscles so minutely described by Mr. Wharton Jones, as any one may see who attentively watches the process? It might be almost as legitimately affirmed, in our apprehension, that the accumulation of the red corpuscles, in the capillaries of an inflamed part, is the *consequence* of the retardation, as that it is its *cause*; and, perhaps, such a view would be nearer the truth. As this question, however, will be better discussed, after we have considered the changes in the condition of the blood and of the tissues, which are characteristic of the inflammatory state, we shall, for the present, content ourselves with citing Mr. Paget's judicious view of it, which indicates the direction wherein the solution is to be looked for, without any dogmatical assumption of a knowledge which has not yet been fully attained:

"On what the alteration of movement of the blood in such a case depends, I cannot tell; but we have facts enough to justify such an hypothesis as that there may be some mutual relation between the blood and its vessels, or the parts around them, which, being natural, permits the most easy transit of the blood, but, being disturbed, increases the hinderances to its passage. Such hinderances appear to be produced by the addition of salts of baryta, or of potash, to the blood: the presence of an excess of urea in the blood probably produces the like effect: and such facts as these make the hypothesis I have referred to not unreasonable. At any rate, the belief that the more or less rapidity of movement of blood through small vessels may depend on other than evident mechanical relations, cannot appear absurd to any one who has seen the movements of fluid in the Chara, or Vallisneria, or any such plants, in which a circulation is maintained without any visible source of mechanical power." (p. 9.)

Mr. Simon obviously entertains the same view. "There exists in the organism," he says (p. 65), "an arrangement for providing the elements of parts, according to their requirements, with an increased supply of developable blastema." He then goes on to show (p. 67), "that this excess of blastema in the tissue depends on an increased afflux to the tissue;" and in proceeding to inquire how this is to be accounted for, he proves most satisfactorily, that it cannot be dependent upon any *vis à tergo* derived from the action of the heart or arteries, but must be attributed to a *vis à fronte*, arising out of the molecular changes in the tissue itself. This principle has been so little attended to by physiologists and pathologists in general,—Dr. Alison and his followers being those who have most positively upheld it,—that we think it worthwhile to quote the following portions of Mr. Simon's argument, familiar though it may be to the minds of many of our readers:

"In the primordial development of parts, we are able, without difficulty, to satisfy

ourselves, that organs which subsequently become dependent on blood-vessels do at first grow quite extra-vascularly; and that the prolongation of blood-vessels into their substance (a change which, relatively to their previous state, may be called a hyperæmia) only occurs after they have reached a certain maturity: so that if we were to use terms implying the relation of cause and effect between the intimate structure of such organs and their vascularity, we might speak of the structure as determining the vascularity, but by no means of the vascularity as determining the structure; we might speak, for instance, of the ossification of cartilage occasioning its permeation by blood-vessels, since it begins to ossify before it begins to be vascular; but we could not reverse this, and speak of the structural change being determined by the greater vascularity of the cartilage. The change of molecular structure determines the change of vascular supply.

“Guided by this analogy, we are able to draw a similar inference respecting some of the hyperæmial changes which occur in secreting organs: if you remove one kidney, so that there may remain an insufficiency of glandular substance for the excretion of urine, you presently find that the blood-vessels of the remaining kidney have become larger, and that the number of glandular elements in this kidney have likewise become vastly multiplied. Now (so far as we know anything in physiology) we know that the abundance of cell-growth in an excreting organ is an immediate consequence of the presence in the blood of certain matters which it is the function of that glandular cell-growth to eliminate. We are accordingly led to consider the enlargement of blood-vessels as a secondary phenomenon; subsequent to the multiplied cell-growth; necessary to maintain it; and essentially analogous (as I have hinted) to the process by which blood-vessels were originally given to the embryonic organ, only after it had undergone the first steps of its development.

“I may take this opportunity of making two other remarks, as explanatory of our present subject. First, no variation in the *action of the heart* can account for any distributive inequalities of circulation; the heart acts as a forcing-pump for the whole body equally: it maintains all the vessels in a state of similar repletion; it can contract more forcibly, or less forcibly, than usual—can give much impetus, or little impetus, to the circulation; but what it does for one organ, it does for all; it can show no preference—can do no more for organ A than for organ B or C, any more than a change in the water-level at the New-River Head can stop the supply of No. 1, in a street, while it floods the premises of No. 2. And secondly, the property of mere *elasticity of arteries* would not account for the phenomena of unequal distribution of blood. Elasticity, no doubt, exists to a great extent in arteries, and it fulfils very important uses: by maintaining constant pressure on the blood within the large vessels, it converts the successive impulses of the heart into a continuous circulation of blood; so that when you look at the capillary circulation (in the frog’s foot, for instance), you do not see it proceeding in successive jerks at each beat of the heart, but running smoothly, evenly, continuously, and never varying. But it will be obvious to you, that elasticity can only give to arteries the faculty of adapting themselves to their contents—the faculty of exerting a counter-pressure on the blood proportionate to the heart’s pressure; and that this faculty, or rather this physical property, is not subject in its manifestations to the laws of vital excitability. No physical property akin to elasticity would account for the conjunctiva becoming red, when you put a grain of capsicum into the eye.” (pp. 67-9.)

Mr. Simon subsequently inquires whether the supply of blood to the inflamed part can be due to any unusual action in the arteries,—“a capacity for alternate closure and expansion, analogous to and synchronous with, the systole and diastole of the heart; a function by which the artery would be enabled to receive more amply, and to propel more vigorously, whatever amount of blood the local changes might require.” Some such hypothesis as this seems to be implied in the phrase “action of the vessels,” which is so currently employed in this country to designate one element, at least, of the inflammatory condition. Upon submitting it to the test



of experiment, however, he found it to be altogether invalid; and this, notwithstanding that he began his investigation with some expectation of arriving at an affirmative result:

"It was a plausible theory; it seemed accordant with the general physiology of the vascular system; it pretended to show that sort of increased effort of nutrition—that synergic activity of blood-vessels, which one was prepared to anticipate under the conditions of inflammation and hypertrophy; and, moreover, it stood as the alternative of an *à priori* improbability; for it promised to supersede the necessity for admitting that very striking anomaly in the laws of muscular contraction, to which I have already adverted as the *reflex-relaxation* of stimulated arteries.

"After many examinations, however, I have never seen anything which could give support to such a view. Again and again I have had under my eye the arteries leading to a focus of inflammatory action; sometimes I have excited the inflammation while the afferent artery has been under the microscope; sometimes the irritant has been applied several hours previously; sometimes, by ligature of the aorta, I have isolated the artery from the influence of the heart, so as better to observe its peculiar share in the phenomena; but, under none of these circumstances have I ever succeeded in observing anything like rhythmical contractions in the vessel.

"Not only have I failed to observe such contractions, but I have seen very distinctly the generation of a symptom which they have been supposed to produce, and which apparently they do not produce—I mean, throbbing. I have traced an artery, have watched it branching to inflamed and to non-inflamed parts, have seen the outlines of its muscular coat absolutely without movement, and its caliber quite uniform; but the large branch which led to the seat of inflammation, being expanded so as to oppose a minimum of resistance to the circulation, suffered the stream at each beat of the heart to strike with a direct and visible shock on the contents of the smaller and obstructed vessels which surround the area of disease." (p. 81.)

He therefore comes, partly by way of exclusion, and partly by the analogy of other physiological phenomena, to the conclusion that, in hypertrophy and in inflammation, it is the striving of the growing elements of the part—hurt by the physical violence, or affected by extremes of temperature, or thrown into rapid chemical changes, or overburdened with their own specific stimuli from the blood—to *grow more, or to grow differently*, than in their previous state; which is the real and essential cause of the derangement of the capillary circulation, the change in the caliber of the vessels being merely a coincident result of the same causative agencies. To this question, however, we shall return in the latter part of our survey; and shall conclude this division of it with the following remarks of Mr. Paget, upon the degree in which changes in the supply of blood may themselves be productive of the phenomena of inflammation; from which it will be seen, that although he considers alterations in the size or the permeability of the blood-vessels of a part, or in those other qualities which affect the performance of their normal functions—such as occur in cases of mechanical obstruction to the venous circulation,—to be capable of producing ulterior changes which simulate those of true inflammation, yet he maintains, that this imitation can only take place to a very limited extent, and that no changes in the movement of blood can of themselves determine the whole series of phenomena which constitute that condition:

"In a case of ascites from diseased heart or liver, the peritoneum often contains coagula of fibrine floating free in the serum, though no organ may present appearances of having been inflamed. In such a case, moreover, I have found the fibrin



developing itself in the form of nucleated blastema, even while floating free. In another case of mechanical dropsy, I have found the fluid of anasarca in the scrotum containing abundant lymph-corpuscles, like those in the fluid of an inflammatory effusion. Such as these are the cases through which mechanical congestions of blood connect themselves with inflammation. And if to these we add the constancy of increased vascularity among the phenomena of inflammation, they may be sufficient to make us believe, that disturbances in the circulation of a part may produce some of the principal phenomena of inflammation, even though all the other conditions of nutrition are, in the first instance, unchanged. But I know no other good evidence for the belief; and I think we should not lay much stress on these cases, since they display an imitation of only one part of the process of inflammation, namely, the production of organisable matter. The nutrition of a part with obstructed circulation suffers but a trivial disturbance, in comparison with that which would accompany an inflammation with an equal amount of hindrance to the movement of the blood. I should, therefore, be cautious of regarding these effusions in mechanical obstructions of blood as more than partial imitations of the inflammatory process. So far as the effusion in an inflammation depends on the altered mechanical relations of the blood and vessels of a part, so far may similar alterations produce effects imitating those of inflammation; but I much doubt whether any change whatever in the circulation of a part, however produced, can alone produce or alone maintain the phenomena or effects of inflammation. *I believe that the disturbances of the circulation are no more adequate to the explanation of inflammation, than the normal movements of the blood are adequate to the explanation of the ordinary process of nutrition.*" (p. 52.)

II. The next head of our inquiry relates to the *condition of the blood*, as affected in Inflammation; and here, also, two questions present themselves for separate consideration;—1st, How far may a morbid state of the blood of itself suffice to develop the other phenomena of inflammation;—and 2d, What is the state of the blood which is characteristic of the inflammatory state when developed?

a. In Mr. Paget's first course of Lectures, he pointed out, when speaking of the "purity of the blood" as an essential condition of healthy nutrition, that the physiologist and pathologist must understand, not merely such "purity" that the chemist cannot detect a wrong constituent in the fluid, or a wrong quantity of any of the normal ones; but that natural constitution of the blood, whereby it is exactly adapted to every tissue which it has to nourish, with an adaptation so exact, that chemistry cannot approach to the determination of whether it is maintained or lost. (See Vol. I of this Review, pp. 404-6). This principle must be familiar to every one who has studied the influence of morbid poisons on the blood; as we have ourselves remarked, in treating of this subject—"The fact is, that just as light, heat, and electricity often make themselves known to us by their specific effects upon organised structures, even better than by their operation upon inorganic matter, so may we regard the living animal body as a more delicate test of the presence of certain material agents, whose operation is manifested in the disturbance of its ordinary functions, than the balance and the test-glass of the most accomplished chemist can yet be accounted." (vol. ii, p. 174.) Now there can be no doubt that many inflammations have their origin in morbid conditions of the blood; which, without any other cause whatever, may determine all the other phenomena. This is most obvious with regard to those of a "specific" kind; but it is also probably true of the majority of the so-called spontaneous or constitutional, as distinguished from traumatic inflammations.

We seem, indeed, in the possession of facts which clearly establish a regular gradation between inflammatory attacks which are entirely traceable to the introduction of a poison into the blood, and those which result from purely local causes. Under the first head, we may unquestionably rank such inflammatory diseases as are producible by inoculation—the eruptive fevers, for example; and scarcely less thoroughly demonstrated, as Mr. Paget remarks, are the cases of rheumatism and gout, of lepra, psoriasis, herpes, eczema, erysipelas, and other such affections, whose constitutional nature—in other words, whose primary seat in the blood,—all readily acknowledge in practice, if not in theory. Now in all these cases, the local affections are the external signs of the general affection of the blood; and the morbid material appears to be embodied in the fluid and corpuscular products of the inflammation, and thus removed from the current of the circulation; just as the presence of an excess of urea in the blood determines the formation of additional secreting cells in the kidney, by which it is eliminated, or as certain medicines are embodied in the cells of the parts which they excite to inflammation. This selecting and searching-out of particular tissues, by particular morbid poisons, is a fact which has been prominently brought into view by the physiological pathologists of our own day; and having dilated upon it fully on the two former occasions to which we have already referred, we shall not stop to dwell further upon it here, save to notice, that Mr. Paget makes use of the same principle as the best theoretic expression of the origin of cancerous and similar tumours. “The cells or the tissues of these growths, we may believe, are formed so as to comprise or enclose specific morbid materials generated or inserted in the blood. That each such morbid material should have, for its appropriate habitation, cells and a mass of peculiar form and appearance, is only consistent with the usual rules of formation.” (p. 53, *note*.)

But we see that even in cases where the poison is of a most specific kind, and is most determinate in its mode and seat of operation, it is liable to be affected by local conditions; and thus its manifestations may be greatly influenced by the previous state of the parts, or by the local application of the poison, whose action is really, notwithstanding, of a general nature. This is clearly indicated by a number of familiar facts, which Mr. Paget has brought together with his usual aptness of illustration:

“In some instances, it is evident, that the localization of a general disease of the blood is determined by a previous condition, such as we may call a weakened or depressed condition—a state of already impaired nutrition—in some one part. For instance, suppose a stream of cold air is impelled on some part, say the shoulder, of a person disposed to rheumatism, it determines, as a more general exposure to cold might do in the same person, the rheumatic state of the blood, with all its general symptoms; but it determines, besides, the part in which that rheumatic state shall manifest itself first or alone. The depressed nutrition of the chilled shoulder makes it more liable than any other part to be the seat of inflammation excited by the diseased blood.

“Or, again, when a virus is inserted, as in all cases of poisoned wounds, the local inflammation produced by the disease with which the whole blood is infected will commonly have its seat in the wounded part. The virus must have produced some change in the place in which it was inserted, as well as in the whole mass of the blood. These cases are, probably, only examples of the general rule, that a part whose natural force of nutrition is in any way depressed, will, more than a healthy part, be liable to become the seat of chief manifestation of a general blood-disease. Thus, a part that has been the seat of former disease or injury, and that

has never recovered its vigour of nutrition, is always more liable than another to be the seat of local manifestation of blood-disease: it is, as they say, *a weak part*. Thus the old gouty or rheumatic joint is apt to receive the brunt of the new attack. And the same may happen in a more general way. A man was under my care with chronic inflammation of the synovial membrane of his knee, and general swelling about it: he was attacked with measles, and the eruption over the diseased knee was a diffused bright scarlet rash. A patient under Dr. Budd's care had smallpox soon after a fall on the nates: the pustules were thinly scattered everywhere, except in the seat of former injury, and on this they were crowded as thickly as possible. Thus, too, when a part has been injured, and it may be, is healing, a disease having begun in the blood will manifest itself in this part. Impetigo appears about blows and scratches in unhealthy children; erysipelas about the same in men with unhealthy blood. Such facts as these appear to be sufficient evidences that morbid conditions of the blood are most probably the causes of the great majority of so-called spontaneous local inflammations; of such as cannot be traced to the direct influence of any external force, but appear, rather, as having an internal origin." (p. 54.)

Such cases form the transition to those, in which the cause of the inflammatory state is purely local, and in which the blood only comes to be affected secondarily; as in inflammatory conditions brought on by the application of physical or chemical agents, by mechanical injuries, &c.—Now in all these cases, be it observed, there is necessarily a disturbance in those relations between the blood and the tissues developed at its expense, the maintenance of which is essential to the performance of the normal process of nutrition. This disturbance may be, in the first instance, either on the side of the blood, or on that of the tissues; and whichever side may be first affected, the other must speedily participate. In the case of inoculable maladies, and those other constitutional affections which we grouped together on a former occasion under the general head of "Poison Diseases," it is undoubtedly the blood which is primarily, and the tissues which are secondarily affected. On the other hand, there seems every probability that the disorder of nutrition rather commences in the tissues, in those cases in which its cause is purely local; and the more considerable the perversion, the more serious is the constitutional disturbance which speedily indicates an alteration in the qualities of the blood. And in those very numerous instances in which both causes are in operation, we find the two concurring to produce, in some particular part, that abnormal condition of the nutritive operations, which neither would have sufficed to generate alone. Thus a rheumatic diathesis may exist for some time,—as when, to use a common expression, the disease is "flying about" the patient,—and yet the poison shall not have sufficient potency to produce an attack of acute inflammation, until the vitality of some particular organ becomes depressed by cold, over-exertion, or some similar influence, which would not of itself have engendered the diseased action, had it not been for the concurrence of the morbid condition of the blood.

Thus, then, we may conclude, that the existence of a morbid condition of the blood may operate as a cause of inflammation; and this either alone, or in combination with other causes. And, further, it may be shown by further inquiry in the same direction, that the state of the blood may determine, not only the locality, but also the degree, the form, and the probable termination of the inflammation. To this topic, however, which has been ably discussed by Mr. Paget, we shall hereafter recur.

b. We have now to inquire into the state of the blood during the super-vention of the inflammatory disorder, and after its complete establishment; and we must consider under this head both the *local* and the *general* alterations which the fluid presents. The former must be considered as among the essential conditions of inflammation; and the latter as belonging rather to the category of secondary phenomena, which may or may not manifest themselves, according to circumstances. No disorder can be perceived in the general mass of the blood, unless the inflammation has acquired a certain extent and intensity; and all the local phenomena of this state may be developed without the production of any such disorder,—as where they arise from the presence of a foreign body between the two opposed surfaces of the conjunctiva. Hence we must believe, either that the changes which present themselves in the general mass of the blood are impressed upon it by its passage through the inflamed part; or that they originate in an action which commences in that inflamed part, and is propagated through the circulating system. Most assuredly may it be stated, that the changes which are distinguishable in the blood of a patient suffering under an attack of acute and extensive inflammation, are not the antecedents, but the consequents, of that condition, and cannot be looked to as affording any explanation of its local phenomena.

Of the state of the blood in the vessels of an inflamed part, we find the fullest account in Mr. Wharton Jones's 'Essay;' and as that gentleman's theory of inflammation mainly rests upon the alterations which he has described, we shall follow him somewhat in detail through his inquiries on this point. In Chapter II, "On the Circulation of the Blood in the Web of the Frog in the Healthy State," we find the following statement respecting the fact, which, from the reference afterwards made to it by the author, is evidently regarded by him as of great importance.

"If a drop of blood just drawn from the animal be examined under the microscope, the red corpuscles are seen aggregated together, lying flatways, and overlapping each other. A few may be observed here and there, raised up on edge, with their flat surfaces applied to each other. When a drop of blood just drawn from the animal is mixed with white of egg, and then examined, the red corpuscles are seen much more closely aggregated together, and in elongated masses. The greater agglomeration of the red corpuscles in this case is owing to the greater viscosity imparted to the plasma by the admixture of the white of egg. A similar degree of agglomeration of the red corpuscles is observed to take place in the blood of young frogs, and in the blood of frogs in autumn, independently of any admixture with white of egg,—a circumstance indicative of the existence of a greater quantity of albuminous or fibrinous matter in the blood." (p. 13.)

The difference between the ordinary movement of the red and of the colourless corpuscles in the circulating current is so well known, that we need not dwell upon it, and shall merely notice the fact, that whilst the red corpuscles move rapidly onwards in the middle of the stream, the colourless slide (as it were) along the walls of the vessels, against which they are frequently seen to flatten themselves,—sometimes even showing a disposition to adhere by one small point or by a larger spot, so as to have their form altered by the force of the stream against their surfaces. This tendency to adhesion, however, is generally counteracted, when the blood is flowing at its normal rate, by the impulse of the current; but as soon as this undergoes retardation, adhesion of the colourless corpuscles

takes place, and they thus accumulate in great numbers, without, however, presenting, in the first instance, any obstacle to the passage of the red corpuscles, which may still glide along without interruption in the centre of the vessel. But though the red corpuscles have no tendency to adhere to the sides of the vessels, they show a disposition to adhere to each other by their flat surfaces; and although this disposition is restrained in the normal condition by the force with which the current is propelled, yet, so soon as an impediment occurs to the free passage of the blood, the tendency to such mutual adhesion begins to show itself. We have already quoted (p. 137) Mr. Wharton Jones's account of the further changes presented by the blood, in the vessels of a part to which a strong solution of salt had been applied;—how the red corpuscles, more collapsed and darker-looking than natural, adhere to the walls of the vessels, whilst other corpuscles adhere to them, until such an aggregation of them takes place as completely to block up the vessels,—whilst the plasma drains away. And we shall now cite Mr. Wharton Jones's *rationale* of this process, which is as follows:

“It appears to me, that the adhesion of the red corpuscles to the walls of the vessels, and to each other, on which the stagnation depends, can be attributed only to a change in the state of the blood itself, produced by the action of the salt—a change, consisting in inspissation of the plasma, as regards its albuminous and fibrinous constituents. On this condition of the blood, we have seen, that the more ready agglomeration of the red corpuscles in blood out of the body depends. The mode in which the salt acts in producing inspissation of the plasma, I consider to be principally by withdrawing water from the blood by endosmose. The change in the appearance of the agglomerating corpuscles above noticed, of itself indicates a change in the composition of the plasma.” (p. 40.)

Professor Henle had previously expressed the opinion, that inspissation of the plasma is the cause of that adhesion of the red corpuscles to the walls of the vessels and to each other, on which the stagnation is thought by both to depend; but he had attributed the exudation of serum, on which the inspissation immediately depends, to the retardation of the flow of blood consequent upon the dilatation of the vessels, and to the thinning of their walls arising from their relaxation and dilatation. Against this view Mr. Wharton Jones urges some cogent arguments; none of them, however, so cogent (in our estimation) as the fact, that if Professor Henle's view were correct, there would be no essential difference between inflammation and passive congestion; and the “inspissation of the plasma” consequent upon the serous exudation which is so characteristic of that congestion, would necessarily go on to produce the phenomena of inflammation which are conceived by him to be dependent upon it. This objection seems to us to be as cogent against Mr. Wharton Jones's theory as against Professor Henle's; for if “inspissation of the plasma” in the vessels, produced by an exosmose of serum, be the essential condition of inflammation, how is it that the inflammatory state is *not* uniformly generated by the causes that produce an exudation of serum, and how does it happen, on the other hand, that it is generated by many causes (such as mechanical injuries, or the presence of morbid matters in the blood), which have no tendency to produce any such exudation, until the other signs of inflammation have already begun to manifest themselves? In so far, then, as Mr. Wharton Jones's theory of inflammation rests upon this as its fundamental element, we



have no hesitation in regarding it as unsound; and we do not think that it will be difficult to show its inconsistency with itself. In fact, as will presently appear, when Mr. Wharton Jones comes to apply his observations on the frog's foot to explain the phenomena of inflammation in man, he himself virtually abandons it.

It is well known, that the observations of Mr. Addison and Dr. C. J. Williams led those gentlemen, as well as some others who adopted their views to a greater or less extent, to the belief, that the adhesion of the *colourless* corpuscles to the walls of the vessels has a special share in producing the stagnation of the current; and their accumulation in the vessels of an inflamed part has been looked upon as explicative of some of the other phenomena of inflammation. According to Mr. Wharton Jones, however, "the colourless corpuscles contribute nothing" to the congestion and stagnation of blood consequent upon the application of a solution of salt to the web of the frog. Yet immediately after making this statement, he proceeds to detail the following observations, which appear to us directly antagonistic of it:

"Sometimes, however, we see an artery become blocked up by a mass composed apparently of colourless corpuscles and fibrin. In a web, in the vessels of which stagnation of the blood had been produced by the action of salt, an artery, in which the blood had continued to flow freely, suddenly became stopped up by a plug of a gray granulous substance, so that the blood was arrested in its course, and passed off by the first considerable branch above the obstruction. Into the artery below the obstruction, the blood flowed in a retrograde direction by one set of branches, and passed out in a direct course by another set of branches, as in the case of section of an artery. By-and-by the plug was pushed along in the artery by the force from behind, and the flow of blood was re-established down to the first considerable branch above the place, where the plug again stopped. A portion of the plug of granulous substance becoming detached and carried away, the mass was reduced in size, so that it admitted of being again forced on. It next caused obstruction in a capillary; but while it was in the act of being broken up and carried away in the stream in fragments, the struggles of the animal interrupted the observation. In another case, one in which a large portion of the web had sloughed away from inflammation, excited two or three days before, I several times saw, while watching the flow of blood in the remaining part of the web, capillaries suddenly become filled with a gray granulous-looking substance. This, for a time, obstructed the vessel; but it was always at last moved on by *vis à tergo*, and getting into a vein was carried away. In other cases, I have seen similar masses adhering to the wall of the vessel, but not entirely stopping it up. In some cases I have been able to satisfy myself, that the plugs of gray granulous substance consisted of colourless corpuscles, agglomerated and held together by a tenacious looking matter, probably coagulated fibrin. In other cases, it appeared to me, that the gray substance consisted of minute granules, held together by the tenacious matter." (pp. 42-3.)

And he then adds (in a note) the following:

"I have found, that on pressing the web over an artery or vein—a large vein especially—pretty firmly with a blunt point, an agglomeration of colourless corpuscles with a few red ones, held together, apparently, by coagulated fibrin, occurs, adheres to the wall of the vessel, and more or less completely obstructs it at the place. Young healthy frogs are the best subjects for the experiment." (p. 43.)

This last phenomena would have seemed to us one of typical value; the disturbing cause being here much more nearly akin to those which produce local inflammations in Man, as that of the conjunctiva from the presence of an irritating particle, than is Mr. Wharton Jones's favorite application



of solution of salt; but we are warned by Mr. Paget against resting too much on observations made upon the colourless corpuscles of frogs:

“In many frogs, especially in those that are young, or sickly, or ill-fed, the white corpuscles are abundant in the blood: they are rudimental blood-cells, such as may have been formed in the lymph or chyle; and in these cases they are either increasing quickly in adaptation to quick growth, or else increasing because, through disease or defective nutriment, although their production is not hindered, yet their development into the perfect red blood-cells cannot take place. In either case, their peculiar adhesiveness making them apt to stick to the walls of the blood-vessels, they may accumulate in a part in which the vessels are injured or the circulation is slow, and thus they may sometimes augment the hinderances to the free movement of the blood. But I believe nothing of the kind happens in older or more healthy frogs, or in any ordinary inflammation in the warm-blooded animals. I have drawn blood from the vessels in the inflamed bat's wing, in which it was quite stagnant, and have found not more than one white corpuscle to 5000 red ones. I have often examined the human blood in the vessels of inflamed parts after death, and have found no more white corpuscles in them than in those of other parts. In blood drawn from inflamed parts during life, I have found only the same proportion of white corpuscles in them as in the healthy parts of the same person. I therefore cannot but accord with the opinion often expressed by Mr. Wharton Jones and Dr. Hughes Bennett, that an especial abundance of white corpuscles, *i. e.* of rudimental blood-cells, in the vessels of an inflamed part, is neither a constant nor even a frequent occurrence; and I believe that, when such corpuscles are numerous in an inflamed part, it is only when they are abundant in the whole mass of the blood. Now, as already stated, they are thus abundant in some cases of inflammation, especially, I think, in those occurring in people that are in weak health, and in the tuberculous; but, even in these cases, I have never seen an instance in which they were present in nearly sufficient quantity to add materially to the obstruction of the blood in the inflamed part, nor one in which any influence of theirs could be suspected to alter peculiarly the constitution of the blood therein.” (pp. 10-11.)

Mr. Paget does not admit, however, that the disposition to adhesion between the *red* corpuscles, observed by Mr. Wharton Jones in inflammatory blood, can be held to account for the stagnation:

“I have seen nothing of the kind in either the inflamed bat's wing or in the vessels of inflamed organs examined after death. When the blood is not stagnant, the corpuscles are indeed closely crowded, but they are not clustered, nor do they appear adherent: neither does such clustering appear even in stagnant blood; the change here appears to be a diffusion of the colouring matter, so that the outlines of individual blood-cells cannot be seen, and all the contents of the vessel present an uniform bright carmine tint.” (p. 11.)

And he concludes that nothing which the microscope or chemistry has yet discovered, can give any assistance in the explanation of the changes which the blood undergoes in inflamed parts. With this opinion we are ourselves inclined, in great degree, to accord; but not so Mr. Wharton Jones, who attributes to the change in the caliber of the vessels a far greater concern in the production of the phenomena of inflammation, than we can think it capable of possessing. In his observations on the frog's foot, he occasionally remarked a condition of the web, in which the arteries are just wide enough to allow the blood to flow, though with difficulty; and in which, in consequence of the deficiency of *vis à tergo*, there was accumulation of red corpuscles in the capillaries and veins, with here and there stagnation. In such cases, on the application of any of the agents already mentioned as capable of producing primary dilatation

of the arteries, the caliber of the tubes is greatly enlarged, and the circulation becomes brisk. This phenomenon he applies after the following fashion, to explain the inflammation excited in a part of the human body by the direct action of cold upon it.

“ We have seen, that the effect of the direct action of cold on the small arteries of a part, is vital contraction of their walls and constriction of their caliber, and we have seen that when all the arteries of a part are more or less constricted, there is sluggish flow of blood, with great accumulation of red corpuscles in the capillaries and veins, and here and there stagnation. A similar state of the blood and the blood-vessels to that here mentioned, is the only possible one which we can conceive as the first step to inflammation of a part of the human body from the direct action of cold on it. The blueish redness of the affected part indicates great accumulation of blood in the venous radicles,—a state which, under the circumstances, can be owing only to constriction of the small arteries, whereby *vis à tergo* is diminished, and blood thus allowed to regurgitate into, and accumulate in, the small veins and capillaries.” (p. 54.)

Now we cannot but think it rather dogmatic in Mr. Wharton Jones to assert that “ a similar state of the blood and the blood-vessels to that here mentioned, is the *only possible* one which we can conceive as the first step to inflammation under such circumstances.” Has it never entered into Mr. Wharton Jones’s mind to conceive that the action of cold upon a part is to depress its vital powers; and that the alteration of the circulation through it may be a consequence, not a cause, of such depression, to which all the subsequent phenomena of inflammation may be more or less directly referable? We are quite sure, that in looking to simple mechanical causes, as Mr. Wharton Jones would have us do, we shall make but very little progress in our explanation of the phenomena; our attention must be fixed on the forces of *life*, on the way they are affected by the various agents whose effects we witness, and on the mode in which their disturbed action manifests itself. We are somewhat anticipating, however, the remarks which we shall subsequently have to offer. Mr. Wharton Jones supposes that when an organ becomes inflamed through the action of cold upon some other part, it is by a constriction of the arteries, brought about by a reflex nervous impression, which may be expected to act with the greatest potency on the vessels of a “ weak” part. And he then continues :

“ When relaxation and dilatation of the arteries take place, the acceleration of the flow of blood in them may overcome the congestion and stagnation in the capillaries and venous radicles; in which case there will be resolution of the inflammation. But this failing to be accomplished, the dilatation of the arteries will lead to aggravation of the inflammation, by allowing a greater quantity of blood to be poured into the vessels of the affected part than can escape, in consequence of the pre-existing obstruction from stagnation. This can be directly observed, in inflammation of the web of the frog from a wound.” (pp. 55-6.)

So when inflammation is excited in the conjunctiva by a particle of sand in the eye, it is through a constriction of the vessels produced by the mechanical irritation; which here probably acts directly, in Mr. Wharton Jones’s opinion, rather than by a reflex channel.—Now, on all this we have to remark, that whatever may be the *modus operandi* of the production of the stagnation in question, such stagnation does not of itself constitute inflammation; as is proved by the very fact, that it can be immediately dissipated by an agent which simply produces dilatation of the arteries.

We should rather compare it to that state of venous congestion, in which the venous current is stagnated, not by a constriction of the arteries, but by an undue dilatation of their own caliber, consequent upon the deficient tonicity of their walls; the balance between the arterial and venous circulations being equally liable to disturbance from an abnormal diminution in the caliber of the arteries, and from an excessive dilatation of that of the veins. According to Mr. Wharton Jones, however, it passes into the state of inflammation in the following manner:

“In the vessels of an inflamed part, the quantity of red corpuscles of the blood is augmented, and that of the plasma is diminished. That the plasma is inspissated, as regards its fibrinous constituent, may be inferred from the circumstance, that the red corpuscles are agglomerated together, in a manner similar to that in which we see them in blood out of the body when the plasma is so inspissated. Another ground for inferring that the plasma is inspissated is, that the agglomeration of the red corpuscles takes place under circumstances, when it is known that water has been abstracted from the blood in the affected part.

“The increase in the quantity of red corpuscles in the vessels of an inflamed part, and the diminution in the quantity of plasma, are owing to the red corpuscles being retained in the vessels in the manner already explained, while the plasma passes on. The inspissation of the plasma, which is owing to the abstraction of water from the blood in the part, by the means immediately again to be referred to, may be quite independent of any inspissation of the plasma of the blood of the body generally, as will be more fully considered below.

“Local inspissation of the plasma of the blood with its consequences, congestion and stagnation of the red corpuscles, we have seen to be readily produced in the frog’s web, by the application of a strong solution of common salt, which abstracts water from the blood by endosmose. Local inspissation of the plasma, again, we have seen to be produced in the following manner:—in consequence of a wound there is an impediment to the flow of blood in the vessels implicated; accumulation of blood, therefore, takes place in them; and, in consequence of this accumulation, serum is exuded more copiously than under ordinary circumstances. In a case in which, in consequence of a wound, there is an impediment to the flow of blood in the vessels implicated, if a solution of salt be applied to the place, inspissation of the plasma is more rapidly and effectually produced, and hence the great readiness with which, as above shown, stagnation takes place under such circumstances. Again, in a case in which, in consequence of a wound, there is an impediment to the flow of blood in the vessels implicated, exposure of the part to a drying air causes local inspissation, and so promotes stagnation.” (pp. 56-7.)

Now here we have a condition apparently the very reverse of that described as resulting from the application of a solution of salt to the web of the frog. In that case, as our readers will see by the quotation of Mr. Wharton Jones’s own words (p. 147), he ascribes “the adhesion of the red corpuscles to the walls of the vessels, and to each other, on which the stagnation depends,” to an inspissation of the plasma produced by the endosmotic action of the salt, which withdraws the water from the vessels. Yet he now tells us (p. 59), that in the experiments and observations on the frog, it has been seen, that “exudation does not precede, but follows congestion and stagnation;” and endeavours to show that constriction of the arteries may of itself be a sufficient cause of stagnation, inspissation of the plasma, and exudation.

Such are the inconsistencies in which, as it appears to us, any theorist, however ingenious, must necessarily fall, who looks to the state of the vessels, and to the motion of the blood in them, as the fundamental con-

ditions of the inflammatory process. We do not cavil at Mr. Wharton Jones's statements of fact; we are content to take every statement he makes, as that which his eyes have actually witnessed; but we maintain that his interpretations of those facts cannot be relied upon, because they are inconsistent with themselves, and are insufficient to explain other phenomena of which he takes no notice. With him, a stagnation of blood in a part, accumulation of the corpuscles in its vessels, and serous effusion into the surrounding tissue, constitute all the essential phenomena of inflammation. But if he had been able to give a definite and consistent rationale of even these changes, we should still have said that we are as far as we were before from comprehending the nature of true inflammation,—a condition in which the nutrition of the tissue is perverted, in a way that no stagnation of the corpuscles and inspissation of the plasma will in the least account for.

b. From this long but indispensable discussion, we pass on to the second question relating to the condition of the blood in inflammation; namely, the alteration which its entire mass undergoes. On this topic we do not find that either of our authors presents us with any novel facts. Mr. Paget, however, warns us against placing too implicit a reliance upon the current statements as to the increase in the proportion of fibrin:

“Among the changes observed in inflammatory blood, the principal one—namely, the supposed increase of fibrin—is ambiguous: it may be at once an increase of fibrin and of the white corpuscles of the blood. These two constituents of the blood, the fibrin and the white or rudimental corpuscles, cannot be well separated by any process yet invented; and in all the estimates of fibrin, whether in health or in disease, the weight of the white corpuscles is included. Now, in many inflammations, these corpuscles are increased, and we have no means of clearly ascertaining how much of an apparent increase of fibrin is really such, and how much is due to the corpuscles entangled in the fibrin. Till this can be settled, I think we may not deduce any of the local phenomena of inflammation from the increase of fibrin in the blood; neither, more assuredly, can we trace, as some do, the fever and other general signs of inflammation to the abstraction of fibrin and albumen by the exudation from the blood.\* (p. 10.)

We find Mr. Wharton Jones abandoning the opinion formerly expressed by him, that a relation exists between the red corpuscles and the fibrin, similar to that which exists between the endogenous cells of a gland and its special secretion; and that the diminution in the proportion of red corpuscles in inflammatory blood is due to their melting down into fibrin. He takes care to inform us, however, that there are, in his opinion, “still less grounds for believing in any relation between the increase of the fibrin and that of the colourless corpuscles; but those who agree in the opinion that the red corpuscles are developed from the colourless, will, perhaps, recognise a relation between the diminution in the quantity of red corpuscles, and the tendency to the increase in the quantity of the colourless corpuscles.” (p. 68).

In Mr. Simon's remarks upon this subject, however, we find material for

\* “The whole of this part of the current pathology of inflammation seems to have been too hastily constructed. The local changes observed in the frog have been used to explain the chemical changes of the blood in man, although no such changes have been proved in the blood of the frog; and the changes in the blood of man have been used to explain in him the existence of local phenomena, which are assumed, but have never been proved, to be similar to those observed in the frog.”

special comment ; this gentleman having, to our great surprise, adopted what we must call the strange hypothesis of Dr. Zimmerman, that the fibrin of the blood is one of those elements of the circulating fluid “ which have arisen in it from its own decay, or have reverted to it from the waste of the tissues ;” instead of being, as most physiologists regard it, “ that ingredient of the blood which, in the ascending scale of development, stands next for appropriation into the living textures of the body, and which represents the ripeness, and perfection, and nutritiveness of the blood.” Had this doctrine been propounded by any one whose opinions on such a subject were entitled to less weight than Mr. Simon’s, we should not have thought it requisite to stop to notice it ; but put forth, as it is, in the midst of a course of lectures, whose general character is so high as undoubtedly to attract attention to it, we shall feel it necessary on a future occasion to enter upon a careful examination of the arguments which have induced Mr. Simon to adopt it. At present, we must confine ourselves to a summary of the affirmative grounds which appear to us most satisfactory for continuing to hold the doctrine, that the fibrin is the element of the blood most directly concerned in the nutritive operations.

The fibrin occupies an intermediate grade between albuminous matter and the solid tissues. It has the chemical composition of albumen ; but whilst the latter is a mere chemical compound, totally destitute of vital endowments, the fibrin, in its disposition to pass spontaneously into an imperfect form of solid tissue, after being removed from the vessels within which it circulates, shows itself to be possessed of something more than physical or chemical properties. Holding this intermediate character, we must regard it as either in a state of *progression* from the albuminous “ raw material” towards the living tissues, or in a state of *retrogression* from the latter towards the former. Now if we look, on the one hand, to the gradual development of the fibrin in chyle, in its progress through the absorbent to the sanguiferous system ; and if, on the other hand, we compare the condition of fibrin with that of the *known* products of the disintegration of the tissues, such as urea, kreatine, or extractive matters ; we cannot feel the slightest doubt that the fibrinous condition is a stage in the onward course of the nutrient material, and not in that retrograde metamorphosis of the effete matters, which we see to be everywhere tending to the reproduction of the conditions of chemistry and physics, to the utter extinction of those of vitality.—Now even Mr. Simon will probably allow, that the fibrillation of fibrin is a result of its vital endowments ; and we would ask him whether any such endowments can be shown to persist in any substance whatever, which is an undoubted product of disintegration. All the objections which he adduces in the latter part of the extracts we have cited, to the idea that fibrin is ever developed into tissues of high organisation, are very easily removed, when the facts are fairly considered. We do not hold the extravagant doctrine that fibrin is capable, by its own inherent endowments, of passing into the higher forms of organisation ; or of developing blood-vessels, nerves, or other special tissues, apart from the influence of a living organism. The coagulum of fibrin, in blood drawn from the body, presents a very imperfect approach towards the fibrillation of a clot of small size effused



within the body, still less towards that of a fibrinous exudation poured out in a thin film upon the surface of a living membrane, or infiltrated into the interstices of a tissue. The further the particles of any such coagulum are removed from a living surface, the more do they correspond with those of a coagulum formed out of the body; and this is why the inner portion of the coagulum of an aneurismal sac does not display the organisation which is presented by the outer. It is admitted on all hands, that a low and simple form of fibrous tissue may be produced by the mere coagulation of fibrin; and this, as we believe, can only undergo development into higher textures, when in such immediate proximity with a living surface as to be able to receive vessels from it. But albumen shows not the slightest vestige of such a tendency to organisation; and surely it is more probable, that a substance whose plasticity is thus manifested by its own efforts to assume an organised character, should be the pabulum for the growth of cells, &c., in which higher tissues are formed, than that they should make use of a material which does not exhibit any plastic properties whatever, so long as it remains in the condition in which it has been first introduced into the body.—Further, we may remark, that it appears from the observations of Mr. Paget upon inflammatory effusions (an account of which we must postpone to another occasion), that the strongly fibrinous exudations unquestionably rank higher in organic character than those in which cell-formation is more abundant; the latter being characteristic of tuberculous and other cachectic conditions, whilst the former mark the healthy state; and the latter having a tendency to degenerate, whilst the former are disposed to become more completely organised. Again, if the doctrine of Zimmerman and Simon be true, we should expect that the proportion of fibrin would be greater in venous than in arterial blood; whereas, if there be any difference, it is on the contrary side. And again, in diseases which are characterised by a peculiar tendency to disintegration of the solid tissues, such as idiopathic fevers, scurvy, &c., we should expect to find a marked increase in the proportion of the fibrin of the blood; whereas, so far is this from being the case, that unless an intercurrent inflammation be present, the proportion of fibrin in these diseases is below the usual average, and its coagulation is less perfect than in health.

Reviewing our whole inquiry into the alteration of the blood in inflammation, we may briefly sum up its results, by saying, *first*, that there is ample evidence that the presence of a poison in the blood, which disturbs its normal nutritive relations to a particular tissue, may of itself be a sufficient *cause* of inflammation in that tissue; that this cause acts, however, with more potency, when the vitality of the part has been previously depressed by local causes, so that the influence of the poison may be thus caused to expend itself chiefly or solely upon such part; and that there are many cases in which inflammation is produced by local causes, which cannot be conceived to act primarily on the blood: and *secondly*, that the alterations in the general mass of the blood, which are commonly accounted to be characteristic of inflammation, (such as the increase in the proportion of its fibrin, colourless corpuscles, and water, and the diminution in the proportion of its red corpuscles, albumen, and salts,) cannot be looked upon

in any other light than as *results* of the establishment of the inflammatory condition—constituting, in fact, that series of changes in the circulating fluid, which is of parallel order to that which occurs in the solid tissues wherein the inflammatory action has been set up.

III. We now proceed to the next subject which presents itself for our consideration—*The participation of nervous influence in the phenomena of inflammation*. This need not detain us long. In our review of Mr. Paget's lectures on Nutrition (Vol. I, p. 410), we expressed our concurrence in the doctrine of those who maintain, that whilst the acts of nutrition and secretion are in themselves independent of nervous influence, yet that they are subjected to that influence, especially in the higher vertebrata; so that it is probably through the agency of the nervous system, that much of the regularity and consentaneousness which characterise the healthful operation of these processes is due; whilst from an abnormal condition of the nervous influence, whether by deficiency, excess, or depravation, various kinds of disturbance of the formative operations may arise. We fully agree with Mr. Paget, that this influence is exercised, "not merely in affecting or governing the size of the vessels of the part, but with a more direct agency, as being one of the forces that concur in the performance of the plastic act;" for whilst an affection of the size of the vessels might modify the *quantity* of a secretion, no such affection could influence its *quality*; and there seems to us ample evidence that the latter, as well as the former, is liable to modification through the nervous system. We cannot but wonder, with Mr. Paget, "at the steadfastness with which some maintain, or imply, that the nervous force can manifest itself in nothing but impressions on the mind, and muscular contraction-force. So limited a view of the convertibility of nervous force is such a one as the older electricians would have held, had they maintained that the only possible manifestations of electricity were the attractions and repulsions of light bodies, or that the electric force could never be made to appear in the form of magnetism, of chemical action, or of heat." As we pointed out, on a former occasion, all the evidence of the mutual convertibility of the physical forces, and of the relation of the nervous force to these, might lead us to anticipate a like variety in the modes of operation of the nervous force upon living structures; and when it is borne in mind, that the nervous force is itself a product of a certain class of nutritive operations (of which it may be considered as the highest result), we seem fully justified in the expectation, that it should influence the nutritive acts elsewhere—just as the electric current, set in motion by certain chemical reactions, is capable of influencing the chemical reactions of other substances subjected to its agency.

We seem fully justified in the anticipation, therefore, that as the normal operations of nutrition are regulated in some degree by nervous influence,—and this, probably, to the greatest extent in the highest animals, in which the nervous system possesses the most marked predominance,—that abnormal form of nutrition to which we give the name of inflammation, should be sometimes traceable to a disturbance of innervation. And this we may consistently hold, without at all going the length of those who assert, that such disturbance of innervation is one of the essential or fundamental conditions of inflammation; still more, without assenting to

the doctrine, that a disturbance in the usual regulation of the caliber of the blood-vessels is *the one* essential or fundamental condition, to which all the other phenomena are traceable. This question has engaged the attention of each of the authors whose views we are endeavouring to bring into comparison; and we find a close accordance in their conclusions with regard to it. Mr. Paget argues that, from the absence of any constant relation between that "excited" state of the nervous force which is felt as pain and the intensity of the inflammatory process, there may be some other state by which the nervous force is more intimately connected with the inflammatory process—a state of disturbance, which may, indeed, be felt as pain, but which more properly affects the influence of the nervous force in the process of nutrition. Of the excitement of the inflammatory state by painful nervous impressions, there is no want of examples. Thus, as Mr. Paget remarks, "in paroxysms of neuralgia, we see sometimes a transient redness or œdema of the part; so, when a more abiding pain has been excited by sympathy with some inflamed part, there may presently supervene the more palpable effects of inflammation." Now we go further, and maintain that such an excitement of the sub-inflammatory condition in the surrounding parts often seems to relieve the neuralgic condition. It has happened to us to experience in ourselves, and to witness in others, on a great number of occasions, that form of "face-ache," which, beginning in neuralgia (brought on by cold, with perhaps some complication from decayed teeth), ends in swelling; and we have invariably noticed, that as soon as the heat, tenderness of the surface, and œdematous effusion are perceptible, the neuralgic pain abates. "The simplest," as Mr. Paget justly remarks, "may be the most proving instances;" and we shall now cite one, which he adduces from his own experience, in proof of the position, that the transmission of nervous influence, exciting inflammation in a remote part, may be altogether independent of *pain*.

"Whoever has worked much with microscopes may have been conscious of some amount of inflammation of the conjunctiva in consequence of over-work. Now the stimulus exciting this inflammation has been directly applied to the retina alone; and I have often had a slightly inflamed left conjunctiva, after long working with the right eye, while the left eye has been all the time closed. I know not how such an inflammation of the conjunctiva can be explained, except on the supposition, that the excited state of the optic nerve is transferred or communicated to the filaments of the nerves of the conjunctiva, generating in them such a state as interferes with its nutrition. It is true that in these simple cases the retina is not itself evidently inflamed; but after yet severer stimulus it commonly is so, and in these the conjunctiva shares in the evil effects of the communicated stimulus,—effects which we cannot ascribe to any alteration in the blood, or the size of the blood-vessels." (p. 12.)

In the following case, the pain must obviously have been only the manifestation of the disturbed state of the nervous system, to which the production of the inflammation was due:

"I may mention another case: the occurrence of inflammation of the testicle in cases of severe irritation of the urethra. The most unexceptionable cases of the kind are those in which irritation is produced by a calculus impacted in a healthy urethra. I have here a specimen in which extensive deposits of lymph and pus are seen in the testicle of a man in whose urethra a portion of calculus was impacted after lithotomy. Here is such an inflammation as we cannot refer to disease of the blood, and attended by such changes as we cannot explain by any enlarge-

ment or paralysis of the blood-vessels: nor do I know how it can be at all explained, except by the disturbance of the exercise of the nervous force in the testicle, which disturbance is excited by transference from the morbidly affected nerves of the primary seat of irritation." (p. 12.)

Mr. Simon adduces arguments of the same kind, from the disorders of nutrition which arise as consequences of disease or injury, operating on the trunks of sensitive nerves. We are sorry to have to take exception to the mode in which this very important topic is introduced; but we feel that too much caution cannot be employed in the use of language which shall have the least tendency to lower the reverence due to the All-Wise Creator. "It almost seems, in such cases," says Mr. Simon, "as though Nature formed a misapprehension of the state of the part, in consequence of wrong intelligence being transmitted through the diseased nerve." After adverting to the well-known fact, that impressions made on the trunks of sensitive nerves are referred to their peripheral extremities, he continues:—"Gradually we become accustomed to this illusion; we recognise it as a misapprehension inseparable from the arrangements of our nervous anatomy. *Yet, strange to say, Nature will sometimes act on that misapprehension*; and the distant uninjured part will become the seat of hyperæmia." Mr. Simon would, doubtless, consider himself vastly superior to those elder pathologists, who considered "monstrosities" as proceeding from Nature's freaks, or bungling performances; yet in what respect, we should like to know, is his explanation preferable to theirs? If he should reply that he did not mean, in sober seriousness, to charge Nature with making a mistake, but that he only intended to render his illustration more pointed, we would beg of him to consider whether language of this kind is not likely to do a great deal more harm than good, and whether it would not be much more philosophical, as well as more prudent and decorous, to seek for the interpretation of the fact in the general laws of the vital economy.—The cases to which Mr. Simon refers are very apposite:

"Several years ago I was consulted, at King's College Hospital, by a man who some months previously had torn his ulnar nerve at the inner condyle; his *two inner fingers had become swollen and livid with vascular injection*. Two years ago, I had a female patient here with disease in a large portion of the lumbar and sciatic plexus of nerves on one side, causing paralysis and anæsthesia of the limb; neuralgia was referred especially to the vicinity of the knee; and at this spot (when I first saw the patient) *ulceration had occurred*. It has long been known, that injuries or diseases of the ophthalmic division of the fifth nerve lead to alteration in the nourishment of parts where the radicles of that branch arise—to injection of the conjunctiva, to opacity and subsequent ulceration of the cornea, and often to final destruction of the globe of the eye. In a case of the kind, which I saw with Mr. Dixon some years ago (and which he has reported in a very instructive form in the 'Medico-Chirurgical Transactions' for 1845), there was likewise *ulceration at the cutaneous source of the nerve*, at the inner canthus of the eye, and at the ala nasi. Similarly, it is known that the nutrition of the lung is gravely affected by injuries or diseases of its sensitive nerve, the pneumogastric; experimental division or bruising of these nerves (if the animal survive the operation) never fails to induce true pneumonia—that is to say, not mere congestion of the lungs, but the development of new products." (pp. 74-5.)

The limits of Mr. Wharton Jones's inquiry do not admit of his giving a prominent place to the discussion of this question; and the facts he adduces as the result of his experiments bear rather upon our next posi-

tion,—that although the state of inflammation may be excited by disordered innervation, or may be greatly influenced by it when excited by other causes, yet that such disorder of innervation is by no means an essential condition of the inflammatory state, as maintained by many distinguished pathologists. Considering that *pain* is one of the four phenomena enumerated in the ancient and approved definition of that state, it is not surprising that an “excited” condition of the nerves of the part should have been commonly supposed to be one of its constituent elements; but we must own that it has greatly surprised us to find, that what appears to us so very untenable a form of this theory, as that not long since put forth by Henle in his ‘*Handbuch der rationellen Pathologie*,’ should have found favour with many of the most learned pathologists of Germany, and should have even gained the approval of Rokitansky, *facile princeps* as he is universally admitted to be among them all. The theory of inflammation, known as the *neuropathic*, or *neuro-pathological*, may be thus stated:—Inflammation, whether arising from an external cause, such as an injury to a part, or from an internal one, as diseased blood, really depends upon an affection of the centripetal, or afferent nerves of the part; this affection is of the nature of an “excited” state; and being conveyed to some nervous centre, is thence reflected on the centrifugal or motor nerves of the blood-vessels of the same, or of some other related part. This reflection, however, is supposed to operate, not (as might be expected) in producing contraction of the muscular fibres of the blood-vessels; but by paralysing these, so that the muscular walls of the blood-vessels relax, and they undergo dilatation. This dilatation being established, the exudation, and other phenomena of inflammation, are assumed to follow as natural, and most of them as mechanical, consequences. It is also considered, by those who hold this doctrine, that the same changes in the peripheral circulation may take place, independently of local impressions, from primary changes in the nervous centres;—just as convulsive movements may originate, either in reflex irritation, or in central disturbance. To this theory Mr. Paget brings the following cogent objections:

“The eminence of those who have supported this hypothesis makes one hesitate in rejecting it; and yet I cannot help believing it to be groundless. If we remember that parts may present some of the phenomena of inflammation, though they have no nerves, as the firmest tendons and articular cartilages; that the degrees of inflammation in parts bear no proportion to the amounts of pain in them when inflamed; that the severest pains may endure for very long periods with only trivial, if any, phenomena of inflammation; that the phenomena of the so-called reflex paralysis are rare, equivocal, and altogether insufficient for the foundation of a law or general principle; we may well think that there can be no sufficient ground for the invention of such an hypothesis as this. And, if we add that, even admitting the dilatation of blood-vessels as a possible consequence of the stimulus of sensitive nerves, yet the phenomena of even simple inflammation would be no necessary consequences thereof; that the varieties of inflammations would be quite unintelligible as results of similar mechanical disturbances of the circulation; and that the dilatation of blood-vessels, in any mechanical way produced, is followed by only feeble imitations of a part of the inflammatory process; then we may think that the hypothesis, if all its postulates be granted, will yet be insufficient for the explanation of the facts.” (p. 55.)

It seems to be quite forgotten by the upholders of this doctrine, that an



injury which affects the nerves of a part acts also on its other tissues ; and there can be no adequate ground for singling out the nerves as the sole, or even as the special recipients of its effects. The question may be in some degree tested by experiment, and by pathological observation ; and we find in Mr. Simon's Lectures some valuable facts bearing upon it :

" It is notorious, that paralysed parts are susceptible of artificial irritation, and become inflamed. I have made many observations on the subject, and have noticed that the absence of a spinal cord, or the division of all the roots of the nerves, or the section of the lumbar and sciatic plexus, will make little or no difference, as to the certainty with which an irritant applied to the web of a frog's foot will quicken the circulation there, and subsequently lead to its retardation and arrest.

" Perhaps the neuro-pathologists might object here, that we do not accurately know what are the branches of nerves—and still less, what are the nervous centres, determining muscular movements in the arteries ; and that, in the experiments just cited, some essential element for the nutritive innervation of the part (*i. e.*, for the supposed function of its artery) might have been left uninjured.

" There is, however, little room for fallacy in the experiment which I am now going to tell you. A patient had complete anæsthesia of the fifth nerve, dependent (as a post-mortem examination subsequently showed) on its organic disease ; the conjunctiva, as well as the integument of the face, was utterly insensible ; not only was the function of the nerve destroyed, but those reflected nutritive changes of which I have already spoken had taken place, and had exhausted themselves ; showing that the nerve was spoilt for participation in the acts of nutrition (whatever they may be) no less than for its more obvious uses as a medium of conscious sensation : the cornea had undergone ulceration, and had healed again. The following experiment was carefully made :—the lids being held open, a single granule of cayenne pepper was laid upon the insensible conjunctiva ; in a few moments it had become the centre of a very distinct circle of increased vascularity, the redness of which slowly became more and more distinct as long as the stimulus was suffered to remain ; so that, on its removal, there was a very evident circumscribed erythema on the surface of the membrane. I consider myself justified in believing that this change occurred without any intermediate nervous excitement ; not only because the history of the case would lead me to consider the fifth nerve as annihilated ; not only because the experiment was totally unattended with sensation ; but likewise because there was the very remarkable absence of that sympathetic phenomenon, which the faintest remnant of nervous excitability would have produced—namely, there was not the slightest trace of lacrymation." (pp. 75-6.)

The attention of Mr. Wharton Jones, also, has been directed to this point ; and though his experiments seem to have been less complete than those of Mr. Simon, in so far as he contented himself with division of the nerve-trunks instead of removing the spinal cord, yet they are very conclusive in showing that the phenomena of inflammation cannot be attributed to the cause assigned by Prof. Henle.

" The dilatation of the arteries, and the coincident acceleration of the flow of blood, have not, when the vessels are otherwise in a natural state, any share in bringing about the subsequent stagnation. This is proved by the fact, that in frogs we may sometimes observe the arteries very much dilated, and the blood flowing freely, without any tendency to stagnation ; whilst, on the contrary, there is a tendency to stagnation in the capillaries when the arteries are constricted. The same thing can be more strikingly demonstrated by experiment. We have above seen, that after section of the ischiatic nerve, the arteries of the web of the same side are found somewhat more dilated, and the flow of blood in them freer and more rapid than in the web of the uninjured side. If, after section of the ischiatic nerve, we apply the solution of salt to the webs of the same side, it is found, that stagnation of blood in the vessels does not so readily take place as it

does when we apply the same solution to the webs of the uninjured side, or to the webs of a frog which has not been injured at all. In short, *stagnation is not so readily induced by the salt when the ischiatic nerve is cut as when it is entire*; and this is, no doubt, owing to the *greater dilatation* of the arteries, and the consequent freer and more rapid flow of blood." (pp. 39-40.)

In fact, Mr. Wharton Jones has been led, by his late re-investigation of the subject, to abandon the opinion formerly expressed by him (*British and Foreign Medical Review*, April, 1844), that the stagnation of the blood is in some way directly dependent on the suspension of nervous influence upon the arteries; but he maintains, as we have seen, that the influence of the nerves upon the process is exerted in causing *constriction* of the vessels (instead of *dilatation*, as supposed by Henle), and that in this way inflammation *may be* excited by a reflex stimulation, which causes contraction of their walls.

As we have already urged, however, it seems to us absolutely impossible that any change in the walls of the blood-vessels, either in the way of contraction or of dilatation, of augmented firmness or of relaxation, can account for the phenomena of inflammation,—a state which essentially consists in a disturbance of the normal nutritive re-actions between the blood and the surrounding tissues; still less can we think it conceivable that such changes of caliber as are seen in the vessels of an inflamed part, are due to any reflex operation of the nervous system, these changes being obviously due to the *direct* action of the exciting cause, and not being prevented by the interruption of the nervous circle. But, in so far as the normal processes of nutrition can be effected by the nervous influence, do we hold that the abnormal condition of those processes, which we call inflammation, can be affected by the same agency. Here, as elsewhere, we must take a sound physiology for our guide; and, although we may be unable to give a precise definition of the *modus operandi* of this force, we should do very wrong to shut it out from our consideration, and thus to give exclusive predominance to other agencies. As Mr. Paget well remarks:

"In discussing such a point as this, one passes from the ground of demonstrable facts; but there is less fault in this than in the belief, that the very little we can see of a morbid process can guide us to its whole pathology. When we look at an inflamed part, we should not think that, if we could see its blood-vessels and test its blood, we should detect all that is in error there; rather we should think, that all the forces are at fault which should be concurring to the due maintenance of that part; and while we are ignorant of the nature of some of these forces, it is better that their places in our minds should be occupied by reasonable hypotheses, than that they should be left blank, or be overspread with the tinge of one exaggerated theory, such as that which ascribes all inflammation to a change in the state of the small blood-vessels." (p. 12.)

IV. The last of the conditions necessary for the healthy nutrition of a part, being "the natural or healthy state of the part itself," we have to consider how far *an alteration in the formative power of the solid tissues* is concerned in the production of the inflammatory state, or comes to present itself in the course of its development. There can be no doubt whatever, in the minds of those who have duly considered the normal phenomena of Nutrition, and those abnormal variations which are designated as Hypertrophy and Atrophy, that the formative power of the solid tissues is as important an element in the process, as the plastic or

organisable properties of the material supplied by the blood. It is by no means sufficient that adequate material for the growth of a particular tissue should be afforded; since unless the part or organ have power to develop that tissue, the material will run to waste. We see this remarkably in regard to the formation of fat. It is not a mere question of the amount of surplus fatty matter in the blood; for some individuals, and some breeds of animals, under the same circumstances as regards food, exercise, temperature, &c., will develop adipose tissue much faster than others; whilst we not uncommonly meet with persons whom no diet will fatten, the immediate effect of rich living being to make them "bilious;" in other words, to disturb the action of the liver by the accumulation of fat in the blood, through the want of formative power in their adipose tissue. In the case of the muscular and nervous systems, again, we have abundant proof that no superabundance of nutritive material in the blood will, of itself, produce increased development of their tissues; if they be disused, they become proportionably atrophied; whilst, if they be called into unusually active exercise, their nutrition is proportionally increased;—all showing that the *formative power of the part* is a most important element in the process of normal nutrition. For another set of illustrations of this general principle, we must refer our readers back to our review of Mr. Paget's former course of lectures. (Vol. I, p. 412.) It is scarcely necessary, we should hope, for us to insist upon the fact, that this formative power is quite independent of the penetration of the tissue by blood-vessels. All that is needed for its exercise, is a supply of nutritive material; and this is brought into more or less close proximity with the elements of the tissue, just in proportion to the activity of their usual demand for it. Thus, in the substance of a muscle, we everywhere find capillary blood-vessels, passing in amongst the fibres, and almost surrounding each with the vascular network; whilst in a tendon, we find the capillary network limited to the spaces between fasciculi of fibres, this tissue not undergoing any rapid changes, and being content (so to speak) with a comparatively limited supply; and in cartilage, the supply of blood is merely distributed upon the surface, or at the edges, of the mass, the growth and development of which are so slow, that the transudation of nutritive material through its substance is sufficient to supply its formative activity. Consequently, as we again repeat, the "action of vessels," as performing an essential part in the nutritive process, is not only an unsupported hypothesis, but is inconsistent with facts, it being perfectly clear that the constructing power resides in the elements of the tissue itself.

Now it seems to us equally clear, that the same is the case in the perverted states of the nutrient process, as in the normal;—that we must regard an alteration of the formative power of the part as one of the essential conditions of the inflammatory state when fully developed; that such alteration may be of itself, in particular cases, the potential cause of the development of the other phenomena; and that it is entirely independent of any other "action of blood-vessels," than that which is concerned in affording the supply of nutrient material. Upon these points we find ourselves in perfect agreement with Mr. Paget; as we shall presently show by extracts from his exposition of them. And we believe that Mr. Simon, also, fully participates in this view, although he less distinctly expresses it. The following extract, however, taken in its connection with a passage we

have already quoted (p. 141), relative to the succession of phenomena in inflammation, places the differences between hypertrophy and inflammation in what we have long believed to be their true light; the condition common to both being an excessive determination of blood to the tissues, or "active hyperæmia:"

"(1.) The determination of blood, though over and above the usual supply, may *admit of application* according to the ordinary and healthy functions of the part. The biceps muscle of a blacksmith's arm receives, perhaps, as much blood as all the muscles of my upper extremity put together; but there is no blood wasted; all that goes there is turned to account, and contributes to the development of a normal tissue. Or (2), the supply of blood may be *more than can be used* and appropriated by the organ so copiously supplied; and then it is that we can get a continued superfluity of exudation pervading the tissue, and find that superfluity undergoing an independent development into certain shaped products—cells or fibres, foreign to the healthy structure of the part.

"Now, what I have just stated is the distinction between hypertrophy and inflammation; their general pathology has much in common—their causes are often alike—their modes of production identical. But in hypertrophy, however large may be the supply of blood, it all goes to the true nourishment of the organ, goes to increase the number of its molecules; while, in inflammation, all that is redundant goes to the formation of new products. After what I have said of the pathological affinities of these two processes, it will not surprise you to be told, that in many organs of the body hypertrophy and inflammation run into one another by almost insensible gradations—as, for instance, with secreting surfaces, where, after a certain time, that which produced at first a mere excess of secreted material, presently causes to be mixed with that secretion more or less albumen, fibrin, blood, pus, and the like." (p. 83.)

Admitting, then, that the derangement of the formative powers of the part in relation to the supply of plastic material, is an essential element of the inflammatory condition, the question arises whether this derangement is in the way of augmentation or of diminution. Now the continued use of the term "increased action," as characteristic of the condition of an inflamed part, even among those who know that such increased action cannot be attributed to the vessels, would seem to indicate an idea that this condition is one of augmented exercise of those vital forces, through which the various acts of organic formation are accomplished. Such an idea appears to be confirmed by the increase in size and weight of an inflamed part, by its higher temperature and increased sensibility, and by the augmentation in the amount of blood which passes through it. All these signs, however, are found to be deceptive, when we examine them more closely. Thus in regard to the formation of new organised products, Mr. Paget justly remarks, that as the material formed presents only the lowest grades of organisation, and as it is not capable of development, but rather tends to degeneration, so long as the inflammation lasts, we cannot regard such production as at all indicating an increase in the exercise of the vital forces of the part.

"It may be but a vague estimate that we can make of the amount of vital force exercised in any act of formation; yet we may be sure that a comparatively small amount is sufficient for the production of low organisms, such as are the fibrinous and corpuscular lymphs of inflammation. The abundant production of lowly organised structures is one of the features of the life of the lowest creatures in both the vegetable and animal kingdoms. And, in our own cases, a corresponding abundant production is often noticed in the lowest states of vital force; witness the

final inflammations, so frequent in the last stages of granular degeneration of the kidneys, of phthisis, of cancer, and other exhausting diseases. In all these, even large quantities of the lowly organised cells of inflammatory lymph may be formed when life is at its last ebb. And with these cases correspond those that show the most rapid increase of tubercle and cancer, and other lowly organised tumours, when the health is most enfeebled, and when the blood and all the natural structures are wasting.

“From these considerations we may conclude, that the productive part of the inflammatory process is not declaratory of the exercise of a large amount of vital, or organising force; and this conclusion is confirmed by observing, that development, which always requires the highest and most favoured exercise of the powers of organic life, does not occur while inflammation lasts. The general conclusions, therefore, may be, as well from the productive as from the destructive effects of the inflammatory process, that it is accomplished with small expenditure of vital force, and that even when large quantities of lymph are formed, such an expression as “increased action” cannot be safely used, unless we can be sure that the defect of the formative power exercised in the proper tissue of the inflamed part is more than counterbalanced by the excess of power manifested in the production and low organisation of lymph.” (p. 50.)

But we would go even further than this; for it seems to us that the development of such low forms of tissue as arise in inflammatory effusions, is not to be attributed to the formative power of the solids, but to that of the blood, which seems to be augmented in somewhat the same proportion as that of the solids is diminished; and the influence of the solid tissues of an inflamed part seems to us to be rather employed in *degrading* the characters of an effusion, which, if thrown out in the substance of an otherwise healthy tissue, or upon a healthy surface, would more readily pass into higher forms of organisation. The difference between the modes of development of such material, dwelt upon by Mr. Paget in his ‘Lectures on the Processes of Repair and Reproduction after Injuries’ (vol. IV, p. 419), seems to us clearly to show, that whilst the *blood* acquires a higher plasticity when the state of inflammation is established, the inflamed tissues possess far less power of aiding in the development of the effused material, than do those surrounding a subcutaneous wound, which is advancing, with the least possible disturbance from inflammation, towards complete reparation. That in the proper substance of the inflamed part there is a decrease of formative power, is a point which can scarcely admit of question. All the changes which it undergoes, as Mr. Paget justly remarks, are signs or results of diminished or suspended nutrition in its tissues; they are all characteristic of atrophy, degeneration, and death;—precisely the doctrine, which, as Mr. Paget does not omit to mention, was taught in the pages of one of our predecessors, and there applied, we believe for the first time, to the explanation of other phenomena of inflammation.

“The tissues become soft or quite disorganised; they are relaxed and weakened; they are degenerate, and remain lowered at once in the structure, chemical composition, and functional power; or else, after degeneration, they are absorbed, or are disintegrated, or dissolved, and cast out; they die in particles or in the mass. During all the process of inflammation, there is no such thing as an increased formation of the natural structures of the inflamed part; they are not even maintained; their nutrition is always impaired, or quite suspended. It is only after the inflammation has ceased that there is an appearance of increased formation in some of the lowly organised tissues, as the bones and cellular tissue.



"So far, then, as the proper substance of the inflamed part is concerned, there appears to be decreased action; that is, decreased formation. There may be, indeed, an increased absorption; but this is also, in one sense, characteristic of decreased exercise of vital force; since all absorption implies a previous degeneration of the part absorbed. Nor can we justly call this, in any sense, 'increased action,' till we can show how absorption is an act of vessels.

"On the whole, then, we may conclude thus far, that one of the constituents of the inflammatory process, one of the characters in which it differs, in respect of quantity, from normal nutrition, is a defect in the nutrition of the proper substance of the inflamed part."\* (p. 49.)

Now, with respect to the large supply of blood, as one of the signs of "increased action," it will be sufficient to remind our readers, that this does not result from the augmented passage of blood through the vessels of the part actually inflamed; for although these are dilated, yet the current of blood through them is retarded or altogether stagnated; and it is through the part to which the state of inflammation, properly so called, does not extend, that the augmented stream of blood is found to move. With regard to the higher temperature of an inflamed part, it would be quite wrong to assume this as a sign of "increased action," understanding by that term an increase in its formative energy. As Mr. Paget justly observes, "this phenomenon is involved in the same difficulty as are all those that concern the local variations of temperature in the body; difficulties which the doctrines of Liebig, however good for the general production of heat, are quite unable to explain." It is probable, that it is partly to be accounted for by the augmented flow of blood through those vessels in which it is still actively moving; but in so far as it depends upon local changes, it obviously indicates rather a more rapid disintegration of tissue than a more energetic production of it; since it is in the former state rather than in the latter, that the conditions for the development of heat (on the chemical theory) are supplied. Thus the heat of a muscle is the greatest when it is being disintegrated by active exercise, not when it is being repaired by the formation of new substance in the intervals of repose. And with regard to the unusual tenderness of an inflamed part, it is obviously due to such a combination of causes, none of which can be legitimately held to indicate an increase of its proper vital activity, that nothing can be rested on this alone. We see an increase in the susceptibility of the sentient nerves, moreover, under many circumstances in which, far from an augmented, there is obviously a diminished activity in the parts from which they spring. There is nothing, then, in these phenomena, which is in the least degree inconsistent with the doctrine, that *a diminution in the formative power of the tissues is an essential, because a constant characteristic of the inflammatory state.* And to this we may add, that it is equally to be observed, whether or not the tissue be itself supplied with blood-vessels; so that it cannot be necessarily connected with their condition. As Mr. Paget remarks:

"For example, some of the phenomena of inflammation may ensue, through injury or disease, in parts that have neither nerves nor blood-vessels. We have instances of this in the deposit of lymph, or the other consequences of severe injuries in the cornea and vitreous humour,—parts which, if ever they are vascular, become so only after the effusion of lymph in them. Such, too, are seen in the ulcerations of articular cartilage, in which the vascular phenomena of inflammation

\* "This has been clearly maintained by Dr. Carpenter. See Brit. and For. Med. Rev., July, 1844."

are confined to the adjacent tissues. We cannot in these cases ascribe the inflammation to an alteration of the relation between the blood and blood-vessels, for blood-vessels do not exist in the part in which the inflammation has its seat." (pp. 12-13.)

This we take to be a point of fundamental importance in the construction of our theory of inflammation, and in our interpretation of the phenomena produced by the operation of *local* causes. We have pointed out, that inflammation produced by the action of general causes may be regarded as immediately due to the presence of some morbid element in the blood, which disturbs the balance of nutrition on *its* side; and that the action of these is frequently determined towards a certain part, by a deficiency of vital power in that part. On the other hand, we stated that it could not admit of doubt, that inflammation may be generated by purely local causes, primarily affecting the tissues of the part, and thus disturbing the balance of nutrition on *their* side. Now if we examine into the *modus operandi* of these causes, we shall find that they are such as tend either directly to depress the vital powers, or to elevate them at first and to depress them by exhaustion. As characteristic examples of the first, we may refer to cold and mechanical injury; also to the effects of many chemical agents, such as tend to bring back the living tissues to the condition of inorganic compounds. As characteristic examples of the latter, on the other hand, we may refer to heat (not too excessive to produce a directly destructive effect, but sufficient to produce an augmentation of vital activity), and to over-exertion of the functional power of the part, both of which agencies have a tendency to exhaust the vital power, and thus to occasion depression. All the local causes of inflammation, we feel sure, might be ranked in one of these two categories; and thus we see that the depression of the formative power in a part may become the determining cause of the other phenomena of inflammation. In this way the inflammatory condition is related to atrophy as well as to hypertrophy; for we might call it an acute atrophy, in so far as the formative power of the tissues is deficient, and the new material laid down tends to degeneration; whilst it may be said to be an acute hypertrophy so far as regards the increased supply of blood, and the amount of its plastic material deposited in the part.

Now we believe it to be in consequence of the non-existence of any other ostensible or *objective* manifestations of this change in the vital condition of the affected part, than the alterations presented in the condition of the blood and its vessels, that its prime *subjective* elements—the change in the vital properties of the blood, or in the formative power of the tissues, separately, or in combination—have been so much overlooked by pathologists. Those alterations being the first that become apparent, it was not surprising that the chief attention should be fixed upon them, so long as inflammation was looked upon as a process altogether *sui generis*, instead of being regarded as a modification of the ordinary nutritive operation. But now that physiologists have been able to arrive at a much more just and satisfactory idea of what constitutes the latter, we cannot but regret that the attention of observers should remain directed so exclusively to the state of the blood-vessels and the alteration in the rate of the blood's movement, as affording an explanation of the phenomena of inflammation—phenomena which a larger view of the subject shows us to

consist essentially in a disturbance of the ordinary formative operations, and, so far, to be capable of occurring in parts which have neither nerves nor blood-vessels. It must be freely admitted, that when a part sustains local injury of any kind, such as excites it to inflammation, its nerves and blood-vessels must have been affected, along with its other tissues ; and if this were all, it might be difficult to prove, except by a circuitous process, that such affection of the nerves and blood-vessels is not the direct and determining cause of the other disorders which speedily manifest themselves. But when we see that a process essentially resembling inflammation, in so far as consists in the phenomena of fibrinous effusion, suppuration, or ulceration, may take place in parts which have neither nerves nor blood-vessels, the conclusion seems irresistible, that although when they are present they are necessarily implicated in the injury, and although the disorder thus induced may concur with and aggravate that which arises from the injury done to other tissues, yet that no change in them is a necessary link in the series of phenomena, either as cause or effect. At any rate, such theorists as fix their whole attention on the affection of the vessels and nerves, are chargeable with a gross neglect of these obvious and palpable proofs, that the causes of inflammation are not thus limited in their operation, but that they must affect, with greater or less potency, *all* the other living tissues of the parts on which they act.

Having conducted our inquiry to this point, we must pause awhile, to give our readers and ourselves a necessary respite. In doing so, however, we would cite the following passage from the conclusion of Mr. Paget's first lecture, as a caution against the misinterpretation of the view, which, with his assistance, we have endeavoured to present, of what *does*, and what *does not*, enter into our essential idea of that which constitutes the Inflammatory process :

"I have spoken so separately of the changes in the several conditions of nutrition, that I may have seemed to imply that inflammation may consist in the disturbance sometimes of one, sometimes of another, of these states. It is true that inflammation may have its beginning in any one of these conditions,—as in an alteration of the blood in rheumatism, in an alteration of the nervous force in irritation of the retina, in an alteration of the proper elements of the tissue in inflammation of the cornea ; but probably it is never fully established without involving in error all the conditions of nutrition ; and, respecting both the manner in which they may be thus all involved and their subsequent changes, they should be studied as concurrent events, rather than as a series of events of which each stands in the relation of a consequence to one or more of those that preceded it. Nowhere more than here is the mischief evident of trying to discern, in the economy of organic beings, a single chain or series of events among which each may appear as the consequence of its immediate predecessor : most fallacious is the supposition that, starting from a turgescence and stagnation of blood in the vessels of a part, we may explain the pain, the swelling, the heat, and all the other early and consecutive phenomena of inflammation. The only secure mode of apprehending the truth in this, as in every other part of the economy of living beings, is by studying what we can observe as concurrent yet often independent phenomena, or as events that follow in a constant, but not necessarily a consequent order." (p. 13.)

## ART. X.

1. *The Nature and Cure of Consumption, Indigestion, Scrofula, and Nervous Affections.* By G. CALVERT HOLLAND, M.D., lately Physician to the Sheffield General Infirmary, &c.—London, 1850. 8vo, pp. 208.
2. *Cases illustrative of the Cure of Consumption and Indigestion.* By G. C. HOLLAND, M.D., &c.—London, 1850. 8vo, pp. 104.
3. *Practical Suggestions for the Prevention of Consumption.* By G. C. HOLLAND, M.D., &c.—London, 1850. 8vo, pp. 143.
4. *A Practical Synopsis of Diseases of the Chest and Air-Passages; with a Review of the several Climates recommended in those Affections.* By JAMES BRIGHT, M.D.—London, 1850. 8vo, pp. 271.
5. *An Inquiry, how far Consumption is Curable; with Observations on the Treatment, and on the Use of Cod-liver Oil and other Remedies: with Cases.* By JAMES TURNBULL, M.D., Physician to the Liverpool Infirmary, formerly Physician to the Northern Hospital, and Lecturer on Clinical Medicine at the Infirmary.—London, 1850. 8vo, pp. 106.

IN noticing these works, it is not our purpose to bring before our readers the subject of Consumption, fully considered in all its wide and important bearings: they do not furnish us with materials for such a purpose. Our object, on this occasion, will be simply to *review*, to speak out plainly, the merits or demerits of the books themselves.

On the title-page of the first volume which stands at the head of this article, we find this motto — “*Quanto verborum parcius, verum divitior.*” It is impossible to read the succeeding pages without becoming strongly convinced, that the selection can only be explained upon the *lucus à non lucendo* principle; for a more wordy and less substantial production it has not been our misfortune to peruse for many a day. Dr. Calvert Holland will, of course, set down this opinion to the score of those “bitter and hostile feelings which discoveries in medical science excite;” and will include us among the number of those who are “peculiarly distinguished by the want of generous conduct, or encouraging expressions towards those who sedulously labour to extend the boundaries of the healing art.” (Preface, p. vii.) But, in spite of this, we must speak the *truth*; and the truth, in our judgment, is simply that which we have stated.

But, though our own opinion is unalterably confident, we neither expect nor desire that others should form their judgment upon our unsupported dictum; and therefore, ungrateful and irksome as the task will be, we purpose laying before our readers such a view of the statements and the *quasi* arguments of the author, as will enable them to form a correct appreciation of the work for themselves.

The Preface, from which we have already quoted, is mainly occupied with that kind of meaningless indiscriminating abuse of the medical profession, with which we have of late become somewhat too familiar;—an abuse so utterly unfounded in reason or in fact, that it would be a mere waste of time to answer it.

The *first* chapter treats of the *Properties and Influence of the Nervous System*. We are here, at the outset, indulged with an exaggerated picture of the uncertainties of medical practice; a long quotation from the

‘Critic,’ on the various remedies suggested for consumption, being added in a foot-note. We are then told, that the reason of these differences is simply, that all the methods hitherto employed in the investigation of disease have been in the wrong direction ; that physiology does not occupy a sufficiently prominent position in the majority of our schools ; that little of practical importance has been added to our knowledge of the circulation of the blood since the time of Harvey ; and that it has not “been investigated with the conviction, that the inquiry is fraught with intensely vital results bearing on the phenomena of disease and remedial measures.” This last sentence has all the prominence that typography can confer upon it, by being printed in capital letters.

Dr. Calvert Holland, we believe, studied in Edinburgh ; he has, therefore, probably listened to the prelections of Dr. Alison ; as a physiologist, he surely cannot be ignorant of the ‘*Outlines*’ of that learned professor ; and yet he can write what we have just quoted ! Many may not be inclined to accord with Dr. Alison’s views ; but we suspect there are few who would be bold enough to deny, that he has approached this, as all other branches of the science, in the spirit of a right *practical* philosophy, and has ever sought to demonstrate the bearing of the principles which he unfolded on the vital phenomena in health and in disease.

But if the Circulation has been thus so slighted, the Nervous System is in scarcely better plight. Our author generously admits, that it has “been studied of late years with zeal and with considerable talent ;” but nevertheless it “has as yet been grasped by no mind in its wide and vital relations to the animal economy. It has been cultivated in detached or fractional portions, rather than as a grand whole, embracing and controlling the entire of the powers of life. The researches into its properties, and the facts which have been established, are by no means undeserving of attention ; but the light with which they abound has not hitherto been elicited in a degree commensurate with the practical importance of the investigations.” What will Dr. Marshall Hall say to this ?

But if we have been thus unhappy hitherto, we are now in an infinitely more favorable condition ; seeing that, in the person of Dr. Calvert Holland, we have an inquirer whose researches give him such a commanding survey, that he “sees in the disturbed and struggling energies of life, not a mass of chaotic elements, unconnected or void of significance, but a field of vital action of vast interest, and abounding in intelligible indications. With the ease, and almost with the accuracy of the analyst of inorganic matter, he mentally separates the efficient from the subordinate causes, estimates at their just value the diversified effects, and distinctly points out the channels through which they are to be reached. His power is shrouded in no mystery. The resources which he brings to bear, result from his comprehensive investigations.” (p. 24.) And the book we are now reviewing, is the grand monument of his success.

The leading idea in the author’s mind, is the sole efficiency of the Nervous system. We are told in the Preface, that all vital phenomena originate in nerves ; and the statement is repeated continually in every portion of the work. To us this appears far too comprehensive and dogmatical an assertion. Plants are the seat of operations no less vital than those of animals, and yet the existence of a nervous system in them has been completely disproved. In many of the lowest tribes of animals,



which move, and grow, and reproduce, no nervous system can be demonstrated; and their condition, in this respect, is paralleled by the early embryo of the very highest. While allowing, therefore, the great and very extensive influence which is thus exerted, we cannot subscribe to Dr. Calvert Holland's dictum, and believe with him, that as all living actions in health are essentially dependent upon nerves, so "every disease, whether local or general, is primarily a derangement of nervous matter." It is quite clear that many members of the class of blood-diseases, a class which the progress of recent investigations is every day extending, must form exceptions to this rule; for though it be true, as our author argues when endeavouring to meet this objection, that the formation of healthy blood is in a great measure dependent upon the integrity and correct action of the nervous system, it is also no less true, that the vital fluid may become unhealthy by the introduction of morbid elements from without, over which the nerves neither have, nor can have, any direct control. And though, when once the blood becomes thus tainted, the nervous system may largely participate in the production of the various symptoms which characterise the disease, yet it is alike inaccurate and unphilosophical to affirm, that the first link in the chain of diseased action is the impression made on the nervous matter. Take, for example, the exanthemata. What is the first step in the production of these disorders? Surely the poisoning of the blood, not the nervous disturbance, which is manifested subsequently. There has been a something added to the blood; and that something working there produces in it certain changes of a peculiar character, from which all the other phenomena directly originate.

So neither can we subscribe to Dr. Calvert Holland's therapeutic axiom, that remedial measures, to be prompt, safe, and effective, "must have an especial application to the nervous system;" for by acting upon this rule we should at once shut ourselves out from the employment of many most valuable appliances, in which the experience of every day serves but to increase our confidence. Nor can we felicitate ourselves upon having gained any available addition to our stock of knowledge, from the perusal of his remarks on the mischiefs of the prevailing practice of treatment and the superiority of his own discoveries; though we are modestly informed, that "they direct the mind, for the first time, in a broad and undeviating path, to the source of all vital phenomena; point out the nature of their initial deviations from health; and with equal clearness indicate the medium through which they may be efficiently corrected, at the least possible expense to the energies of life." (p. 29.)

The *second* chapter is devoted to the *Origin and Nature of Indigestion*. It commences with a sketch of the disease, which presents nothing worthy of remark. The causes are divided into two great classes: first, those which act immediately on the stomach and the bowels; and, secondly, those which, by exciting or depressing other parts of the body, indirectly disturb the conditions of those organs. Among the first are included food, and the frequent use of medicines. Under the former of these heads, Dr. Calvert Holland has some sensible remarks; his statements of the mischief produced by the latter are, as usual with writers of his stamp, grossly exaggerated. For example: "The prevailing practice of drugging the animal system, disorders and poisons its vital properties. It lays the foundation of a vast amount of disease." "Among the baneful causes of

indigestion, is unquestionably the frequent use of purgatives. Independently of the direct irritation they produce throughout the whole of the digestive apparatus, *they occasion a serious waste of the vital energies.*" (The italics are our author's own.) When we read sentences like these, and they are to be met with in almost every publication of those who have deserted the ranks of legitimate medicine, we are irresistibly drawn to the conclusion, that the men who write thus have been themselves, in former days, most unscientific and most unwise practitioners; and that measuring others by their own low standard, they have come to attribute to all what is in reality characteristic but of a few. We are ourselves in the constant habit of seeing patients from all parts of the country, who have been in the hands of men of every degree of professional standing; and we can conscientiously affirm, that drug-injuries are among the rarest of the ailments with which we meet. They are essentially the exceptions, not the rule.

But whatever be the causes of indigestion, it is invariably a nervous disorder, "or in other terms, *is dependent on the morbid condition of the nerves of the stomach and its associated viscera.*" In some cases there is a deficiency of nervous power, in others an excess.

"In both classes, as a rule, constipation is a prevailing condition. In the first, however, it is seldom so obstinate as in the second, — a distinction which has not been clearly pointed out by writers: nor has the cause been justly traced. Diarrhœa is an occasional symptom in the first class, or is readily promoted by aperient remedies; in the second it is exceedingly rare, and far more difficult to induce. The greater irritation of the stomach, in the latter, which is to be measured by the augmented sensibility, draws to it a large amount of nervous energy necessarily from the neighbouring organs, as the liver and intestines, consequently these are to a great extent deprived of that power essential to their action; or from the radiation of the irritation to these viscera, they are unfavorably situated for the exercise of their normal functions." (p. 46.)

Our readers will observe how very obligingly Dr. Calvert Holland's theory explains all the observed phenomena. In indigestion from deficient nervous energy, there is constipation by reason of that deficiency; in indigestion from excessive nervous energy there is constipation in spite of that excess, because the gastric irritation attracts to itself the energy which ought to be residing in the liver and intestines. Why purgatives should act upon the bowels in the one case and not in the other, when the nervous defect is alike in both, we cannot quite understand.

The constipation of dyspepsia, we are informed, by means of capital letters, at p. 56, "arises from a deficiency of nervous energy distributed to the entire digestive apparatus." It is also "*a means which nature frequently employs to conserve her energies.*" And, therefore, the bowels must be allowed, within certain limits, "and these must be liberal," (here we again meet with the favorite emphatic capitals,) "to take their own time in acting upon what they receive, as well as in rejecting the residue of their vital actions." Medicines of all kinds, but especially aperients, are injurious. The great thing is to employ repeated friction with the hand over the abdomen and along the spine. This is Dr. Calvert Holland's prime panacea, the sovereign remedy for many an ailment.

• "Those who have had no experience in such a remedy, can form no adequate idea of the vast amount of good which it is capable of conferring. It awakens the

nervous energy both in the great nervous centres and throughout the abdominal viscera. Every fibre of the body participates in the improved vital actions. These are not the observations of a visionary, who has speculated only on disease in the closet, and who has had no opportunity of testing the correctness of his views at the bedside of the patient, but of one who, for a quarter of a century, has delighted in the exercise of his art, and has sought with avidity every occasion to extend and refine its application. The practice here recommended is of immense value, not merely in the treatment of the class of cases under consideration, but in many other morbid conditions, &c." (p. 67.)

The *third* chapter treats of the *Origin and Nature of Phthisis*. It commences with the old cuckoo-cry of the uncertainty of the ordinary views, and with the very common disparagement of pathological investigations; as if any observer, whose opinions are at all worthy of notice, were ignorant of the unquestioned fact, that mere morbid anatomy will not explain diseases. Dr. Calvert Holland takes a higher flight; but is he more successful? We shall see before long. Here is his pathology of the disease: "Phthisis is to be explained only on the disordered conditions of the nervous system." (p. 76.) So also is indigestion, as we have seen,—so also is scrofula,—and so is every other disease to which flesh is heir. We hope this will assist our readers to acquire precision of thought, and clearness of understanding.

But we must be more particular. Certain persons are, by natural conformation, predisposed to phthisis; no writer has ever yet attempted to explain the reason of this; that is, no one antecedent to our *Magnus Apollo*. He has made it all clear, in this way. "There are two immense surfaces which we have to consider: THE SURFACE OF THE BODY, AND THE INTERNAL SURFACE REPRESENTED BY THE WHOLE OF THE VISCERA." (It is rather a new thing certainly to call the liver, or the heart, or the kidney, a *surface*; but let that pass.) "The two are in indissoluble association, and, at every moment of existence, reciprocally influence each other." (p. 79.)

"EVERY DISEASE IS A DERANGEMENT OF THE RELATIONS OF THESE SURFACES, AND VARIES WITH EVERY MODIFICATION OF THE MORBID ACTION." (p. 80.) Phthisis, therefore, has this origin, and must be cured *by restoring the balance of the circulation*. Change of air and climate, certain medicinal agents, &c., are of use; but the grand thing, "of far greater potency than any of the means hitherto employed," is "persevering friction over the whole surface of the body, but especially over the chest, abdomen, and along the spine." (p. 84.)

But, if disordered relations between the two surfaces of the body cause disease in the general, why in the phthisically disposed should they produce lung disease in particular? The oracle replies thus, and we give the quotation entire, lest we should err by misrepresentation:

"The lungs are exceedingly vascular. They contain a much greater quantity of blood, in relation to the structure in which it circulates, excepting of course the arteries and veins, than any other part of the body. This is the first important condition demanding especial attention. The blood is, also, distributed in extremely minute vessels, ramifying on the delicately-formed air-cells, for the purpose of exposing the largest possible mass to the action of the inspired air, as well as for the emission of matters no longer required by, or injurious to the animal system if retained. These two effects constitute the functions of the lungs. Water, in the state of vapour, and carbonic acid gas, are expired. Oxygen and nitrogen are correspondingly inspired. To promote this interchange is the office of these organs.

It is, therefore, evident, that the elements thus disengaged and received, in relation to the mass of blood in the lungs, are indeed small in quantity. If from the agency of external causes, the vessels of any portion of the lungs become congested, or otherwise disordered, **THEY HAVE NOT THE SAME MEANS AS THOSE OF THE STOMACH, BOWELS, LIVER, OR KIDNEYS, FOR RELIEVING THEMSELVES OF THE CAUSE BY WHICH THEY ARE OPPRESSED.** The gas and vapour emitted, will very slightly influence the existing congestion of the pulmonary capillaries, or tend to set the blood in motion, if stagnant. In fact such a condition, by *diminishing* the chemical changes of the blood, will place this fluid under unfavorable circumstances for the free action of the inspired air, which is essential to the interchange of the gaseous elements; consequently, so far from these vessels having an occasional provision in their structure and office, like that of the secretory organs in general, by which they frequently correct their excited or depressed functions, *such vessels lose, in the ratio of their derangement, the ability to relieve themselves by any modification of their condition.* If these views be justly founded, do they not afford, for the first time, a rational explanation of the tendency of the lungs to structural changes in those predisposed to consumption?" (pp. 89-90.)

We answer emphatically—No. They merely state what we all knew before, that the lungs are apt to become congested, and that when so congested, they are not in a good condition for the exercise of their proper functions.

Granting, then, that the lungs are peculiarly liable to become congested, how does the tubercular matter originate? Hear our author again:

"If we suppose the blood, or its imperfectly-elaborated elements, to become stagnant in various situations of the pulmonary capillaries, though at the first forming mere isolated points, the matter so arrested in its course would be deficient in the ordinary properties of vitality, and would be withdrawn from the influence of the nervous energy essential to constitute it a part of the living system. It would, therefore, become an organic substance, and would act as a foreign body in the tissue in which it exists. Its subsequent enlargement and change of condition are easy to account for. Once being deposited, the constitution continuing in the same state of predisposition, and other general circumstances remaining unaltered, it must inevitably progress. It will impede the circulation in the adjoining capillaries, and in proportion to its influence in this respect, will accelerate the deposition of similar matter. Thus the mere miliary or small grain-like tubercle will become an extensive mass of disease, the pulmonary tissue being either broken down or converted into a fluid of pulpy consistency." (p. 95.)

When cavities are thus formed, they are sometimes surrounded by an exceedingly strong and thick membrane:

"In consequence of which, the patient, in the very last stages of consumption, has recovered, and enjoyed excellent health for a protracted period. Cases of this kind have fallen under our own observation. In one particular instance, in which all the formidable symptoms of phthisis were present, the body being reduced to a mere skeleton, the disease was arrested, and the individual was capable of following a laborious occupation for nine years. At this time he died, after a very short illness. A cavity was found in the right lung, which accounted for the previous remarkable cure, in capacity far exceeding the bulk of the closed hand." (p. 97.)

This is really news to us, and the very first time we ever heard of a cavity in the lung accounting for the cure of a patient who died! Dr. Calvert Holland is a wonderful man.

The next chapter, which professes to treat of the *origin and nature of spasms, and painful affections of the nerves*, is, if that be possible, even more worthless than the preceding. They all depend, as we are informed, upon variations in the quantity of nervous principle existing in the parts,

for this *principle* is a something which may be accumulated or dispersed. (p. 127.) And they are to be cured, chiefly, by that universal remedy, friction, the *modus operandi* of which is here again fully discussed. The only note-worthy portion of the chapter is the explanation of the differences between neuralgia and rheumatism, and this is certainly of a somewhat original character. Rheumatism, we are told, is a *nervous* affection, the nerves being attacked at their peripheral extremities over a more or less extensive surface. "And though the sensitive nerves are the first to receive the morbid impressions, yet from the great number which receive the baneful influence, the motory nerves are subsequently involved in the derangement, and hence the inability to move the affected limb, or the acute suffering which attends the effort." In neuralgic affections, "the abnormal action is generally confined to the trunk of one nerve, or its branches, within a limited space: it is therefore easy, on this view, to account for the particular symptoms which it excites, as the darting, throbbing, and lancinating pains, as well as for its intermittent and remittent phenomena." Of course!

*Scrofula* forms the subject of the *fifth* chapter. It commences with a fierce onslaught upon the doctrines of Lugol, with which we have no concern. The French physician may be safely left to defend himself, if he should think it worth his while. Having, then, demolished his antagonist, Dr. Calvert Holland favours his readers with his own definition of *scrofula*.

"It is not to be regarded as a specific germ or poison, *but as a peculiar state of the vital powers prone to the manifestation of certain morbid conditions characteristic of debility.*" (p. 172.) And every form and modification of the scrofulous constitution is to be referred to the nervous system. This makes everything quite clear, explaining at once, with a precision which is no less delightful than marvellous, all the phenomena of the disease, and the right method of cure. One great element of the latter, we need scarcely say, is FRICTION." (p. 192.)

Everybody knows that, in *scrofula*, the joints are particularly liable to become affected; but everybody does *not* know the reason:

"All joints, according to the degree of their action, require an abundance of blood and nervous power, especially of the latter, as it is this which is called into play by and regulates every muscular motion. Taking into account the depressed state of the nervous system, and the co-existing debility of the circulation, it would be expected, from the influence of these two conditions, that the joints generally would be particularly liable to be affected. If the nervous energy be greatly diminished in these situations, the circulation will be equally disturbed, and hence the gradual morbid changes which take place in them." (p. 188.)

We confess that this is quite a new light to us. We had often recognised the necessity of nervous energy, as respected the active moving organs; but its requirement for the mechanical structures of the joints themselves had escaped our notice. We stand corrected.

Dr. Calvert Holland believes that *scrofula* and phthisis are different diseases, and thus explains the immunity of the lungs in the former:

"The lungs receive an immense amount of blood, but it is here differently situated from what it is in other parts of the body. It is to a great extent directly influenced in its motion by the contractions of the heart, and its circulation is further promoted by the reception of the stimulating principle of the air in exchange for those which are deleterious,—circumstances which necessarily keep the blood in



lively motion, and consequently it is not, as in other regions, liable to *stagnate*, which is the first essential step to the structural changes subsequently induced.” (p. 185.)

It has been our lot, not seldom, to meet with authors who were inconsistent with themselves ; but so plain, barefaced a contradiction as this, we have rarely seen. Let our readers, for a moment, refer back to the reasons given for the frequent implication of the lungs in phthisical subjects, and remember that, according to Dr. Calvert Holland, as well as all other writers, both phthisis and scrofula are diseases characterised by debility.

It is pretty generally known, that Dr. Calvert Holland has deserted the ranks of legitimate medicine, and gone over to those of the followers of Hahnemann ; *ecce signum*, the treatise we have been reviewing is dedicated to Dr. Quin. We congratulate them on the valuable accession, and for ourselves feel that we can well spare the author of a book like this, so full of pretensions and so empty of all value.

Why are the cases which should illustrate this volume published separately, in different form and type ? Are they intended for the profession, or for the public ? We greatly suspect the latter. The chief remedies recommended for phthisis are tar-water, small doses of iodine, cod-liver oil, and friction. For indigestion, the wet bandage to the abdomen (a weak solution of common salt being used), and friction. All the cases are *cures*.

Of the work which stands next in order,—whose dedication to Sir James Clark seems to show a sort of hankering on the author’s part after his old associations,—we do not feel ourselves called upon to speak. The author, in the first sentence of his preface, candidly tells us, “that it has not been written with the hope to interest, or with the intention to enlighten the medical profession ;” and having read it through, we can most assuredly say that we have been neither interested nor enlightened by the perusal.

Dr. James Bright’s production is, we regret to say, equally deserving of unmitigated condemnation. It does not offend, like those we have just noticed, by incessant self-laudation and unfounded pretensions ; but it is absolutely inane—a most miserable specimen of book-manufacture. When our readers learn that in the space of 271 pages, printed in goodly-sized type, with only twenty lines in a page, the anatomy and physiology of the organs of respiration, *all* the diseases to which they are liable, and the climates to which pulmonary patients are sent, are professed to be discussed, they will not feel any surprise if we inform them, that the information conveyed is of the most meagre description, alike useless for theory and for practice. Had Dr. James Bright, in the course of that experience of which he speaks in the preface, observed or excogitated anything new in regard to any one of these numerous affections, we should have listened to what he said with respectful attention, and have weighed his words, to the best of our ability, in the balance of truth. But he has done nothing of the kind. We have just the old dishes served up, in a most un-Soyer-like fashion of cookery, and our literary appetite rebels.

We beg Dr. James Bright’s pardon ; we are doing him an injustice ;—there *are* some new things in his book. Whether they are true or not is

another question, of which our readers shall be judges. Speaking of the anatomy of the lungs, he says :

“The air-cells are roundish angular vesicles closely aggregated together, but *not* communicating one with another. They are united, however, by a *common vascular twig*. Such a congeries of cells with its tiny vessel constitutes a minute roundish lobule, half a line in diameter, which is surrounded by a layer of cellular texture, of from a tenth to a fifth of a line in thickness. This cellular layer conjoins several of these congeries, so as to form a lung lobule, and which is supplied by a larger and arborescent vascular twig.” (p. 12.)

During respiration, he states :

“A portion of the oxygen of the air penetrates the delicate walls of the capillaries (while the nitrogen serves to keep the pulmonary cells dilated), *and combines with the carbon and hydrogen of the venous blood*. . . . Under these circumstances the temperature is modified, inasmuch as the free caloric of the venous blood is partly carried off with the watery vapour, while the caloric in the arterial blood becomes more fixed, the capacity being changed.” (pp. 26-27.)

We are told that there is dullness of percussion in *pleurodynia* (p. 45), that “the term *pulmonary hæmorrhage* is equivocal, for it comprises any discharge of blood from the mouth, whether arising from the throat, fauces, nasal passages, trachea, or lungs” (p. 143) ; and that “metallic tinkling is *always* heard when there is a morbid excavation in the lung.” (p. 185.)

It is most refreshing to turn from these miserable specimens to Dr. Turnbull's modest, unpretending volume, which is conceived and executed in a right philosophical spirit, and constitutes a really valuable addition to the literature of consumption. Dr. Turnbull makes no profession of new views, and advocates no new plan of treatment ; his object is simply to investigate the question of the *curability* of phthisis, and having rendered an affirmative answer, to indicate the best means for bringing about a result so desirable. We are quite at one with him in almost all that he says ; and being deeply persuaded of the necessity of a clear apprehension of the point at issue, seeing that a combatant without *hope* is most likely to be defeated, we purpose, very briefly, to indicate the nature and source of the proofs we possess, that consumptive patients are not *ipso facto* incurable.

The most conclusive evidence of the possibility of a recovery from phthisis is derived from necroscopic examinations. Tubercular degenerations, as we all know, may be found in the lungs in three principal states or stages,—in the form of small miliary granulations, in masses of variable size, and as cavities from softening.

“Pathological facts show that recovery may take place in each of these stages. The evidence that they furnish of its occurrence in the third stage by cicatrization, is the most perfect, that of its occurrence by cretaceous transformation is the most common, and the evidence of its occurrence in the first is the least common,—a circumstance, however, which does not lead us to believe that it is really the less frequent ; but, on the contrary, that all trace of the disease is removed in the earliest stage by absorption.” (p. 17.)

For that tubercular matter can be absorbed, we know from the fact that it is often removed in this way, when deposited in glands. Rokitanski, as we have formerly noticed (Brit. and For. Med.-Chir. Rev., vol. I), believes that miliary tubercles undergo a kind of metamorphosis, which he calls

*obsolescence*, by means of which the tubercle, after it has passed through its condition of crudity, loses its shining appearance, and increases in density, becomes converted into a small hard lump, and then shrinks into a tough amorphous or slightly horny mass—cornification. This forms the basis of a complete destruction or death of the tubercle, and no further metamorphosis can take place.

The *chalky* and *calcareous concretions*, which are so often found in the summits of lungs, are invariably, we believe, acknowledged to be the results of the transformation of tubercles. According to Rokitansky, this never occurs in tubercle in its original form, being confined to the dissolving and dissolved blastema; and in connection with this, it is worthy of note, that transformation commences, as softening does, at the centre of tubercles. Dr. Valleix has observed tubercles having a hard calcareous concretion at the centre, round this cretaceous matter, and at the circumference a layer of tubercular matter. Boudet believes that the transformation may take place at all the stages,—gray granulations, crude, or yellow or softened tubercles. Be this as it may, the fact of cures accomplished by this process is established.

So also it is proved that cavities may be healed by cicatrization; by contraction with chalky or calcareous concretion; or by the formation of a thick tough fibro-cellular lining membrane, the cavity remaining persistent.

Nor are these pathological changes unfrequent. Dr. Williams states, that he found phthisical lesions in the lungs of half the adults beyond the age of forty that he had examined. Professor J. H. Bennett found concretions and puckerings of the lungs in 28 out of 73 bodies. Rogée states, that of 100 aged persons who died at the Salpêtrière, 51 had concretions and other traces of tubercular disease of the lungs. In five of the cases he found cicatrices of cavities which had healed; and he states, that in the course of a single year he had been able to collect ten or twelve incontrovertible examples of the same kind. (p. 24.) Nor is this fortunate issue confined of necessity to cases in which the disease has been of very limited extent; for Dr. Bennett has recorded the case of a man, who, at the age of 22, laboured under all the symptoms of deep decline, but recovered, and died at the age of 50 of an affection of the brain. The apices of both lungs contained cretaceous tubercles, and were puckered, and the cicatrix at the summit of the right lung was from a quarter to three-fourths of an inch in breadth, and three inches in length. (p. 26.) When we remember the tendency which all cicatrices have to contract, it will be evident that one of this kind must have resulted from a cavity of very considerable size.

But we are not limited to pathological revelations for all our arguments in favour of the curability of phthisis. There are, besides these, numerous instances on record of the recovery of patients, who had presented all the rational signs and symptoms of consumptive disease; and every year adds to the number of these. In the work before us, Dr. Turnbull narrates thirteen cases. In two of these recovery took place from the first stage. In a third the second stage had commenced, there was complete restoration of health, and the physical signs were reduced to mere roughness of respiration, with prolonged expiration, and vocal and tussive resonance. In another, with hæmoptysis and cavity, there was recovery of the general health, and removal of the cavity. In another, in the third stage, the

cavity remained patent, but contracted, and the health was restored. In another, where there were evidences of extensive disease and cavity, similar improvement took place in the system at large, and almost all physical signs were removed. In the remainder the disease was arrested.

How far these recoveries may be permanent, for recoveries they are, though it is too early to speak of them as absolute *cures*, we cannot of course say. But the experience derived from the first source will surely warrant the hope, that, by the avoidance of known exciting causes, the health may be retained at a moderately good point for the ordinary term of life. We have ourselves had for six or seven years a very interesting case under our notice, in which the gradual but steady improvement has been most gratifying,—the signs of active disease which once existed being gone, snuffling bronchophony remaining as the almost sole evidence of local mischief. In this case, we believe that a cavity was healed by the formation of a thick lining membrane, and is now probably undergoing the process of contraction.

These things surely teach us a lesson of no little importance; they cannot but tell us that despair is unwise, that we may hope even for our consumptive patients; and that hoping we are bound to use all the appliances which our art makes known,—medical, hygienic, climatic,—to bring about what some will call an *arrest*, what others will designate by the more cheering name of *recovery*, but what, call it as we may, is in numberless instances a priceless boon.

We very cordially commend Dr. Turnbull's little book to the notice of the profession.

#### ART. XI.

*Pathological and Surgical Observations on the Diseases of the Joints.* By Sir BENJAMIN C. BRODIE, Bart., V.P.R.S., &c. &c. Fifth Edition, with Alterations and Additions.—London, 1850. 8vo, pp. 399.

It is to the matured writings of such men as Cooper, Brodie, Lawrence, Travers, Crampton, Bell, and (we rejoice to think) of a host of others, that British Surgery owes its present exalted position. The specious reasoning and pompous display which characterise the ephemeral productions of the present day, may indeed excite a momentary fear for the permanence of that position; but can never seriously imperil it, so long as we have amongst us men whose minds are fired with a single spark of that genius, which lives in the writings of those to whom we have referred, and will remain to instruct and warn, long after the authors themselves shall have quitted for ever the scenes of their earthly labours. We may smile at the daring assumptions of the oftentimes juvenile theorist, which we read in language that, to suit the spirit of the age, is rendered meaningless by the metaphysical jargon of a dreamy transcendentalism; or we may shrink from the reckless audacity of the dashing operator, whose minutest "procedure" is "put in force," and chronicled, week by week, for the admiration of the vulgar, and the contempt of the wise and good:—we may stand aghast at the unblushing quackery of those who ought to be freest from its minutest taint:—and we may mourn at the bitter rivalry and cordial hate of men, whose profession it is to love and help:—but

we need not yet despair, though we may feel sad and weary at our hearts.

The love of truth, for its own sake, is perhaps our brightest national distinction ; and it is this that has gained for British writings in every science the noble appellation of "sterling." In our own profession there are many such ; wherein patient observation, wise and cautious deduction, are stamped on every page. The modest octavo given to the world long after the ordinary incentives to publication have passed away, suffices to contain the accumulated experience of years. Such works are written with the sole object of benefiting others, by the communication of knowledge which has been acquired at the price of a whole life of toil and opportunity. Their germ is often to be found in some unpretending paper, buried in the Transactions of a learned Society, and almost forgotten in the multitude of its volumes ; but yet containing lofty thoughts, the foreshadowing of much that has since been made distinct, and hints of inestimable value in the treatment of disease. Such writings are the evidences of the growth of men's minds ; they are bequeathed as the legacies of genius to truth ; they constitute the "usury" of the "not wasted talent."

It is singularly pleasing, when such a work has reached a fourth or fifth edition, and when its author might well be excused for reposing on his laurels, to find in it the same evidences of zeal, and the same familiarity with the most recent discoveries, as first made it valuable when he was in the heyday of his youth, and had yet his way to make in the world. These qualities, which render it so valuable, make it at the same time most difficult to describe, or, as it is called, to "review ;" for where all is worthy of selection, it is hard to be fastidious, and not easy to transcribe without abridgment.

These few remarks have been called forth by the perusal of this new edition of Sir Benjamin Brodie's '*Treatise on the Diseases of Joints.*' The foundation of the Treatise is to be met with in papers published in the '*Transactions of the Medico-Chirurgical Society,*' so far back as the year 1813. Five years later, these observations appeared in the form of a Treatise ; which in subsequent years, viz. : in 1829, 1834, and 1841, received additions and alterations, that have in the present year reached their complete form in a fifth edition, enlarged and extended. The present volume is larger than any of its predecessors ; and, as might be expected, contains many new observations and cases, the fruits of the author's more extended experience. Chapters VI and VII, on *Necrosis of Joints*, and on *Chronic Disease of the Joints connected with Gout and Rheumatic Gout*, may be regarded as the principal novelties. The arrangement of the chapters has also been altered and improved.

Chapter I. *Inflammation of the Synovial Membranes of Joints.*—We are induced to quote the following passage, as containing the general result of Sir Benjamin's experience of the constitutional origin of these and most other diseases :

"Inflammation of a synovial membrane may arise as a local affection, the consequence of a sprain, a contusion, or other mechanical injury. In other cases, various joints being affected, either simultaneously or in succession, it is manifestly the effect of a disordered state of the general system ; and even in those instances in which the inflammation is confined to a single joint, a careful inquiry will generally



satisfy the surgeon that it has had a similar origin. Indeed I must confess that, in proportion as I have acquired a more extended experience in my profession, I have found more and more reason to believe that local diseases, in the strict sense of the term, are comparatively rare. Local causes may operate so as to render one organ more liable to disease than another; but everything tends to prove that, in the great majority of cases, there is a morbid condition, either of the circulating fluid, or of the nervous system, antecedent to the manifestation of disease in any particular structure. Moreover, even in those cases in which a disease may be distinctly traced to some kind of mechanical injury, the character which it assumes depends as much on the state of the general health as on the injury itself. Thus we find a sprain of the ankle, in one instance, to be followed by no urgent symptoms, while, in another, a sprain not apparently more severe, is followed by intense inflammation, for the removal of which the most active antiphlogistic treatment is required." (pp. 24-25.)

In a great number of instances, the synovial inflammation is attributable to a gouty diathesis, evidenced by the existence of lithates in the urine, and according to Dr. Garrod in the blood; or to rheumatic inflammation, accompanied or caused by the predominance of lactic acid in the system (Prout). And the circumstance that such diseases show themselves in the synovial membranes of joints, rather than in the serous membranes which are nearly allied to them, is to be explained by the constant motion of joints, and generally speaking, by their liability to injury from their exposed positions. Sir Benjamin also thinks it probable that diseases are included under the term rheumatic, which are of a totally different character, and require a different mode of treatment; as, for instance, inflammation of joints in connection with papular syphilitic eruptions, or chronic inflammation of the bones and periosteum. All these diseases may be either acute or chronic; but, as is well remarked by the author:

"It must be observed, however, that the boundaries of acute and chronic inflammation do not admit of being very well defined. These terms accurately enough express the two extremes; but there are numerous intermediate degrees of inflammation, of which it is difficult to determine whether they should be considered as being of the acute or chronic kind. On this, and on many other occasions, the pathologist must be content if he can succeed in pointing out the principal varieties of morbid action which occur, and the symptoms which they produce, in such a manner as will enable others, with the assistance of a certain degree of original observation, to distinguish those nicer shades in the characters of disease, which language is inadequate to explain, but a knowledge of which is of considerable importance in medical and surgical practice." (pp. 32-33.)

All the joints may, of course, be attacked with inflammation of the synovial membrane covering them; still, the symptoms produced are more severe in some than in others. Thus, although inflammation of the synovial membrane of the hip is not common, yet, when it does occur, the pain it produces is excessive, owing chiefly to the limited extent to which swelling can take place; and there is the further danger of the head of the bone being thrust out beyond the margin of the acetabulum, and being then completely dislocated by the action of the muscles. One example of this accident the author dissected, and thus describes:

"I ascertained that there had been no rupture of the capsular ligament; but that the superior and posterior attachment had gradually shifted its place from the margin of the acetabulum to the dorsum of the ilium above it, so that, although the head of the thigh bone was no longer in the acetabulum, it might be said to be still within the joint." (p. 38.)

Reference is then made to gonorrhœal inflammation of the joints, a disease which was first accurately described by Sir Benjamin in 1818. It is strange that in this affection the metastasis should be from a mucous to a serous membrane; and as yet it is impossible to say, that either the learned author of this Treatise, or any one else, has published a complete pathology of the disease. In the treatment of all these varieties of synovial inflammation, the author strongly insists upon what he has already expressed in one of his pithy sentences,—the necessity of regarding them as constitutional and not merely local affections. We pass over many practical remarks upon the best method of giving rest and support to inflamed joints; and pause only to observe, that when the joint is greatly and distressingly distended with serum, it is recommended to make a few minute punctures, and apply an exhausted cupping-glass; and to make a free incision into the joint, as soon as ever pus is known to have been secreted. Sir Benjamin then says:

“If it be a question whether a collection of fluid in a joint be purulent or otherwise, it is prudent, in the first instance, to make a puncture with a grooved needle. If it prove to be purulent, a free opening should then at once be made with a lancet, in a depending situation. It is important that this operation should not be long delayed, lest the matter should make its way out of the joint in other directions, and form irregular and circuitous sinuses among the neighbouring tendons and muscles. It is equally important that the opening should be sufficiently large to allow the matter to flow spontaneously, without it being necessary to have recourse to pressure on the joint. If afterwards there be reason to believe that there is still a lodgement of matter in any part of the joint or among the neighbouring soft parts, the original opening should be dilated, or the surgeon should avail himself of the first opportunity which occurs, of making another opening in a convenient situation; and it will often happen that several such openings will be required before the cure is completed.” (p. 59.)

The employment of exploring needles has certainly found its chief champion in the writer of the volume before us, and against so great an authority it is difficult to contend. The practice is, however, in our view, a questionable because a dangerous one; and, as we have many times heard it said by perhaps as great a surgeon as Sir Benjamin, need rarely or never be resorted to by a man of tact and experience. We do not of course mean to say, that there is not often extreme difficulty in diagnosing the nature of a tumour, or the contents of a cyst; but we do mean to say, that by unnecessary exploratory punctures, many a malignant growth has been excited to inflame and increase, when it was in a quiescent state.

Such is a brief and imperfect outline of the contents of this valuable chapter. Many things of importance we have passed by altogether; as, for instance, cases in which the synovial membrane has proceeded rapidly to suppuration, and in which it has been supposed to be the seat of primary ulceration (Chap. II); but for these we had not space, and all our extracts and remarks must necessarily fall short of what they would be, were our Review more extensive than it is.

Of that disease, which is very generally known under the name of pulpy degeneration of the synovial membrane (Chap. III), Sir Benjamin has not much additional knowledge to communicate. He is inclined, however, to take a more cheerful view of the chances of curing it than he formerly did, provided the disease be in an early stage. The grounds upon which he forms this opinion, are thus stated:

"The analogy of what happens in the case of the inflamed and granulated *conjunctiva* consequent on conjunctival ophthalmia, seems to justify the opinion, that when the change in its condition exists only in a limited degree, the synovial membrane may, under a proper mode of treatment, persevered in for a considerable length of time, be restored to a healthy state. But it would be unreasonable to expect this favorable result in the more advanced stage of the disease. Under these circumstances, the questions will arise, — Is the joint altogether in a hopeless condition? Can the limb be preserved; or must the patient sacrifice it in order that he may save his life?" (pp. 88-89.)

Without questioning the fact that the disease may proceed to a favorable termination, we cannot help expressing our surprise, that so imperfect an analogy, if an analogy at all, as that subsisting between a *mucous* and a *serous* membrane, should be put forth as the groundwork of the opinion. If such a statement were made by a less eminent person, we fear that the probable surmises would not be flattering to the writer's self-love.—When the disease is far advanced, but is uncomplicated with abscess in the joint, Sir Benjamin is inclined to think that complete and absolute repose will sometimes cure the disease, by inducing absorption of the synovial membrane, and ankylosis of the opposed surfaces.

The ultimate pathology of the disease is still obscure. In an account of the dissection of a knee-joint, which probably exhibited an early stage of this disease, the writer states :

"I found that the synovial membrane of the affected knee externally had its natural appearance. Internally it was lined by a straw-coloured gelatinous substance, so intimately adhering to it, that it could not be detached, except by an artificial separation. The synovial membrane was encrusted in this manner every where except on the cartilaginous surfaces. The gelatinous substance in general appeared to be about one eighth of an inch in thickness, but in some parts near the margin of the cartilage it was much thicker, so as to project considerably into the cavity of the joint. In a few places, towards the edge of the articulating surfaces, the cartilage had begun to ulcerate. In some of these it was entirely absorbed, so that the bone was exposed; but for the most part there was only an irregular ulceration on the surface, the remaining portion of the cartilage being entire, and retaining its natural adhesion to the bone.

"The synovial membrane itself bore no marks of inflammation. In the substance with which it was lined, some vessels were observed ramifying, injected with their own blood; but these were few in number, and only in certain parts. This substance differed from the coagulated lymph which is found on the surface of an inflamed membrane; and we presume, considering the circumstances, that it was the result, not of inflammation, but of some other morbid action." (pp. 86-87.)

The diseases of those textures near joints, such as the synovial membranes, the thecæ of tendons, and the bursæ mucosæ, the offices of which are similar,—viz., the diminishing of friction,—have yet to receive a satisfactory investigation; and will one day, in all probability, prove to have a greater mutual correspondence than is at present recognised. The pippin-like bodies that are found in bursæ and the thecæ of tendons, bear striking marks of analogy with the adventitious growths, loose cartilages, &c., of joints; and the strange productions which sometimes spring from the surfaces of ulcerated bursæ, are not unlike, on a small scale, the appendices which hang down into some diseased joints.

Chapter IV. *Scrofulous Disease of the Joints, having its origin in the Cancellous Structure of the Bones.*—From this chapter we might make many interesting extracts, had we space at our command; as it is, we can

only say a few words on the treatment of the disease. Besides great attention to general hygienic measures, such as diet and fresh air, especially that of the sea-coast, Sir Benjamin recommends the exhibition of iron more than anything else. The following passage contains so much of that matter-of-fact common sense, for which the writer is remarkable, that we prefer to quote it entire :

“As a general rule, children who are thus afflicted will derive much benefit from passing the greater part of each year on the sea-coast. They may bathe in the open sea in the summer, and use the warm sea-bath during the winter. The air of the country is to be preferred to that of a crowded city, and in fine weather they should pass as much of their time as possible out of doors. They should live on a plain nutritious diet, avoiding many of those articles which are given to children, not because they are wholesome, but because they are agreeable to the palate. The bowels should be carefully regulated, without recourse being had to anything like drastic purgatives. Occasionally, a wrong state of the secretions furnished by the organs of digestion may indicate the administration of some alterative doses of mercury, but mercury should never be used on any large scale, so as to place the system under its specific influence.

“It is more difficult to determine the real value of remedies in a disease which is so completely chronic, than it is in acute diseases ; but, from the long experience which I have now had, I am satisfied that, of what are called tonic medicines, none are so generally useful in these cases as preparations of iron. They must be given, however, not merely for a few weeks every now and then, but, with occasional intermissions, for a very long period of time. To children I generally give some simple preparation, such as the *vinum ferri* of the old pharmacopœia, or the syrup of the citrate, and sometimes of the iodide of iron, for three or four weeks. I then direct it to be omitted for a week or ten days, then to be given again ; and so on, for two or three years, or even for a longer period. If the dose given in the first instance should prove to be too stimulating, it may be diminished. For those children with whom iron does not agree, other tonics, one at one period, another at another, may be substituted for it,—quinine, decoction of cinchona, sarsaparilla combined with the *liquor potassæ*, or infusion of gentian. The mineral acids, on the other hand, may be given when the appetite fails, or there is a disposition to night-sweats.. I do not venture to say that the iodide of potassium, or other preparations of iodine, are never useful in these cases ; but my own experience has led me to believe, that great as the beneficial influence of these remedies undoubtedly is in many other diseases, their usefulness in the various forms of scrofulous disease has been very much overrated. But there is another remedy, regarding which, although I have had much less experience of its effects than I have had of iodine, I cannot doubt that it may be often employed in these cases with the greatest benefit ; I allude to the cod-liver oil. A child may take a teaspoonful three times daily, and an adult may take a proportionally larger quantity, for three or four months at a time. It is quite compatible with the exhibition of iron, which may be given in the intervals, or simultaneously with it.” (pp. 130-32.)

The local treatment of the common complication of these diseases—abscess—is treated of at length. In general, in scrofulous diseases of the joints, leeches, and counter-irritation by open blisters or issues, are condemned. The joint is to be maintained at rest by means of splints of stiff leather or gutta percha ; and should an abscess form, it is not to be interfered with until it approaches the surface. If we open it when at any depth, the hæmorrhage from the incision may be considerable, and the admixture of blood with the contents of the abscess induces putrefaction, with all the evil results of pent-up foul secretions upon the general health. In like manner, a small incision permits the lodgment of pus, which acts

like a pea in an issue, it stimulates the secretion, and augments its quantity. The following practical instruction on the method of treating these abscesses, is too important to be omitted :

“An opening having been made with an abscess-lancet, the limb may be wrapped up in a flannel wrung out of hot water, and this may be continued until the first flow of matter has ceased, a poultice, or water dressing, being applied afterwards. In some instances, after a short time, the discharge ceases ; the orifice heals, and the puncture may then be repeated some time afterwards. But where the puncture has not become closed, I have never found any ill consequences to arise from its remaining open. On the contrary, I have no doubt that it is desirable that the wound should not be closed until the abscess has contracted, granulated, and healed from the bottom ; and this is one reason for making, not a small puncture, but a free opening with an abscess-lancet. Another reason is, that the matter will escape readily without squeezing or pressure. *All rough manipulation is to be carefully avoided.* It produces hæmorrhage into the cavity of the abscess, the ill consequences of which I have already pointed out ; and, independently of this, it may excite inflammation of the cyst, attended, where the surface is extensive, with so much constitutional disturbance as to endanger the life of the patient at the time, and materially lessen the chance of his recovery afterwards.

“The treatment of the sinus which is left after the opening of an abscess, may be comprised in a few words. If the orifice be disposed to heal prematurely, this may be prevented by the occasional application of the caustic potash, care being taken that the caustic does not enter the sinus itself ; otherwise some simple ointment or a water dressing is all that is required. The old practice of probing a sinus scarcely ever affords us any useful information ; nor does it in ordinary cases answer any other good purpose. On the other hand, by irritating the sinus, or even the joint itself, it is often productive of serious mischief. The same observation is applicable, but with greater force, to the use of stimulating injections. I do not believe that they promote the healing of sinuses under any circumstances ; but, with respect to those which are now under our consideration, there is no doubt that their operation is highly injurious. I saw a young man who nearly lost his life in consequence of a surgeon having ventured to inject port wine into a sinus connected with some diseased or dead bone of the pelvis.” (pp. 136-37.)

When, in scrofulous disease of the hip-joint, dislocation has occurred, the question arises as to the propriety of removing the head and neck of the femur by surgical operation. Sir B. Brodie does not give a very decided opinion upon the point ; but taking into consideration the necessary loss of blood, though this be small in amount, the local disturbance to the parts, as well as the fact, that the greater part of the disease is left behind in the acetabulum, he “does not recommend it, except where some very unequivocal advantage may be expected from it.” We must add one more to our already lengthened extracts from this chapter ; it is another of those acute observations, which a less experienced man could hardly make :

“But for the attainment of a cure, in addition to skill and attention on the part of the surgeon, there is required a large stock of patience and self-discipline on that of the patient and his friends. It is no small proportion of those who are born to the enjoyment of ease and affluence who expect such an exemption from the evils of life, as does not belong to human nature. Such persons, in cases of this as well as of most other chronic diseases, are too often not content to await the good which may gradually be obtained from a long perseverance in the use of some simple but efficient remedies. They pass from the hands of one empiric to those of another ; listen to, and believe any promises which are made to them ; and at last, when it is too late, discover that they have been in an error, and that in their anxiety to obtain a speedy cure, they have lost the chance of that ultimate one which they might have obtained otherwise.” (pp. 146-47.)



The next chapter, No. V, is of extreme interest and importance in a physiological as well as in a surgical view. It is entitled, "*On Ulceration of the Articular Cartilages.*"

Most persons have a general notion, and in so far a correct one, that the nutrition, disease, and repair of cartilage, present points of difference with the corresponding phenomena of the other tissues of the body; but we are inclined to think, that few surgeons are fully aware of the progress which the investigations of late years have made, in the explanation of the seeming anomalies which the subject presents. It is quite certain that no blood-vessels and no nerves exist naturally in cartilage; and it is, therefore, equally certain that the changes which take place in it, and the symptoms which arise when it is diseased, cannot be due to "*inflammation*" in its ordinary signification. We must refer our readers to the review of Dr. Redfern's monograph on "*the Diseases of Articular Cartilage,*" in our July number, for an account of the evidence upon which this opinion is founded; our concern with it here can only be in its practical and therapeutical application.

So long as articular cartilage is the only texture in a joint that is affected with disease, great and striking changes may be produced in its substance, without any objective symptoms indicating that a morbid process is going on. All these changes are due to a lesion of nutrition, as was stated by Rokitansky, and has recently been very clearly demonstrated by Dr. Redfern. The particular nature of these changes is explained in the Review to which we before alluded; but although in many instances involving great loss of cartilage substance, they are totally distinct from a process of *ulceration*,—at least, so says Rokitansky, and in this respect he is at variance with Dr. Redfern. Those very rapid diseases of cartilage, to which the name ulceration might with much propriety be given, are due, in all likelihood, to disease of the synovial membrane, of the layer of bone in immediate contact with the cartilage, and of the perichondrium. It is the circumstance of one or other of these textures being inflamed, that occasions the excessive pain, as has been explained by Dr. Redfern.

With these views upon the pathology of the disease called Ulceration of the Articular Cartilages, the profession will have a better opportunity of becoming acquainted, in the forthcoming translation of Rokitansky's work by the Sydenham Society, as well as by the perusal of Dr. Redfern's recent researches; and they are so different from those commonly entertained, that we are unwilling to enter into a critical analysis of this chapter of Sir Benjamin Brodie's work. The following, however, is an account of a peculiar affection of the shoulder-joint, and of some circumstances connected with it, that are not familiar to all. The affection is far from uncommon in hospital practice, although Sir Benjamin has encountered it more frequently in private; and is one of the most tedious and annoying with which the surgeon has to deal. In many instances it has seemed to us to depend upon a chronic inflammation of the substance of the deltoid muscle, or of the bursa under the deltoid, and to have been greatly benefited by the employment of counter-irritation and the iodide of potassium:

"The cases here referred to occur more frequently in private than in hospital practice: and (whether it be accidentally or not I do not know) it certainly has hap-

pened that I have met with it more frequently in the female than in the male sex. The patient complains of pain, which however is referred not so much to the joint itself as to the arm a little below it, near the insertion of the deltoid muscle. At first the pain is trifling, but it soon becomes severe and constant. The patient describes it as a *wearing* pain, of which she is constantly reminded. It is aggravated by every motion of the limb, and by pressing the articulating surfaces against each other. Not only is there no perceptible enlargement of the shoulder, but after some time, in consequence of the want of use and wasting of the deltoid muscle, it seems to be actually reduced in size. It is not long before the mobility of the joint is impaired, becoming gradually more and more limited. When the patient attempts to raise the elbow from her side, it is observed that the scapula is elevated at the same time with the humerus. She is unable to raise her hand to her face, nor can she rotate the limb so as to place it behind her. When the progress of the disease is stopped at an early period, the mobility of the joint may be restored; but otherwise, although the pain and all other symptoms of the disease have subsided, the joint remains stiff, and to all appearance completely ankylosed. Whatever motion the arm is capable of, under these circumstances, depends not on the humerus, but on the scapula; and it is remarkable to what an extent the scapula, or rather the muscles belonging to it, will accommodate themselves to this new state of things, so as to make up for the deficient motion of the shoulder.

"It certainly is seldom that this disease terminates in abscess of the joint, when proper attention has been paid to the treatment of it, nor has this happened in any case in which I have had the opportunity of closely observing its progress. I have, however, seen cases of abscess with ulceration of the cartilages, and complete destruction of the shoulder-joint, in which I conclude that, if I had been consulted at an early period, I should have found the symptoms to correspond with those which I have just described.

"Whether it be from this, or from any other disease, that the joint of the shoulder is brought to such an extreme state of disorganisation, one result is, that it is liable to dislocation, or, more properly, to sub-luxation in the direction forwards. In one case, in which I had the opportunity of examining the parts after death, I found the anterior margin of the glenoid cavity of the scapula destroyed by ulceration, the head of the humerus permanently resting on the ulcerated surface. In another case, in the living person, I found the dislocation to be only occasional, the head of the bone slipping forward so as to make a visible projection in certain motions of the arm, and in certain other motions returning to its natural situation.

"I may take this opportunity of noticing another circumstance, which, though not of much interest in pathology, is of some importance in practice. An abscess originating in the shoulder-joint sometimes presents a peculiar appearance, when it is making its way to the surface. A dissection, which I once had the opportunity of making, will explain at once the nature and the cause of this peculiarity. The abscess, taking the course of the tendon of the long head of the *biceps flexor cubiti* muscle, had suddenly emerged from the joint at the lower end of the bicipital groove of the humerus: then, having taken a direction forward, on the anterior edge of the deltoid muscle, had presented itself under the integuments, having a spherical form, so that it might have been mistaken for an encysted tumour. I met with one case, in which this mistake respecting an abscess of this kind was actually made by a surgeon of considerable experience, who proposed the removal of the tumour by the knife." (pp. 205-7.)

Chapter VI, on *Necrosis of Joints*, is a new feature in the volume, at least in its present form. The disease is met with in severe scrofulous caries of the joints;—a piece of bone dies, becomes detached from the shaft of the bone of which it was a part, and lying loose in the articulating cavity, occasions great irritation. If the affected joint is superficial and of small size, something may be hoped for by patience and time; but, should a large articulation be the one affected, Sir B. Brodie is of opinion,

that as soon as the disease is clearly recognised, the patient should be advised to submit to amputation, for there is no reasonable prospect that the limb can be preserved in a useful state.

Chapter VII. *Chronic Disease of the Joints connected with Gout and Rheumatic Gout.* We are sorely tempted to transfer this chapter entire to our columns, in the fear lest an attempt at abridgment may rob it of the style and completeness which constitute no small part of its value. The disease of which it gives an account is one often met with in persons who have led luxurious and inactive lives, and consists essentially in the deposition of the lithate of soda in the various textures of the joints. It commences with inflammation of the synovial membrane, followed by the deposition of lithate of soda, either "underneath the synovial membrane, on the bone near the margin of the cartilage, on the surface and in the substance of the cartilage, in the cancelli of the bone, and in the cellular tissue external to the joint." (p. 237.)

Soon after, the cartilages of the joint disappear, and the surfaces of the bones are exposed; but these do not become carious, as under ordinary circumstances they would do,—a process of hardening seems to go on, at the same time that they are subject to friction in the motions of the joint; and the result is, a porcellaneous transformation of the bony tissue into a substance like ivory.

The treatment of the disease, it is evident, must be directed to the elimination from the system of those gouty materials, the deposition of which forms the most striking feature of the complaint.

Chapter VIII, *on Loose Cartilages and Excrescences in the Cavities of Joints*, is hardly so full or satisfactory as we could have desired, and contains nothing sufficiently novel to require our dwelling upon it.

Chapter IX. *Malignant Diseases, and other Morbid Growths connected with the Joints.* What shall we say respecting malignant diseases, the opprobria of medicine and surgery? With all our labour, do we now possess any information that has advanced us one step in the cure of them?—In our consciences believing in nothing more firmly, than that we know nothing about them, we were not surprised to discover the little advance that Sir Benjamin's opportunities have enabled him to make. He has seen and relates many excellent examples of what are called malignant diseases, and that is all.

Chapter X. *Neuralgia of the Joints* is generally connected with hysteria, and requires moral as well as professional management. It is in this class of affections, as Sir Benjamin remarks, that mesmerism and homœopathy are often successful, by the strong mental impression they make; the same result being sometimes brought about, he tells us, by a supposed "divine interposition," and the commands of spiritual instructors.

Chapter XI, *on Chronic Abscess in the Articular Extremity of the Tibia*, has not received much accession since the last edition; but it contains the same amount of real solid information, and altogether affords a highly favorable specimen of the author's powers of observation and inductive reasoning.

Chapter XII, *on Caries of the Spine.* It is probable that the joints of the vertebræ are subject to like diseases with those of the other articulations; but examples of disease in the soft parts alone are, for obvious

reasons, uncommon. Sir Benjamin has never observed, either in the living or in the dead, a disease comparable with those affections of the synovial membrane, which are so common in the joints of the extremities; but he has met with primary uncomplicated disease of the intervertebral cartilages, for which there is no other name than that of ulceration. Caries of the spine from scrofulous disease is, as every one knows, or can easily learn in a very short experience of the surgical practice of an hospital, a disease of great frequency and of sad result. Sir Benjamin says that there are three forms of it: 1st. The scrofulous degeneration that is common in the other joints; 2d. Ulceration of the articular cartilages, and of the surfaces of bone in immediate contact with them; and 3d. A variety less commonly met with, viz., chronic inflammation of the bodies of the vertebræ, and subsequent ulceration of the intervertebral cartilages. Whatever be the commencement of the disease, the result is the same; the bodies of the vertebræ and the intervertebral cartilages are destroyed, either with or without coexistent affection of the spinal chord; and the only cure is that established by nature in a permanent curvature of the spine. The cases in which the spine becomes secondarily affected, in consequence of the pressure of tumours, as (for example) aneurisms, are of course not included in this account. The symptoms of this disease are too well known for us to dwell upon them; it has often advanced so as to destroy some considerable part of the vertebræ, before it is discovered; and Sir Benjamin Brodie has known the first sign of its existence to be the sudden appearance of an abscess in the groin or back, which has been mistaken for an encysted tumour. Tenderness of the spine is generally present in advanced stages of the disease; but at an earlier period it is often absent, and is therefore not an infallible means of diagnosis. The author also remarks, that there are few young ladies in the higher ranks of life, who will not complain of tenderness in some part of the spine if firmly pressed upon; and therefore it is advisable, before forming a positive conclusion, to make the patient jump off a table or chair, when the jar to the back will discover tenderness that might else escape detection.

In the treatment of caries of the spine, absolute repose, continued for a lengthened period, as for a year or two, is imperative upon the patient. As regards other measures of a more violent kind, it is well that we should lay before our readers the following passage from the work before us:

“In the early part of my professional life, I was led to follow the practice which was then very generally adopted of treating caries of the spine by means of setons and caustic-issues, one on each side of the diseased vertebræ. A more enlarged experience has satisfied me that, in the very great majority of cases, this painful and loathsome mode of treatment is not only not useful, but actually injurious. The observations which I made on this subject formerly, with reference to scrofulous diseases of other joints, are equally applicable to cases of scrofulous diseases of the spine. For many years past I have ceased to torment my patients who were thus afflicted in this manner, and I am convinced that the change of treatment has been attended with the happiest result. There are a few cases only in which I am still inclined to believe that issues or setons may be employed with advantage. The cases to which I allude occur almost exclusively in adult persons, where there is severe pain in the seat of the disease, and where from this and other circumstances we are justified in the conclusion, that the caries depends, not on a scrofulous condition of the bones, but on disease of the intervertebral cartilages, or a chronic in-

flammation, probably of rheumatic origin, of the vertebræ themselves. Even in these last-mentioned cases, issues and setons do not seem to form any necessary part of the treatment; and I am not myself in the habit of resorting to them unless I find that the pain is not relieved by the recumbent posture, and the other remedies of which I shall speak presently. It appears to me also, that in cases of caries of the spine, as well as in those of caries of other joints, issues and setons are to be employed only in the early stage of the disease, and that no advantage is to be expected from them after abscess is formed." (pp. 345-46.)

Of course it is understood that the greatest attention must be paid to the general health of the sufferer, and to strengthening the constitution by the use of the various tonics.

This chapter also includes a notice of *some Cases which are liable to be mistaken for those of Caries of the Spine*. The principal affection to which allusion is here made, is one in which, from weakness in the muscles supporting the spine, the whole vertebral column becomes arched in the form of a half hoop. Such cases were first described by Mr. Henry Earle, and in some instances are so remarkable, as to deceive even a careful observer, who may suppose them to be examples of angular curvature.

There is another class of cases, also, which does not produce obvious symptoms by which it may be recognised during life; namely, an ankylosis of several vertebræ, and the deposit of a bridge of bone between the bodies of them. Sir Benjamin details one interesting case, in which the symptoms were such as to lead him to suspect the existence of this disease; and in which a decided improvement took place under a course of treatment, which essentially consisted in the administration of mercurials, at first in full doses so as to affect the mouth, and afterwards in alterative courses.

In this division of the subject, also, the author narrates some good examples of malignant disease of the spine.

Chapter XIII is *on some Diseases of the Joints not included under the foregoing heads*; and the concluding chapter is *on Inflammation of the Synovial Bursæ*, but we are unable to do more than indicate their titles.

In bringing to a close our notice of Sir Benjamin Brodie's Treatise, we would remark, that the very uniformity of its excellence, with the exceptions we have noted, has rendered it difficult for us to do it that justice which it deserves. Where every subject is so admirably treated, the selection of passages for quotation seems meaningless; and where the entire work is the condensed result of the long and varied experience of its distinguished author, further condensation could not be effected without injury. No less as a model of a scientific and yet truly practical Treatise, than as a first-rate monograph on the subject on which it treats, would we recommend it to the perusal of our readers, and more especially to such as may be excogitating works of their own.—*Oh, si sic omnia.*



## ART. XII.

1. *Of the Causes, Nature, and Treatment of Palsy and Apoplexy ; of the Forms, Seats, Complications, and Morbid Relations of Paralytic and Apoplectic Diseases.* By JAMES COPLAND, M.D. F.R.S., &c. &c.—London, 1850. 8vo, pp. 414.
2. *The Causes, Symptoms, and Treatment of Eccentric Nervous Affections.* By WILLIAM JOHN ANDERSON, F.R.C.S.—London, 1850. 8vo, pp. 199.
3. *Traité Pratique des Maladies Nerveuses.* Par C. M. S. SANDRAS, Agrégé de la Faculté de Médecine de Paris, Médecin de l'Hôpital Beaujon, &c.—Paris, 1850.
- A Practical Treatise on Nervous Diseases.* By C. M. S. SANDRAS, Physician to the Hospital Beaujon, &c.—Paris, 1850. 2 vols. 8vo, pp. 636, 564.
- 4, i. *The Lumleian Lectures for 1850.—On the Pathology and Treatment of Delirium and Coma.*
- ii. *Lectures on the Treatment of Delirium and Coma (in Sequel to the Lumleian Lectures for 1850).* By ROBERT B. TODD, M.D. F.R.S., &c. ('Medical Gazette' and 'Medical Times,' 1850.)

It is one of the functions of a medical reviewer, to inquire into the ends an author purposes to attain by the publication of his work, with a view to ascertain how far those ends are conducive to the advancement of the science and art of medicine, and how far to the promotion of the personal interests of the author. If the reviewer find that the latter is the sole or the chief object sought by the publication of the work, the author cannot reasonably expect that he should occupy his time and the pages of the Review by any but a very slight notice ; if, however, there be a *bonâ fide* attempt to advance medical science, although it may be unsuccessful, the author is entitled to consideration. In the latter case the reviewer may reasonably require something *new*, as well as *true* ; for a mere republication of current doctrines, or of observations that are familiar to the practitioner, however sound and correct, can hardly be said to advance science or art, since it is only a useless addition to the mass of repetitions that already encumbers the shelves of the medical library.

We have thought it due to Mr. Anderson to make these preliminary observations ; because we recognise in his work one of those publications which are rather calculated to impress the reader with a favorable opinion of the writer's acquirements and abilities, than to impart to him any new information. Mr. Anderson appears to have been a diligent case-taker at St. George's Hospital when a student ; and after passing his examination for the membership of the College of Surgeons in 1848, to have continued his studies and obtained the diploma of Fellow. It would appear that he then occupied his leisure in the composition of the present work, which he dedicates to Dr. Seymour, "in grateful acknowledgment of the kind and valuable instruction received from him."

I. The contents and plan of the work are stated by the author in the following extract from his preface :

"In the following pages, the author has endeavoured to set forth, briefly, the

causes, symptoms, and treatment of eccentric nervous affections; that is to say, of such complaints as originate in causes extraneous to the nervous centres, and are totally unconnected with any organic lesion of those parts. In the first place is described the intimate connection which exists between the nervous and circulating systems, as well as their mutual dependence the one upon the other. The processes of assimilation, both primary and secondary, are adverted to, in order to point out the manner in which the blood is formed, and from it the various tissues of the body, and how, consequently, any derangement of these processes must affect the constitution."

The physiology of the circulating and nervous system is thus made the basis of Mr. Anderson's views, and we have accordingly a synopsis of that physiology in Chapter I. Why the author should think it necessary to give the anatomy as well as physiology, does not appear; but he duly prints and publishes facts, which a first-year student must be a dull youth indeed not to know well.

We pass from this chapter of the merest elementary anatomy and physiology to the chapter in which "the difference between centric and eccentric nervous affections is pointed out, and some experiments are related in order to show the difference between reflex action and the motions of irritability." We are informed, on the authority of Dr. Hall, that "diseases of the nervous system may be divided into those which have their origin at the nervous centres, and those which originate at a distance from those centres,—into those of centric and those of eccentric origin." In the author's own words:

"Eccentric nervous affections are exceedingly numerous, depending upon a multitude of different causes, and varying in degree of intensity, from trifling transitory symptoms, up to the most formidable and inevitably fatal diseases to which the human frame is liable. They depend upon causes remote from the nervous centres, and may be induced by direct irritation either of the peripheral extremities of nerves, or of the nervous trunks themselves connected with the cerebro-spinal system. The sympathetic system also is liable to be affected in a similar manner." (p. 43.)

From this passage we inferred, that Mr. Anderson proposed to limit his labours to a consideration of diseases of the nervous system having a peripheral origin; but we found, on turning the leaf, that those which originate from the circulation of poisoned blood through the nervous centres, or that are excited by mental influences, are included under the true eccentric.

"Derangement of the nervous system will predispose to, if not produce, the same effect; blood which is insufficient in quantity, and of an improper quality, as in anæmia, together with the opposite state of plethora, in which the blood is of too stimulating a nature, will both hereafter be shown to act powerfully in this manner.

"Poisons of various kinds, whether generated in the system itself, or received into the circulation from extraneous sources, give rise to them; and at last, though not least, the mind itself, acting through the medium of our corporeal senses [?], will produce some of the most alarming and dangerous of them all." (p. 44.)

We are at a loss to understand how it can be reasonably stated, that a cure of hemiplegia from mental shock (as detailed by our author), can be classed with diseases "originating at a distance from the nervous centres," or dependent "upon causes remote therefrom," or with those "induced by direct irritation of the peripheral extremities of nerves, or of the nervous

trunks themselves." It appears to us, that an arrangement of this kind is merely verbal, and has no foundation in pathology.

Be this as it may, Mr. Anderson treats, in four successive chapters, of hysteria, hypochondriasis, and chorea, of epilepsy, catalepsy, and ecstasy; of nervous collapse (or "shock"), traumatic delirium, and tetanus; of delirium tremens and hydrophobia. The text is illustrated by cases, and by details of some common-place experiments made on unfortunate frogs—made to illustrate what no one doubts, who possesses even a slight acquaintance with modern neurology;—we may add, the cases detailed are equally common-place with the experiments, and the pathological and therapeutical views of the author present nothing novel. We need hardly say, that this work contributes nothing new to the art or science of medicine, and will do nothing for the real reputation of the writer. We regret that a sense of duty compels us to record an adverse opinion; but we are not without a hope that Mr. Anderson will only the more vigorously pursue his studies in neurology, and *seek to be original*. We will then give him a hearty welcome, and we feel satisfied that on his part he will then acknowledge the justice of our present criticism.

II. Dr. Copland is no stripling in medical literature; his 'Dictionary of Practical Medicine' is too well known and appreciated to render any introductory observation from us necessary; nevertheless we must inquire, even in relation to his labours, to what end has he written this volume; and the inquiry is the more germane to the matter, inasmuch as his great Dictionary is still unfinished, amidst the reiterated complaints of the subscribers to the work. The reasons should be weighty for thus turning aside from the fulfilment of an imperative duty (as we hold the earliest possible completion of a serial work to be), and undertaking the minor task of writing a monograph, especially when a large portion of that monograph has been already published in the first and third volumes of the Dictionary itself. We gather from the Preface, that his views in their entirety have not as yet been published. Dr. Copland observes:

"A considerable part of the following Treatise was published many years ago in the first and third volume of the author's 'Dictionary of Practical Medicine,' and several of the chapters on the connection of Paralytic and Apoplectic seizures, with other disorders, formed the Croonian Lectures for 1846 and 1847 at the Royal College of Physicians. The author now publishes the whole in a connected form, believing that palsy and apoplexy should be studied in intimate connection, not only with each other, but also with other diseases, of which they are often the consequences and terminations, and with other affections which are frequently associated with them."

We do not think that the subscribers to Dr. Copland's great Dictionary will be quite satisfied with this statement, or will think it necessary to add this volume also to their libraries. The non-subscribers, however, will find in it a most complete monograph on the subjects on which it treats, and an excellent work of reference. We hope at some future time to examine Dr. Copland's doctrines in detail, and submit a critical analysis of them to our readers.

III. M. Sandras professes to give a purely practical treatise on that large class of diseases, in which the functions of the nervous system are deranged, without our being able, in the present state of our knowledge, to trace the derangement to a material, real, and necessary change in the

system itself. His opportunities for the observation of this class of affections have been numerous, for his colleagues at the Hôtel Dieu have been accustomed to send to him all the most interesting cases of the kind which presented themselves for admission into the hospital. He has endeavoured to make his work hippocratic in its character, by excluding at least the most recent bibliographical inquiries, by avoiding details of reported cases, and by neglecting pathological anatomy and physiology. The doctrines of reflex action are quite ignored, as are, indeed, almost all the neurological writers and inquirers of the last twenty years. Nevertheless, the book is a useful practical work, and is not without its value as a work of reference. M. Sandras is evidently a close observer and an original thinker.

The arrangement adopted is simple. Firstly, the author treats of General Diseases of the Nervous System, in which he includes "l'état nerveux," or the nervous diathesis, cachexia, or condition: and which is a fundamentally predisposing cause of other diseases. Nervous fever,—periodic nervous affections,—epilepsy and its congeners,—leipothymia,—eclampsia and hysteria,—and tetanus and hydrophobia,—all come under this head. Special nervous diseases are next considered, in succession, as they involve the brain and the nerves. This arrangement is well enough for the author's purpose, since he limits the diseases of which he treats to the functional only; but he thereby renders his work imperfect as a treatise on diseases of the nervous system. The basis of the arrangement being etiological, it is necessarily imperfect, because our etiology is imperfect; and it appears to us that M. Sandras includes diseases in his work, which are found to arise in the great majority of cases from structural change. However, there is a good deal of useful matter in the work, and of a kind not usually met with in French writers of M. Sandras's class, and we shall refer to it again.

IV. Dr. Todd's lectures have, in some respects, the merit of novelty, and are certainly practical. Many of our readers will agree with Dr. Todd in his opinion as to the great difficulties the practitioner experiences in the diagnosis and treatment of the various forms of Delirium and Coma; as to the discordance of opinion touching their pathology; and as to the imperfection of our knowledge touching their true nature. These imperfections and difficulties arise necessarily out of the imperfections and difficulties of a true cerebral physiology; or, in other words, of the relations which the brain, as the seat of consciousness and the organ of the mind, bears to the mind on the one hand, and the vital forces on the other. We purpose, on the present occasion, to enter into a critical analysis of Dr. Todd's views; and we shall take as our guides the experience which we have acquired of this class of diseases by a not very limited range of practice,—the facts which our reading has supplied,—and the conclusions to which our own meditations on the results of our reading and observation have led us.

*Coma* is the total abolition of sensation and perception. External impressions no longer excite sensations; internal changes in the cerebrum are no longer developed into the perception of self-existence. Or, as Dr. Todd more diffusely defines it, coma is a complete suspension of that mutual influence of the mind and of the organ of consciousness, in which, speaking physically and physiologically, our consciousness exists,—a suspension which (in Dr. Todd's opinion) begins with the physical organ,

and therefore involves the powers of thought and of perception, so that the comatose patient neither wills, nor feels, nor thinks, and he awakes from the state as from a sleep, not conscious where he had been, or how he had been, and feeling as if during a certain interval he had ceased to exist.

*Delirium* is in some respects almost the converse of this state. Consciousness, it is true, is abolished in the most intense form, and yet the thoughts appear preternaturally active; but Dr. Todd's description is so clear, that we subjoin it.

"The state of delirium, in its highest degree, is a complete disturbance of the intellectual actions; the thoughts are not inactive, but rather far more active than in health; they are uncontrolled, and wander from one subject to another with extraordinary rapidity; or, taking up one single subject, they twist and turn it in every way and shape, with endless and innumerable repetitions. The thinking faculty seems to have escaped from all control and restraint, and thought after thought is engendered without any power of the patient to direct or regulate them. Sometimes they succeed each other with such velocity, that all power of perception is destroyed, and the mind, wholly engrossed with this rapid development of thoughts, is unable to perceive impressions made upon the senses; the patient goes on unceasingly raving, apparently unconscious of what is taking place around him; or it may be, that his senses have become more acute, and that every word dropped from a bystander, or every object presented to his vision, will become the nucleus of a new train of thought; and, moreover, such may be the exaltation of his sensual perception, that subjective phenomena will arise in connection with each sense, and the patient fancies he hears voices or other sounds: ocular spectres in various forms and shapes appear before his eyes, and excite further rhapsodies of thought." (Lecture I.)

Such are the phenomena which Dr. Todd proposes to elucidate by clinical research and physiological considerations; and having thus elucidated them, to "describe the treatment suited to the various forms of them, which is most accordant with reason and experience." Although we cordially welcome the results of Dr. Todd's inquiries, we cannot agree with him in his low estimate of the labours of his predecessors, when he states that, after carefully looking through the literature of his subject, he has failed to discover anything like a full discussion of the pathology of these affections founded upon careful *clinical* observations. Nor do we think that he is quite accurate when he states that "writers and practitioners seem to rest content with the opinion, that all comatose and delirious states are referable to various degrees of congestion of blood in the blood-vessels of the brain, or to various quantities of fluid poured out in the sub-arachnoid cavity, or in the cerebral ventricles." Toxic delirium, that is to say, delirium arising from the circulation of a poison through the brain commingled with the blood, has been recognised from time immemorial. Andral has pointed out the connection between anæmia and delirium; and delirium from an irritation of the mucous surfaces transmitted to the brain, and immediately dependent upon the operation of the irritation on the cerebral tissue, is a form familiar to ourselves. Besides, the two forms of intellectual disturbance, mania and delirium, are so closely allied, that the humoral pathology of the former may be readily applied, and is so applied, *mutatis mutandis*, to the latter. Indeed, no statement is so common in the necroscopic histories of cases of delirium, as that no change in the brain could be discovered, except in the allied disease we have mentioned. In short, it is as familiar to pathologists "as household words,"



that in *post-mortem* investigations of comatose, maniacal, and delirious cases, it is quite as much the rule as the exception to find neither congestion nor effusion within the cranium. To such cases the term nervous delirium is applied by M. Sandras, and its causation attributed to a nervous diathesis, or a proclivity to diseases of the nervous system—*état nerveux*. On this point M. Sandras observes, after referring to delirium excited by mental emotions :

“This naturally leads us to speak of delirium caused by the nervous condition. It is through this that the passions and abstractions excite delirium. There is therefore nothing surprising in the circumstance that the condition itself leads to delirium, as is, in fact, the case. It is then that a very acute physical suffering is followed by delirium ; prolonged fasting, long-continued agitation of mind, excess of pleasureable excitement, especially the sexual, chlorosis, habitual neuropathy, and a thousand other circumstances, will lead to the same result. These are the forms of delirium which the practitioner meets with daily ; and respecting which precise information is useful to him, for without it he is in danger of falling into serious errors in treatment.” (vol. i, p. 580.)

M. Sandras includes hysterical and mesmeric delirium under this head, together with traumatic, puerperal, febrile, and toxic delirium ; observing, that they occur without its being possible to attribute them to structural change in the organs subservient to mind or their appendages ; and that it is of vast importance to note the difference between these forms of delirium, and those which are merely symptomatic of structural change.

Both Dr. Todd and M. Sandras have experienced the difficulty of arranging the various forms of delirium which come under the notice of the practitioner. Manifested in every degree of intensity, and dependent upon so large a number of exciting and predisposing causes, they can only be grouped under general heads. The arrangement which Dr. Todd has adopted is simple. He recognises ten forms, namely :

1. The epileptic and choreic delirium.
2. The renal epileptic delirium.
3. The hysterical delirium, and that of over-worked men.
4. The puerperal delirium.
5. The anæmic delirium.
6. The traumatic delirium.
7. The delirium of typhus.
8. The delirium of erysipelas.
9. The rheumatic and gouty delirium.
10. The toxic delirium.

In accordance with certain opinions to which we shall subsequently refer, Dr. Todd arranges the various manifestations of coma under precisely the same heads, so that each form of delirium has a corresponding form of coma. We shall consider them *seriatim*.

*Epileptic delirium.* Under this head Dr. Todd appears to class two or three forms of disordered mental function : namely, 1, what has been termed impulsive insanity ; 2, the prodromoi of the epileptic state ; and 3, the first stage of acute mania. The pathognomonic characteristic of epileptic delirium, he states to be the suddenness of its attack. It is not accompanied by any particular constitutional disturbance ; the pulse is accelerated, but not to any great extent ; its range is ordinarily from 80 to 100 ; it exhibits no character of strength, but is often full and throbbing.

If this delirium continues for awhile, it may induce exhaustion. Dr. Todd thinks it is "of frequent occurrence; and that in some of those instances in which, under a sudden impulse, persons are led to commit some dreadful deed, which is opposed to the whole tenor of their previous lives, it is the sudden access of epileptic delirium which has thus disturbed the balance of their moral nature." This delirious state may end in epilepsy, or coma, or may assume the character of mania. As we do not think that Dr. Todd has fully substantiated his views, we will subjoin his description of *epileptic delirium*.

"A man, hitherto healthy, fails somewhat in health, becomes dull and melancholy, takes a gloomy view of things, but still his ill-health is not sufficient to prevent him from following his usual avocations, nor is it noticed by any, save perhaps those who are constantly with him. There may or may not be some cause for this—some excess—some mental trouble or anxiety—some altered position of his affairs. Presently, either at night, or on first waking from sleep in the morning, or it may be while he is at his usual employment or business, he becomes strange and incoherent, talks at random, mistakes things and persons, writes odd letters; in short, he displays unequivocally by words and actions that the mind is disturbed.

"This state of delirium may speedily end in an ordinary paroxysm of epilepsy, with all its accompaniments, after which the patient resumes his wonted health; or it may continue for a considerable time, assuming even the character of violent mania, with sleeplessness, exciting the utmost terror among the patient's family and attendants; it may last even for days, and then the occurrence of an epileptic fit relieves all doubt as to the nature of the maniacal paroxysm. It may be, however, that these phenomena will occur with a patient who is subject to epileptic attacks; in which case, if the fact be made known to the medical attendant, he will have the less difficulty in recognising the true nature of the paroxysm.

"And it may also happen that the delirium may pass off, or it may terminate in coma, from which the patient may waken up restored, without, in either case, the occurrence of any *convulsive* attack of epilepsy.

"The delirium, in cases of this description, is in general of the most decided kind; and it often amounts to mania. The patient is wakeful, noisy; sometimes mischievous; sometimes muttering, and incoherent, and unintelligible; sometimes distinct and easily understood: the subject of his ravings being determined by circumstances or events, which had previously more or less occupied his mind." (Lecture I.)

Dr. Todd details some cases which he refers to this head of epileptic delirium, and which give some countenance to his statement as to the suddenness of the attack. Mere *suddenness* of attack, however, is not peculiar to epilepsy, or to the epileptic delirium. In fever, the onset of the delirium is frequently without warning; and suicide or homicide has been committed by the patient, with but little amount of delirious raving. Neither, on this point, are the cases detailed by any means conclusive; and we cannot therefore think, with Dr. Todd, that in any instance of this kind, the paroxysm comes on "without previous disorder, or without warning of any kind like the epileptic paroxysm." Indeed, we doubt whether it is the rule that even epilepsy attacks "without warning of any kind," in those cases in which the disease has not impaired the intellectual functions, or is uncomplicated with idiocy or imbecility. In the illustrative cases detailed by Dr. Todd, the evidence as to this point is either adverse to his idea, or merely negative. His first case is that of a medical gentleman, who went up stairs to dress previously to dining out, and within five minutes was found by his housekeeper sprawling on the floor in a paroxysm of mania,

shouting as if he had been assaulted by thieves. This case soon proved to be of the epileptic kind. Now all the etiological evidence we have is, that previously "he had evinced no particular symptom of illness, but suffered some degree of mental anxiety." The next case is that of a man, who, "without any assignable cause, became delirious at night." He had not been drinking, nor had he been overworked, but his wife thought he had been dull and depressed for the three or four days before the attack. This also proved to be epileptic. The third case is more specific than the preceding. A tailor, while at work, is seized with giddiness and confusion of ideas. He ran out of the house in which he worked, and, without knowing what he did, ran up and down the street. He continued in this state for a quarter of an hour, and then came to himself, feeling for some time afterwards depressed and shivering, with a mistiness before his eyes. Next day he was attacked with the same symptoms, but in a less degree; he was soon cured by a course of purgatives and regulated diet. Here the evidence as to the total absence of premonitory symptoms is entirely negative; as it is in the next history of a country carman, who, while walking alongside his horse, was observed by the policeman on duty to be bewildered and unconscious where he was going. Being taken to the hospital, he continued violently delirious for about twenty-four hours, and then fell into a comatose state, from which he recovered in twenty-four hours more, after the use of simple remedies. This man "had never had any similar attack previously, nor was there any evidence of intemperance;"—an etiological statement altogether insufficient for the purposes of clinical research or scientific classification. So violent a disturbance of the cerebral functions, we should think, *must* have been preceded or followed by other pathological phenomena, if the seizure were epileptic in its nature; but we should *guess* the case to have been *toxic* rather than epileptic. The next example is that of a pale, delicate lad, with large head, who, about once in three months, whilst at work, is seized suddenly with giddiness and confusion of thought, followed by a delirious state, in which he talks incoherently, and his pupils become largely dilated; the delirium passes into a state of stupor and drowsiness, and considerable weakness. Iron and quinine were useful in this case.

We regret that we cannot agree with Dr. Todd in the deductions he draws from these cases; indeed, the details are so few, that the reader can hardly be warranted in drawing any deductions from them whatever. We, however, subjoin Dr. Todd's instructions as to the diagnosis of epileptic delirium:

"For the purpose of this diagnosis, you must avail yourselves of both negative and positive evidence. By the former you will be able to exclude the various other forms of delirium which I have enumerated. The history, and the absence of certain obvious phenomena, will denote that it is neither erysipelatous nor typhoid, nor pneumonic, nor rheumatic; and the absence of certain other symptoms, as pain in the head, sickness, sluggish pulse, and the non-existence of the tendency to coma, will point out that the symptoms are not due to inflammation of the brain or its membranes. Then you must satisfy yourselves that it is not delirium tremens; in which you will again derive much aid from the history of the patient, and from ascertaining whether he has been intemperate in his habits or not; also from the absence or presence of the peculiar tremor in the voluntary actions, and from the character of the delirium, which in delirium tremens is generally of the busy kind.

You will further inquire, whether the patient's delirium may not be of the hysterical kind, to which I referred in a former lecture; whether he had not subjected himself to exhausting influences, sexual or otherwise, and so given rise to the delirious state.

"Having thus determined that the delirium under which your patient labours is not to be referred to any one of these varieties, it is highly probable that it must be of the epileptic kind; and you must now look for some positive signs to prove that it is so. The aspect of the patient will afford you some help. There is, in these cases, a peculiar haggard, wearied aspect of the countenance, with dilated pupils, which should always excite your suspicions as to the epileptic nature of the disease. The character of the delirium is also to be taken into account: it is almost always of the noisy and violent kind; the patient is uproarious, to use a common expression, wakeful, and talkative. Then, if previously the patient have suffered from regular epileptic paroxysms; if the delirium have been ushered in with a fit; if epilepsy be distinctly a feature of the family history; if the convulsive fit have occurred in the course of the paroxysm of delirium;—any or all of these points will assist you greatly in determining the epileptic character of the delirium." (Lecture VI, at King's College Hospital.)

We leave our readers to judge for themselves, as to how far Dr. Todd has applied his own principles of diagnosis to the cases he has related; and proceed to notice his method of treatment. His plan is simply that which he would apply to a prolonged epileptic fit. It must be mainly supporting and expectant. As there is no inflammation, the patient is not to be harassed by venesection or cupping, or the administration of antiphlogistic remedies. When the pulse is weak, vacillating, irregular, and more especially when it is quick and running, then stimulants, carefully given, will prove advantageous. The scalp may be shaved, and, if it be hot, cold water applied, yet not so as to depress the heart's action. Small blisters may also be applied to it in succession; and the patient prevented from injuring himself or others by the gentlest possible means. If there be sleeplessness, and a sedative be required, opium is not to be administered, but henbane or hop. Sometimes the cold douche will be found useful; and Dr. Todd has seen chloroform of great use where opium had utterly failed. Tonics should be administered, and by preference zinc, iron, and quinine or cinchona.

*Renal epileptic delirium* is a term applied by Dr. Todd to those cases of epileptic delirium in which there is albuminous urine,—either only at the commencement of the attack, and disappearing as it goes off,—or, as more frequently happens, continuing throughout it and after it, and pathognomonic of structural disease of the kidneys. The prognosis in cases of this kind is unfavorable.

*Choreic delirium* is developed in the latter stages of chorea, tending to a fatal termination. In a case treated successfully in King's College Hospital, the delirium supervened upon chorea in a youth, aged 20, who, eleven years previously, had chorea, which affected his intellect to such a degree, that he became almost idiotic. He recovered, and, seven years after, had another attack. In this (the *third*) attack, the delirium came on about a month after the chorea, and was of the kind known as delirium *ferox*. It continued several days, and the state of exhaustion was so great, as to require a considerable amount of stimulants frequently repeated, to which was added, cold affusion twice a day for three or four days. In a fortnight the delirium ceased.

*Hysterical delirium.* Dr. Todd details one or two interesting examples

of this form of disease, the affinity of which to epileptic delirium he recognises, in common with the majority of writers. The case of a young lady, aged 22, is interesting, from the circumstance that the phenomena observed during life were those of acute disease, namely, acute pain in the head, constant vomiting, susceptibility to light, strabismus in the right eye (which had already a "natural cast"), delirium, alternating with a semi-comatose state, and, finally, complete coma, ending life on the eleventh day. Yet, on examination of the encephalon, the brain and its membranes were found "in a healthy state. The pia mater was well injected, but not more than might naturally be expected in a young person of active mind, and with a well-developed brain, and the arachnoid was perfectly natural." We do not feel inclined to go along with Dr. Todd in his doubts as to the correctness of his first diagnosis, and therefore would not adduce this case as an example of *hysterical* delirium. Yet we fully agree with Dr. Todd, "that it would have been better had the patient been spared all antiphlogistic treatment," because we believe, that, in all cases in which the nervous system is predominantly active or susceptible (as in this case, for the patient is reported as of "a highly hysterical diathesis"), depletion by bleeding and purgation, and the administration of active remedies, especially mercury, are of doubtful use, and are often really dangerous. We have seen a paroxysm of delirium excited in those cases of insanity, in which it is paroxysmal in its occurrence, by the administration of calomel; and we know well, that, in individuals of the nervous temperament or diathesis, comparatively small doses of mercury, whether in the form of the protoxide or the chloride, will induce great irritability of temper and various other phenomena, which are seen to usher in actual delirium. This case, like the others, is so incomplete in its details, that we do not feel justified in expressing any decided opinion as to its real nature; but we should surmise, that the case was one in which the delirium was sympathetic of, or consecutive upon, some latent disorder elsewhere. No account is given of the condition of other organs, and they were probably not examined. It is worthy of note, that the menstrual function had been habitually irregular; and we are strongly inclined to believe, with Dr. Todd, that in many instances of such irregularity, in which the deficient secretion is due to a local disturbance of function, that fails to eliminate from the circulation something which ought to be removed from it, the hysterical disorder of the nervous centres is *toxic* in its character.

Men are liable to hysterical delirium; and Dr. Todd has observed it in overworked professional men, in students, and in hard-working artisans, even when of temperate habits. We do not think, however, that Dr. Todd has selected the most typical forms for illustration. A solicitor, of gouty habit and highly sanguine temperament, having been unusually engaged in business of an anxious kind, was attacked with symptoms of catarrh, extending to the bronchial tubes, but neither violent nor extensive. This, however, Dr. Todd remarks, had a most depressing effect upon him; and on the third or fourth day he became violently delirious, which state continued more or less for a week, and passed off with profuse sweats and long sleeps, leaving the patient in a state of great exhaustion. Dr. Todd does not inform us on what grounds he established his diagnosis in this case; but we cannot perceive any one symptom on which his decision, that it was one of *hysterical delirium*, can be legitimately founded. Was



catarrh epidemic at the time? for, if so, the case would clearly be classed with those cases of epidemic catarrh (or influenza) which were complicated with delirium, and were not unfrequent in persons of a highly susceptible or overworked nervous system. Was it complicated with a wandering gouty affection? for, if so, then it might be classed with another form,—the gouty or rheumatic delirium. The second illustration is similar to the first. A teacher of music, working very industriously at the studies necessary for his profession, and having, at the same time, his mind much engaged on religious subjects and greatly excited by preachings, is attacked by catarrh of a slight kind, but accompanied with considerable prostration of strength. Delirium was developed in a few days, having a religious character. He was treated (as regards the delirium) morally, that is, as if he were insane; and in a fortnight he had completely recovered. In this case, as in the preceding, we fail to recognise anything to which the term *hysterical* should be given.

*Puerperal delirium*, Dr. Todd thinks, is essentially of the same nature as hysterical delirium, which it very closely resembles; he applies the term, however, to what is usually known as puerperal mania, and quotes, as to its pathology, from Gooch and others. We do not find any notice of the frequent co-existence of *renal disease* with puerperal delirium.

*Anæmic delirium* occurs in cases in which the blood is imperfectly formed, or in which there has been profuse hæmorrhage. It will occur in extreme cases of chlorosis. Both the puerperal and hysterical forms are nearly allied to it. Dr. Todd mentions the following monitory case:

“This form of delirium is not unfrequently preceded or followed by attacks of violent convulsions. I related a remarkable case of this kind in the Lumleian Lectures of last year. The patient was a delicate woman, who miscarried, with some hæmorrhage; after this she became thin and pale. While in this state, she experienced some giddiness of the head, as well as slight delirium, for which she was bled and had leeches applied. Owing to the giving way of the bandage on her arm, and the application of additional leeches to relieve the supposed congestion of the head, she lost still more blood; her convulsions recurred, and she became delirious and maniacal.” (Lecture I.)

Andral mentions delirium as one of the leading symptoms of anæmia of the nervous centres,—a state characterised by a preternatural whiteness of the gray matter, causing it to appear as if macerated, and sometimes attended with induration, sometimes with softening. He quotes M. Papavoine's cases of children carried off amidst violent delirium, in which the brain was found remarkably pale. Andral describes it as analogous to the delirium arising from too spare diet, or which occurs when the brain is deprived of stimulants to which it has been accustomed. Great sensibility of the surface is also mentioned as accompanying this anæmic state.

We had hoped that few practitioners of the present day, if acquainted with the labours of Dr. M. Hall, were unaware of the effects of profuse hæmorrhage and other exhausting agencies on the cerebral functions; and we fully agree with Dr. Todd in his observation, that cases like the one he describes will be more rare, when a more general assent is given to the doctrine, that congestion of the brain will not account for giddiness and delirium, and other signs of disordered function within the cranium.

*Traumatic delirium* is a form well known to practitioners and students, and we need hardly refer the reader to Dupuytren's description of it.

The delirium of *Typhus* and that of *Erysipelas* are closely allied, inasmuch as in both forms we have an animal febrific poison present in the blood. That which complicates typhus is, however, more rarely *ferox*, or raving, than the erysipelatous; but is rather the *D. desipiens* or *D. mite*.

*Rheumatic delirium* is that form which complicates rheumatic fever. It is thus described by Dr. Todd:

"A patient is seized with all the ordinary symptoms of rheumatic fever, and he goes on without any untoward symptoms—it may be for only three or four days, it may be for a week, or even later—when the nurse having reported that he passed a restless night or two, and wandered more or less, we find him delirious, raving, talking wildly, and, as in the traumatic delirium, entirely disregarding his hitherto exquisitely painful, and still swollen joints. The tendency in these cases is to the acute maniacal state, and to wakefulness; so that frequently the patient requires restraint, and always the closest watchfulness.

"As in the other acute forms of delirium, patients often die suddenly in this, evidently from exhaustion. Sometimes they quickly fall into a state of profound coma, which lasts from one to twenty-four hours, and terminates in the death of the patient. I suspect that moving patients from one place to another, in rheumatic fever, is apt to bring on this mode of termination; for I have had several cases, in which a patient was brought into the hospital late in the afternoon, having been three or four days ill of rheumatic fever, and in the course of the night he became delirious, and then comatose, and died." (Lecture I.)

Dr. Todd also notes the complication of chorea-like jactitations with this delirium, affecting the upper extremities and muscles of the face, and sometimes bordering on tetanus. These "delirious" movements of the motor apparatus, if we may be permitted the term, often occur coincidentally with the first signs of pericarditis. The delirium is most frequently observed in those who are pale; depletion is altogether useless, if not injurious. *Gouty delirium* seems a closely allied form, occurring most frequently when the larger joints are affected.\*

The *toxic delirium* has its most complete type in *delirium tremens*, since that form can be clearly traced to the introduction of alcohol into the blood. Opium will also induce it, and chloroform, ether, and other volatile matters, as well as Indian hemp, henbane, hemlock, and the whole class of narcotic drugs:

"The poisons of the exanthemata, too, produce delirium; that form which often develops itself in the premonitory fever of scarlatina, measles, smallpox, is of this kind, and will often disappear as soon as the characteristic skin-affection becomes fully developed, &c." (Lecture II.)

Dr. Todd might have added the malarious and miasmatic class of poisons; also the poison of erysipelas, typhus, yellow fever, plague, &c. Perhaps we ought to notice also, in this place, that delirium and coma from jaundice and from extremes of temperature, whether of heat or cold, do not find a place in Dr. Todd's nosology. We are inclined to think that delirium from cold is by no means uncommon, and is from time to time mistaken, both by the police and by medical practitioners, for intoxication.

Having passed in review the various forms of delirium, somewhat in

\* At the moment of penning this paragraph, the newspapers announce the death of Count Brandenburg, the Prime Minister of Prussia. It appears that, after manifesting symptoms of gout, he was attacked with violent delirium, from which he never recovered, adding another instance to the many on record, of fatal cerebral disease from the overwhelming anxieties of political life. His malady seems to have been allied to the gouty delirium.

detail, we are in a position to notice their *comatose* analogues more briefly. Dr. Todd classes "congestive apoplexy," or simple apoplexy, with epileptic coma, and traces its distinct connection with renal disease, just as he found epileptic delirium to be sometimes so connected :

"The coma connected with renal disease, will come on under three forms :— 1. Suddenly, becoming profound, and passing to a fatal termination ; 2. Gradually, and also passing into the profound state ; and, 3. In paroxysms,—presenting an exact resemblance to the epileptic, either of coma simply, or of coma with convulsion, or of either or both, accompanied with delirium. The state of the urinary secretion also varies ; always, however, presenting a decided departure from the normal state, being either wholly suppressed or greatly diminished in quantity, or even increased in quantity, with low specific gravity." (Lecture II.)

*Coma* in the dropsy succeeding scarlet fever, and in *acute* dropsy, belong to this head, when an *acute* renal affection supervenes on *chronic* disease. Comatose affections of this kind are sometimes extremely slight, and constitute the evanescent *paralytic strokes* not unfrequent in occurrence. In this practical point we fully concur with Dr. Todd. Dysuria, or difficult micturition, from enlarged prostate or other obstacles, may also lead to this form. *Hysterical coma* is a well-known form ; we cannot fully agree, however, with Dr. Todd in his diagnosis of the case of Alfred Russon, which is related as an illustration of this kind. It appears to us to have been rather epileptic in its nature. We apprehend that Dr. Todd's idea of what constitutes hysteria is a comprehensive one, and includes states not usually classed under that term. *Traumatic coma* includes the coma resulting from concussion of the brain ; from severe burns and scalds, and other physical injuries ; and from a shock to the feelings. Acknowledging that *coma* will result from the compression on the brain induced by clot,—by dropsy of the ventricles,—by growths within the cranium,—Dr. Todd doubts whether it is ever induced by increased sub-arachnoid effusion. Physiologically, he thinks it established that the quantity of sub-arachnoid fluid is in the inverse ratio of the bulk of the brain ; and that the effusion of a large quantity is a *result*—probably of a conservative nature—of the shrinking or diminished bulk of the brain from some cause.

*Rheumatic and gouty coma* are so nearly allied etiologically to renal coma, that we need not do more than mention them as duly entering into Dr. Todd's classification. The only difference that we can discover is, that cases of the latter are not necessarily connected with gout or rheumatism.

*Toxic coma* from alcohol, chloroform, opium, &c., need not detain us ; nor the well-known complication of coma with eruptive and other fevers, with anæmia, and with loss of blood. The conclusions at which Dr. Todd finally arrives, are those which may be fully understood from our analysis.

Having settled the nosology and etiology, Dr. Todd proceeds to inquire into the pathology of delirium and coma ; that is to say, what part or organ is affected, and what is the nature of the disorder. In reply to these questions, Dr. Todd adopts the now almost universal opinion, that the organ affected is the organ of intellect, namely, the gray matter in the convolutions of the brain—"the centres of intellectual action." It is first of all requisite, Dr. Todd thinks, to understand the physiological conditions necessary for the maintenance of consciousness, before we can determine

the nature of the affection of that organ which constitutes delirium. He therefore analyses a simple sensation, which is one of the primary forms of consciousness, and takes a sensation excited through the olfactory nerves as his subject:

"When I smell a rose, what are the physiological phenomena? First, there is an impression made upon the sentient nerves; secondly, the change wrought in these nerves is propagated to the centre of sensation; and thirdly, this change produced in the centre of sensation *must be perceived* by the mind in order that a true act of sensation may be accomplished. Thus in the act of sensation we have two classes of phenomena—one physical, proceeding from periphery to centre; the other mental or intellectual, by which all physical change is recognised. The impression of the odoriferous particles on the olfactory nerves, and the subsequent change in the centre of sensation, is the physical part; the perception by the mind of that change is the mental or intellectual part. My mind may be occupied with some engrossing subject at the time the rose is presented to the organ of smell; the physical phenomena will nevertheless take place; but the mind being occupied with some other object, will not perceive the change in that centre, and therefore there will be no sensation; I shall not be conscious that such an object was presented to the organ of sense." (Lecture iii.)

Thus, then, Dr. Todd argues, the co-operation of two parts of the brain is necessary for this "simplest act of consciousness;" namely, the centre of sensation, or that part which receives sensorial impressions, and the centre of intellectual actions, or that part which perceives them. Dr. Todd then adds:

"Now the centre of intellectual actions is the hemispheric lobes of the brain or the convolutions; the centre of sensation is, as I have shown in my Lumleian Lectures of last year, and also elsewhere, the optic thalami and their downward continuations, the olivary columns of the medulla oblongata, and the posterior horns of the gray matter of the spinal cord." (Lecture iii.)

These are the parts involved, then, according to Dr. Todd's views, in delirium and coma. The seat of the cause of delirium is in the convolutions of the brain; the seat of the diseased action which may cause coma is the same centre, with or without the centre of sensation. Or the morbid process may begin in the centre of sensation, destroying certain kinds of consciousness, and may extend to the intellectual centres, making the coma complete. As an illustration of his views, Dr. Todd mentions the forms of rheumatic and traumatic delirium, in which the sufferer becomes insensible to the irritation which must be created, when he moves the injured or broken leg, or the painful joints. In each case the man is insensible to pain, "because the centre of intellectual action is entirely engrossed with the rapid train of ideas and fancies which occupy his mind; and *therefore* he does not perceive the irritant change which must be produced in the nerves of the limb by the movement and displacement of the injured part."

Now an analysis of these statements will show, that there is an imperfection both in the theory and in the illustration. On these points Dr. Todd seems to remain in the condition in which we left him in our review of his former course (vol. V, p. 26). The *perception* of an odour is hardly to be termed "an act of the *simplest* consciousness." There is a special organ of sense required, a special nerve adapted to that organ, and a special ganglionic centre in which that nerve terminates. Further, there is a commissural connection between that sensory ganglion and the cerebrum,

from which it is structurally, and (as we cannot but believe) functionally distinct. So that anatomically Dr. Todd has omitted the very portion of "the centre of sensation" upon which he grounds his illustration. Metaphysically Dr. Todd follows metaphysical writers; but his illustration is applicable rather to the perception of the primary and secondary qualities of bodies, than to "an act of simplest consciousness." According to the nomenclature now generally received among the most distinguished metaphysicians, (Sir W. Hamilton, for example,) the simple consciousness of the state of the nervous system excited by the odour is that which constitutes *sensation*, whilst the recognition of the cause of that sensation as something external to self, is that which constitutes *perception*. Sensation is the simple *subjective* condition; whilst perception may be said to be the consciousness of the *object* which induced that condition. The distinction is not one of words alone, but has its foundation in reality, as any one may prove by the analysis of his own states of consciousness; and whether or not we use the terms "sensation" and "perception" as respectively characterising these, we hold that a clear recognition of it, of which Dr. Todd has given no indication, must lie at the bottom of all our application of psychology to physiology. The seat of the consciousness, whether of external impressions or of mental changes, we believe to be the same; namely, that series of sensory ganglia, which is not merely the centre of the nerves proceeding towards it from the periphery of the body, but also of those which come to it (the "nerves of the internal senses," as Reil termed them,) from the periphery of the brain itself. Without these ganglia, which (as we pointed out on a former occasion) constitute the fundamental portion of the brain, there can be no sensation; but there may be sensation, we feel quite satisfied, without a cerebrum, although no perception can be formed save through its instrumentality. It appears to us that the consciousness of physical pain or of relief therefrom, or of ease and suffering, belongs to the simple state of *sensation*; indeed, we incline to think, that in many lower animals—in whole classes, indeed—as in the earliest period of infantile existence, there is no other kind of consciousness, no true perception.

We are quite at issue with Dr. Todd, therefore, in believing, that in "the simplest act of consciousness," the co-operation of two parts of the brain is necessary; and limit this requirement to that higher class of changes, in which the simple consciousness arouses ideas. Now the sensorial ganglia may be active *quoad* the ordinary nerves of sensation, whilst inactive in regard to the cerebral fibres; and we then have that obliteration of all *intellectual* power, with which consciousness to external impressions has often been found persistent. But the more common case is for the sensorial ganglia to be inactive *quoad* the sensory nerves, and active only on that side (to speak figuratively) which looks towards the cerebrum; and we then have that *unconsciousness* of ordinary sensational impressions, with persistent intellectual activity, which is the typical characteristic of dreaming. We no more think, therefore, that the delirious patient feels no pain *because* his mind is occupied by his delirious ideas, than we believe the dreamer to be unconscious of external objects *because* his mind is filled with his dreams. In the one case, as in the other, we apprehend that there is a state of actual insensibility to external impressions; and that the delirium, like the dreaming, is but the residue of the cerebral



activity, now no longer under control. Hence it appears to us that it is more consonant with a sound physiology and pathology to consider this anæsthetic condition of delirious patients as the first stage of the comatose state, and as dependent upon a physical change in the sensorium itself, rather than upon a preoccupation of the mind by delirious ideas.

It is not difficult to trace the gradual extension of this state, in a patient in whom delirium is about to terminate in coma, from excessive sensibility and receptivity, through all the subsequent stages of disregard of sounds, visual objects, touches, feelings of hunger, thirst, &c., until deafness, blindness, and total insensibility end the struggle.

The whole question must, we think, be considered more comprehensively than Dr. Todd considers it, to arrive at anything like a sound theory and a rational treatment of delirium and coma, and, indeed, of all diseases of the encephalon. We have a condition analogous to delirium and coma occurring daily as the ordinary and beneficial state of the individual;—sound sleep is the type of coma; the dreaming state, and the interval between sleeping and waking, correspond to the stage of delirium. Again, in hybernating animals, and in those which go into a *summer* sleep, we have a condition of the nervous system analogous to coma; and before we can say that we have arrived at a sound pathology of the *abnormal* conditions, we must have attained to a sound physiology of the *normal*. Have we done this? Is there anything in Dr. Todd's views that will illustrate the phenomena of sleep, of dreams, of hybernation? What are these states in the lower animals? and what their relations to *brute* consciousness? These are questions of deep moment; and their solution involves certain considerations, which have scarcely, we think, received from Dr. Todd the attention they merit. We cannot fairly attribute the recurrence of such states to conditions of the blood; they must be rather looked upon as results of the vital properties of the nervous tissue itself, which are so far natural to it, as to constitute, in fact, part of its normal scheme of functional activity. When we extend our survey to the instinctive operations of insects and others among the lower tribes of animals, and witness the wonderful series of reactions that are there affected by the cephalic ganglia, in accordance with "laws written on the nervous pulp," as Prochaska phrased it, we perceive that this reactionary power is exercised in classes of actions of a much higher character than those which are commonly denominated reflex. And further, when we take a comprehensive view of the *cerebral* actions in man, we can scarcely fail to perceive, that many of these, also, must be explained upon the same principle, and that there is a "reflex" action (using this term in its wide sense) in the brain as well as in the spinal cord. This principle was explained and illustrated some years since by Dr. Laycock;\* and although we consider that his doctrines need some amendment, in so far as they attribute everything to the periphery of the hemispheres, and do not recognise the independent operation of the sensory ganglia as the centres of consciousness and the immediate sources of movement, yet we believe them to be essentially correct in the position laid down,—that the peripheral gray matter reacts according to determinate laws of its own; its mode of response, however, being not the same in different individuals (like the purely instinctive actions of the lower animals),

\* See 'Edinb. Med. and Surg. Journal.' No. cxi, and the 'British and Foreign Medical Review,' vol. xix, p. 298 et seq.

but being dependent upon the material state of the nervous system, as influenced by its habitual modes of activity, emotional or intellectual, and by its formative condition,—and this last again being dependent upon the constitutional predisposition, the general state of the nutritive processes, and a variety of other remote influences, all of which tend to modify the reactions of this part of the nervous centres. Of all these influences Dr. Laycock considers, that the immediate operation is to develop certain *substrata* in the gray matter, which become the centres of reaction when their appropriate stimuli are applied; and he adduces in support of his hypothesis so much ingenious argument, founded upon a comprehensive survey of well-observed facts, that we cannot help regarding it—whatever we may think of the terms in which it is stated—as, to say the least, a very convenient one for grouping together a number of phenomena that would otherwise seem unrelated to each other.

The following extracts will show the bearing of his views on our present subject :

“Coma consists in an abolition of consciousness. There is no volition, nor any perception of external or internal stimuli; and life consists in a series of automatic movements. It is scarcely necessary to particularise its numerous proximate causes. It is frequently a very alarming symptom of hysteria, and, as might be expected, from my preceding remarks, is consequent upon every exhausting agency of a certain degree of intensity, as profuse bloodlettings, or other evacuations; debilitating diseases of mucous surfaces, mental or bodily fatigue, and violent passions. It is very frequently induced by poisons, whether they enter the system by absorption, or consist of the excrementitious matter accumulating in the blood from deficient elimination; as when the excretion of bile, urea, or carbonic acid is prevented by diseases of the kidneys, liver, or lungs. Profound sleep is the type of coma; in both, there is complete paralysis of the brain, and the usual changes which constitute ideas and excite volition, are not originated by any stimulus whatever.

“When the causes of coma act with less intensity, changes are excited which we perceive; but there is still paralysis of the *primary* organs of perception and volition. We can both perceive and will, but the changes in the motor nerves necessary to motion cannot be excited; nor can we perceive the primary changes (or sensations) in the sensitive fibrils resulting from external stimuli. These, nevertheless, originate other changes, which become objects of perception, and which are analogous to reflex movements.” (Edinb. Med. and Surg. Journ., No. cxi, p. 20.)

It is thus Dr. Laycock explains the various sensorial changes which occur when stimuli act upon the hemispheres through the circulation, and excite the sensori-motor actions which accompany the semi-comatose condition. With regard to the delirium, he observes :

“Delirium originates from all those causes which will excite coma or paralysis, only they act with less intensity. We have a less amount of paralysis of the cerebral organs; the *ideagenous* changes, or changes producing ideas, are more complicated, because the primary sensations derived from without are more or less perceived; the motor nerves may, in an equal degree, be influenced by the will, and the sensory apparatus is also in part under the same control, for the individual is capable of an act of attention, or, in common phrase, he will listen or speak when roused—the act of will placing the sensory apparatus in a state such that external stimuli produce a more vivid impression, removing, in fact, the paralysis during the effort, just as motor paralysis may be removed by the same means.” (Ibid., p. 21.)

Dr. Laycock refers the phenomena of dreaming, spectral and other sensorial illusions, the deliria of mania and insanity, and the peculiar con-

dition termed somnambulism, to the same head ; but we must for the present content ourselves with this mention of his views.

From the considerations to which we have adverted, and from various others, it is obvious, we think, that since in natural sleep, and in the paroxysms and evanescent forms of delirium, somnambulism, coma, and the like, there is so entire and speedy a return to normal function, the changes on which the morbid state depends are but temporary. And this brings us to the inquiry into the nature of the change which the cerebral system undergoes in delirium and coma, which Dr. Todd attributes, as we have seen, *exclusively* to *toxic* causes that impair the nutrition of the gray matter, or render it morbid ; and to ask how will Dr. Todd's hypothesis account for their typical states—dreaming and profound sleep ? But with regard to these we find that it is the consciousness which is modified, and the power of feeling pleasure or pain which is first suspended. The change, then, which the system undergoes, must be in that tissue or series of vital actions, in virtue of which the property of feeling and consciousness is superadded to the automatic apparatus. Of these we really know nothing. What is certain, however, is, that repose necessarily follows activity ; and that without daily repose, and the consequent periodic abolition of central action of this kind, the exquisite machinery upon and through which the mind acts, becomes deranged and disorganised. Hence we may conclude that delirium and coma depend upon the same class of functional changes in the gray matter, as dreaming and sleep ; and may also thence deduce the principles of treatment to which experience has led Dr. Todd, and not Dr. Todd only, but all other physicians who have had frequent opportunities of witnessing *certain* prevalent forms of delirium and coma ; *when the patient is delirious, induce sleep ; when coma has followed on delirium, leave the patient for a time, at least, in undisturbed repose.*

But there are several forms of delirium and coma,—forms differing widely in their etiology, and requiring important modifications of treatment ; and this leads us to the principles of both taxonomy and treatment which Dr. Todd has adopted. It will be observed that they are founded entirely upon his views of etiology, which we regard as of too exclusive a character. No one will deny that poisons received into the circulation will induce delirium—the doctrine is as old as the hills ; but it is equally well known, that these poisons may be in operation in the system of numerous individuals, and may never induce delirium ; or, affecting the nervous system, will cause some other forms of neurosis. It is in this point that Dr. Todd seems to us defective in his etiology ; his arrangement is therefore defective ; and consequently, as we have seen, his diagnosis imperfect ; for it is obvious, that a too exclusive consideration of a humoral pathology has led to this defect ; and he has hardly comprehended the full meaning of the significant term *neuræmic*, as first applied by Dr. Laycock to designate this whole class of affections.

Now the predisposing and exciting causes of delirium and coma, as well as of the greater number of encephalic diseases, are within our reach, however much the *proximate* cause may elude our grasp. But if we consider the normal condition of the brain in dreaming and sleep, as typical to a *certain extent* of the morbid condition in delirium and coma, and examine the predisposing and exciting causes of the latter in reference to this principle, we may make even our imperfect knowledge of the *causa proxima* available in a determination of the hypnotic means appropriate to each form

of delirium, and of the circumstances under which they should be used or administered.

*Diagnosis and Treatment of Delirium and Coma.* Before referring to the humoral forms of these diseases, we will notice those dependent upon *structural* change, of such a character, that the return to a healthy condition is not immediate, or never occurs at all. Dr. Todd rides his hobby a little hard, when he gives such paramount frequency of occurrence to the functional forms; and we are especially far from agreeing with him as to the infrequency of the inflammatory diseases of the encephalon. We think that *cerebral meningitis*, or rather, inflammation of the hemispheric periphery, is quite a distinct disease from *cephalitis*; a term which (we are surprised to observe) Dr. Todd thinks "has at least the recommendation of being comprehensive, for it is applicable to inflammation of any or all the textures that form the head." Then, with a view to establish his opinion as to their infrequency by the statistics of inflammatory encephalic diseases, Dr. Todd turns to the weekly reports of the Registrar-general, and concludes, from the comprehensiveness of the term, that "we may expect that all cases having any pretension to be called cases of inflammation of the brain, or of its membranes, are included in the report" under that designation. We need hardly inform Dr. Todd, that no practical writers of any note have ever confounded things so essentially distinct as inflammation of the *membranes* and surface, and of the *substance* of the brain, in children at least. All agree, however, in acknowledging the difficulty of diagnosis; and having recognised and felt this difficulty himself, we think that it is not quite justifiable in Dr. Todd to conclude that it is not experienced by the majority of practitioners; and we suspect that the term *cephalitis* is not so all-comprehensive as he asserts. Dr. Todd finds that the average weekly number of deaths from inflammatory diseases of the brain or its membranes ("cephalitis"), is only 9 for the ten years from 1840 to 1849. In the weekly Report before us, the average of deaths from cephalitis, for the week ending Nov. 3, is 10·3; from "convulsions" (almost all in children), 44·9,—many of which would probably be only cases of acute meningitis ending in effusion; of "hydrocephalus" (or meningitis?), 30·1; and of "diseases of brain, &c.," 10·2, *some* of which would surely be inflammatory. Thus, in addition to the average of ten weekly deaths from "cephalitis," there are no fewer than eighty-five from registered diseases of the brain under names which in many instances undoubtedly indicate them to have been inflammatory. Indeed, we should think tubercular arachnitis almost as common to infancy as any severe disease whatever.

In laying down the principles of diagnosis between inflammatory and non-inflammatory delirium, Dr. Todd enumerates certain symptoms in the order of frequency as indicative of cerebral inflammation, and the first he mentions are *nausea* and *vomiting*. These are undoubtedly observed to usher in inflammation; but, taken *per se*, nothing could be more fallacious, for faintness, nausea, and vomiting, are not of unfrequent occurrence in cases of nervous apoplexy, or in the slight paralytic stroke induced by renal disease or over-work of the brain. We apprehend that vomiting from cerebral inflammation will occur for the most part in inflammations at the base of the brain, or in those forms, in which compression or concussion involve the basilar portions, for we look upon true cerebral vomiting as dependent upon *centric* irritation. *Pain* is a very common symptom of inflammation, whether acute or chronic, especially in the early stages, but

its diagnostic value *per se* is as little as the preceding, and can only be estimated with the *anamnesis* and inter-connection of symptoms; and we must remember, that the *brain itself* is insensible. The *condition* of the pulse is very variable. Andral is of opinion, that it is in general accelerated, although it may be slow. Dr. Todd mentions "sluggishness" as a characteristic—"the artery strikes in a heavy sluggish manner, not rapidly or sharply, but conveying the sensation as if impressed by a larger surface of the finger than usual." Dr. Todd nevertheless adds, that "generally, however, in cases of inflammation of the brain, the pulse is both slow and sluggish, and the heart's action heavy." We believe that much will depend upon the nature and seat of the inflammation, much upon its stage, something upon the condition of the heart itself, something upon the state of the blood at the time. If the serous membranes, rather than the parenchymatous structure, be involved, the pulse will be quick; if the blood be poisoned with urea (as is not unfrequently the case in encephalitis), from paralysis of the bladder or kidneys, the pulse will be less affected; if the inflammation is in an advanced stage, and the basilar portions are involved, the serous or sero-purulent effusion is ushered in with increased rapidity of the pulse, so that it can hardly be counted, and true coma ends it; if structural or functional disease of the heart pre-exist (which is frequently the case), the pulse will be correspondingly modified in each stage.

The diagnosis between inflammatory and humoral delirium is undoubtedly difficult at that stage, when it is all important, namely, the earliest. We can only, in fact, determine it, from a careful inquiry into the history of the case, and by the grouping of the symptoms; and we should particularly seek to ascertain whether the delirium can be traced to sources of *peripheral* irritation. But when the symptoms indicate certain lesions of the motor apparatus, as convulsive movements of single muscles or groups of muscles—strabismus, contracted iris, twitchings of the facial muscles, or spasm of the fingers—and there is increased irritability of temper, with flushing of the face, heat of the head, and throbbing of the temporal arteries, while yet delirium is not present; if to these be added partial retention or suppression of urine, vomiting without any assignable cause, and constipation, together with all the usual phenomena of pyrexia, the pulse being little if at all accelerated; it is probable that the case is inflammatory, and if delirium comes on, that the stage of effusion or supuration has already begun. If to these be added general convulsions or paralysis, strabismus, incontinence of urine or fæces, pulse 130, or more, and true coma, then that stage is completed, and death is about to supervene.

*Treatment of delirium tremens.* We shall now consider the *treatment* of the humoral forms of delirium, especially by sedatives; and firstly, delirium tremens or alcoholic delirium, may be either acute—when it is the *delirium a potù*,—or chronic, when it is the true form. The latter is divided by Dr. Todd into the mild and the intense; the mild constituting the state popularly known as "the horrors," in which there are spectral illusions, tremors, and wakefulness; and if sleep comes on it is disturbed by horrid dreams. This is best treated by good diet, porter and wine in moderation, and a full opiate at night. In the more intense form, the treatment is the same, only the food must be highly nutritious, administered in small quantities, and frequently repeated. The stomach



is usually highly irritable, and its mucous membrane is, we believe, often inflamed. In these cases we have found it advisable to combine morphia with nitrate of silver in the form of pills; to administer nothing by the mouth, except cool unirritating fluids; and to give enemata of beef tea or broth, with laudanum. When the stomach is less irritable, moderate purgation is useful. The treatment of the paroxysms, as recommended by Dr. Todd, is, we think, eminently judicious; it is founded on the important principle of sedulously abstracting the patient from all impressions; to this end the room may be darkened, or the bed surrounded by a screen, and the gentlest restraint used if that be necessary. To procure sleep, endermic means may be used over the scalp. Warm sponging, with a solution of opium or poppy-head decoction, will be beneficial, and internally, opium administered in large doses. If, however, the pupil becomes contracted under its influence, and no sleep has been induced, the remedy should be discontinued. Under these circumstances, *chloroform* has been administered by Dr. Todd with success, in three severe cases. It seems to us probable, that even in *ordinary* cases, a prolonged state of chloroform sleep would be highly beneficial; and that it might often be safely substituted for opium, especially if combined with those other means which an enlightened etiology indicates. Bleeding and mercury, we need hardly say, are objected to, and very justly, by Dr. Todd.

The treatment of traumatic and rheumatic delirium is by opium, in preference to any other sedative, unless we except chloroform. The great afferent stimulus of the sensorial system in this form, is physical pain; and although, after the delirium has commenced, the patient may become partially anæsthetic, yet it may still happen, that the physical impression has its effect on the sensory or hemispheric ganglia, although not felt. Indeed, in all those forms of delirium occurring subsequently to, and perhaps consequently on, *mental* or physical suffering, the same remedy is indicated—the induction of sleep by opium, alcohol, or chloroform, and the withdrawal of all stimuli. Dr. Todd warns us against the use of opium in those cases in which there is a tendency to coma, as is sometimes observed in rheumatic or gouty delirium, and with justice; for as the sedative is only indicated when the delirium is active, opium would be of no service in impending coma, but be actually injurious.

In the delirium which accompanies pulmonary inflammation, Dr. Todd recommends alcohol as the sedative rather than opium, because opium has a tendency to increase congestion in the lungs. Perhaps its most direct influence is on the bronchial fibres, which it tends to paralyse. Quinine with camphor, hop, and henbane, are also useful.

*Febrile delirium*, induced by erysipelas or typhus, is also best treated by stimulants. Dr. Todd thinks there is nothing which seems to act as an antidote to the erysipelatous poison so well as alcohol, in the form of brandy or wine. The bowels are first evacuated, and then nutritious food with brandy given. Where there is a tendency to very active delirium, opium may be given; but chloric ether, henbane, camphor, and hop, are safer. Dr. Todd applies the same method to the delirium of typhus; gives alcohol, blisters the head, and avoids opium, unless when exhaustion is to be feared from the violence of the ravings.

*Puerperal delirium* appears under two forms: one with a rapid running pulse, varying from 120 to 140, in which the prognosis is unfavorable;

another in which the pulse never rises above 100, and ends usually in recovery. *Hysterical delirium* usually appears under the latter form; in all the forms, the soothing nutritious treatment is to be preferred, such as is adapted to delirium tremens. A great deal, Dr. Todd well observes, is to be done by moral means; stimuli should be shut out, and mechanical restraint avoided. In some cases of hysteria and hysterical excitement, bordering on delirium, the craving for stimulants is intense; so that wine or brandy is demanded with extraordinary urgency. In this class of cases we think the appetite is instinctive, and ought not to be hastily rebuked or neglected, as it is undoubtedly significant of functional cerebral disturbance. *Epileptic delirium* must be treated with reference to its supposed etiology, and "be mainly *supporting* and *expectant*;" as the paroxysm will exhaust itself in time, if we do not allow the patient's strength to be exhausted. If there be *vigilia*, avoid opium; and let the sedative be henbane or hop, with the cold douche. An urgent craving for stimuli is occasionally a premonitory symptom of impulsive delirium or mania; it is also not unfrequent in persons having an hereditary predisposition to insanity, and arises, we suspect, from the same cerebral condition which is the cause of hysterical delirium; it always merits careful consideration; for we think an absolute denial of the stimulus may not be without danger, if no steps be taken to correct the condition on which the craving depends.

*The prophylactic and curative treatment.* We have discussed little more in the preceding pages than the treatment of delirium in the paroxysm, with reference to the *proximate* cause; but something more than this is required, as there are numerous exciting and predisposing causes not yet adverted to. All the humoral causes of the cerebral disorder may be in operation in various individuals, and yet no delirium or coma may supervene. The reason of this evidently is, that in this, as in all other diseases, a *combination* of circumstances is required. These, upon a consideration of the whole question, may be resolved into circumstances involving the structure of the brain, the seat of the disorder; into those involving its vital action as dependent upon the blood; and into those involving its functional activity, either in reference to the exercise of its own powers, or to its excitation by the influence of excitor impressions derived from distant organs. All these circumstances involve important considerations, and are, indeed, of so weighty a character, that our exhausted space will not allow us to give more than an outline. Firstly, it is well-known that an acquired or hereditary predisposition to disease of the nervous system is an important circumstance. This being the case, then a morbid condition of the blood will, when the exciting cause or causes are developed, fill up the sequence of causation; and cold, hunger, great labour, want of sleep, malaria, miasmata, and communicable febrile poisons, alcohol, &c., *excreta* retained in the system (as carbonic acid in pulmonary disease, bile in jaundice, urea in renal disease), will induce the cerebral affection, by rendering the blood poisonous to the brain. When these two classes of causes, that is to say, a latent susceptibility of morbid action in the cerebral tissue itself, and a morbid condition of the blood, have a *predisposing* operation, then *exciting causes*, acting either through the external senses,—through the mind,—or, in virtue of that connection which the brain as the centre of consciousness and of unity, maintains and

holds with every part of the body generally, and, as regards the activity of certain instincts and passions, with certain organs specially, through afferent impressions received in infinite variety,—will suddenly put the brain, or parts of the brain, into morbid activity, and cause some form of cerebral disorder. It is in this way that disease of the heart operates so powerfully on the cerebrum, that irritation of the uterus or ovaria is so frequently an exciting cause of delirious and convulsive affections, that excess of sexual enjoyment and of sexual stimulation is so potent in the organism for evil. Every mucous surface, also, and the whole cutaneous surface, send their exciting impressions inward, if there be any irritant applied thereto; and thus it is that worms, accumulated fæces, ulceration, inflammation, &c., often cause an outbreak.

Now this, so large a portion of his subject, has not been touched upon by Dr. Todd; but we trust he will turn his attention to the state of the brain itself, and the predisposing causes inherent in its constitution; to the etiological relations which distant but closely-connected organs bear to it physiologically, and to the operation of the infinitely numerous incident-excitor impressions which reach it from every part of the organism, and contribute much to the development of its diseased states. If these points be considered in relation to the morbid state of the blood, we are satisfied that much now unintelligible in the pathology, and obscure in the nature and treatment, of cerebral disorders of every kind would be elucidated. But we cannot think that an exclusively humoral pathology will be so available, for it will only carry us round a circle already well trod.

### ART. XIII.

*A Theoretical and Practical Treatise on Human Parturition.* By H. MILLAR, M.D., Professor of Obstetrics and the Diseases of Women and Children in the Medical Department of the University of Louisville. —*Louisville*, 1850. 8vo, pp. 463.

WHILE we acknowledge a strong antipathy to the wholesale system of republication of English medical works which prevails in the United States, we gladly hold out the right hand of fellowship to an American author. In this light Dr. Millar presents himself to us; and we hail his work as a genuine specimen of American obstetric literature. Dr. Millar is, in the strictest sense of the word, an original:

“The author is a backwoodsman, having been brought to light in Kentucky by a process which it is his purpose to unfold in this treatise. His education was not acquired in Academic Hall, but in the primitive school-houses of his native state, and upon the ample sward, shaded by forest trees, appurtenant thereunto; so that you see he was reared after the fashion of Socrates, imbibing knowledge in the school-house under the shades of trees, and not unfrequently perched upon their boughs.” (Preface, p. vi.)

Our author fully anticipates all the difficulties of his struggle for success in authorship. No objection is more fatal in the mind of brother Jonathan than “we do not see the use of it.” The accumulated literature of other countries supplies all his necessities, and Dr. Millar is not wanted; but the success of Dewees and Meigs encourages him to think that he may have a chance. It is clear, however, that he can have none, unless

something new is advanced. Dr. Millar, therefore, boldly seizes upon two topics, in which he dissents from the majority of writers. One is the mechanism of the child's passage through the pelvis. The other is the classification and nomenclature of the positions and presentations, which, he affirms, "have been thrown into a hotch-potch," so that "obstetricians can no more communicate with each other concerning them, than could the Babel-builders after the confusion of tongues."

We shall now proceed to consider how far Dr. Millar has succeeded in the arduous task which he has undertaken.

The work consists of twenty-four chapters; the first six of which are devoted to so much of anatomy and physiology as are sufficient to explain Dr. Millar's views of "human parturition:"—"The Obstetric Properties of the Pelvis—The Obstetric Aptitudes of the Fœtus—The Appurtenances of the Fœtus—The Uterus considered as an Organ of Expulsion—The Efficient Cause of Labour—The Determinative Cause of Labour,"—are all considered in detail. The phenomena and management of the first and second stages of labour are then taken up, and are followed by two important chapters on "Presentations and Positions of the Fœtus," and "The Mechanism, Diagnosis, and Prognosis of Vertex Presentations." The remaining chapters are devoted to the Use of Instruments, the consideration of Nates and Shoulder Presentations: concluding with the Phenomena, Management, and Treatment of the Third Stage of Labour, in which retentions of the placenta and hemorrhages are briefly discussed.

The first chapter, *On the Obstetric Properties of the Pelvis*, reveals the elements of Dr. Millar's original views respecting the passage of the head of the child through the pelvis. On this point he dissents from Naegele and Rigby; and denies their explanation of the rotation of the child's head. "No such rotation takes place, in fact, *during the descent of the head*, as I shall have occasion to show, when the mechanism of labour is treated of." (p. 12.) Dr. Millar, therefore, combats Dr. Rigby's views of the anatomy of the pelvis, and, of course, discards his nomenclature. "With regard to the designation of the oblique diameters, it is necessary to observe, that I adopt M. Cazeaux's rather than Dr. Rigby's nomenclature. Dr. Rigby, tracing these diameters from the sacro-iliac symphysis to the acetabula, distinguishes our (Cazeaux's) left as right, and vice versa." That is, an imaginary right line, lying obliquely between the sacro-iliac symphysis and the acetabulum, is supposed by Dr. Rigby to be drawn from behind forwards, by M. Cazeaux from before backwards. The former calls the right oblique diameter that which is bounded by the right sacro-iliac synchondrosis; the latter, that which terminates in the right acetabulum. It seems very unimportant which of these are adopted, provided they are generally understood. We cannot, therefore, perceive how Dr. Millar educes order out of this confusion, by adopting the nomenclature of the French obstetrician and rejecting that of the English author, whose work we would presume to be more generally read in a country where English is universally spoken. If Dr. Millar had adopted a new term, discarding the words "right" and "left," which are certainly calculated to confuse, when used by different authors in opposite senses, his object would have been better served. The description and measurements of the brim of the pelvis present no other difficulty; it is not so, however, with those of the outlet, which Cazeaux, Velpeau, Dugès, have

each exerted their ingenuity to bring within the limits of a mathematical demonstration. Our author selects that of M. Dugès (and we think with some justice), as being the clearest. Dugès proposes to divide the inferior strait into two nearly equal parts, one anterior, and the other posterior, which meet upon the tuberosities of the ischia :

“The anterior division he calls the vulvar, the posterior the coccy-perineal space, and these may be represented by two pieces of paste-board,—one of which fills up the pubic arch, and the other the posterior vacuities of the pelvic outlet,—which meet, and are united opposite the ischiatic tuberosities. These pieces of paste-board, it is evident, will form two planes with opposite inclination. Tracing them from their junction, the posterior looks *upward* and *backward*, and the anterior *upward* and *forward*.” (pp. 9-10.)

If the irregular outlet must be brought within the limits of plane surfaces, it certainly cannot be reduced reasonably to a single plane.

The pelvic excavation, however, is that part of the pelvis with which Dr. Millar is chiefly concerned, because it involves the movements of the head of the child. According to many authors, the sides of this excavation, the ischial surfaces, are considered as inclined planes upon which the head is rotated forwards. Dr. Millar, however, denies that any such rotation takes place, and therefore objects to this view, as well as to the habit of describing this portion of the pelvis as “nothing but a conjunction of inclined planes.” Nevertheless, as Dr. Millar proceeds, we find him dividing the ischial surfaces into two planes in place of one, making a double inclined plane of each, as had been already described by Murphy and by Dugès ; and acknowledging that these favour the rotatory movement of the head, “which does really take place when it reaches the inferior strait, the occiput gliding upon one of the *anterior inferior* planes, while the forehead moves over upon the surface of the diagonally opposite *posterior superior* plane.” (p. 13.) A rotation, then, does take place ; and the difference between Millar and Rigby is only as to the precise part of the excavation where it begins. Dr. Rigby says, *above* : Dr. Millar, *below*, at the inferior strait. Now, if the double inclined planes of the ischia have any effect in producing this rotation, the head must begin to alter its direction when it arrives at these planes, that is, when it enters the excavation. The anterior plane must incline the occiput forward towards the obturator spaces in the first instance, and to the pillar of the pubic arch as it descends further ; while in a similar manner the forehead passes backward, towards the ischiatic spaces, and thence along the short sacro-ischiatic ligament to the sacrum. The rotatory movement, therefore, commences above, although it is not completed until it arrives at the outlet. In the explanation of this rotation, Dr. Rigby, like other authors, has laid some stress upon the measurements of the excavation ; pointing out the increased length of the oblique measurement of the cavity as compared with that of the brim, the oblique measurements of the cavity terminating in “free spaces,” the obturator and sacro-ischiatic, and, consequently, seeming to afford more room for the head in this direction as it descends. Dr. Millar, however, objects to all this ; and considers that, as these free spaces are filled with muscles, their contractions diminish the oblique measurement of the excavation, just as much as the conjoined psoas and iliacus do the transverse diameter of the brim :

“But the ‘free spaces’ of the sacro-ischiatic notches and foramina ovalia, are also occupied by muscles which have bellics to swell, as well, though not so considerably,



as the muscles of the brim. What spell less potent than the oblique theory of parturition could transpose these into 'soft yielding textures,' receding before pressure, as they are ascribed to be by Dr. Rigby? (p. 15.)

In this objection, given in this racy style, Dr. Millar has forgotten the different relations that the head bears to those parts with which it is in contact, when above the excavation, as compared with those within it. When the head is above the brim of the pelvis, a feather might turn it, so that the action of the muscles that cover the transverse axis of the brim readily changes its position; but when the head is fixed in the excavation, pressing with great force against every part to which it is applied, any muscles it may there come in contact with can have no such effect; in none of the movements of the patient are these muscles called into action; and we are satisfied that if by any accident their contractions were excited, she would soon change her position to avoid the increased pains thus caused. We do not think, therefore, that the bellies of these muscles do swell; but believe, on the contrary, that every effort of the patient is directed to relax them, and to afford room for the advancing child; hence, when the head is in the excavation, more space is given to it when pressed against those "yielding textures," than if it were opposed by the unyielding bone.

Dr. Millar objects to the axis of the excavation being considered as the arc of a circle.

"In considering this axis, it is easily discovered that it is not the *arc of a circle*, as Professor Carus describes it." In this we quite agree with him; and could wish that obstetric works were freer from these mathematical heresies than they are. An axis and an arc are two different things, and never can be compared together; to speak, therefore, of an axis, as if it were a curve, is an absurdity. We also agree with Dr. Millar in his objection to Carus's view, that the head descends in the arc of a circle. It appears to us, on the contrary, that it first moves in the axis of the brim, a straight line, and does not begin to describe a curve until the occiput arrives at the ischio-pubic ramus, against which it rests, while the forehead and anterior portions are passing into the axis of the vagina and outlet, also a straight line. In this movement a curve is described from one axis into the other; but this curve is not the axis of the excavation, although it may pass through it. On this point Dr. Millar seems to be fascinated by the ingenuity of the French authors, who always have triangles, squares, cones, cylinders, and rhomboids dancing down their pens whenever they write descriptive anatomy. He is, therefore, led to adopt Cazeaux's view of the axis of the excavation, which is very ingenious, but unfortunately is not true.

We shall for the present pass over the Chapters on the "Physiology of the Fœtus;" "the Uterus as an Organ of Expulsion;" "the Causes, Phenomena, and Treatment of the First Stage of Labour;" in order to consider the mechanism of the second stage, as having a more direct relation to the obstetric properties of the pelvis which we have just discussed. We turn, therefore, to Chapters xiii and xiv, "the Presentations and Positions of the Fœtus," and "the Mechanism, Diagnosis, and Prognosis of Vertex Presentations." Dr. Millar very properly commences with a definition of the terms "presentation" and "position," "meaning by the former the part which offers at the superior strait,—by the latter, the relation of this part to the different points of the strait." He further considers pre-

sentations as genera, and positions as species, in any systematic classification of them. The principle upon which these definitions are founded is this: "Whenever the presence of any part of the foetus at the superior strait of the pelvis requires a mechanism of its own, it is entitled to rank as a presentation; and if different relations of this part to the superior strait do or even may affect its mechanism, these differences ought to constitute so many distinct positions of the presenting part." (p. 223.) Dr. Millar then reviews the classifications proposed by different writers, commencing with Baudelocque, and ending with Dugès, whose classification he adopts; the twenty-three genera of the former writer being reduced to five by the latter. Baudelocque, in attempting to classify what he observed fully, determined to avoid the possibility of omitting anything that might have escaped his notice; he therefore divided the foetal body into regions, making a presentation for each: "many of these presentations he does not pretend to have met with in practice, but admits on the authority of a single observer, whose accuracy may be questioned, and whose love of the marvellous may have misled him." MM. Gardien, Capuron, Maygrier, and Dugès, and Madame La Chapelle, have each lopped off a presentation, and assisted in reducing the exuberance of Baudelocque, until we arrive at the five presentations of Dugès, viz. 1, vertex; 2, pelvis; 3, face; 4, right shoulder; 5, left shoulder. This classification is adopted by Dr. Millar in preference to that of Professor Naegele:

"In saying that Madame La Chapelle and Dugès have completed this desirable reformation, I must be understood to express only my own opinion. I am not unaware that still greater simplification has been attempted by others, chiefly by Professor Naegele in Germany, seconded by Dr. Rigby in England, and Professor Dubois in France. Of their attempt I shall presently express my judgment." (p. 217.)

Naegele divides positions into two classes, right and left. If it be the head that presents, it is either in the left occipito-iliac or the right occipito-iliac position, having varieties according as the occiput is anterior, transverse, or posterior; so with the nates and face. To this classification Dr. Millar objects, that it is contrary to the principle already laid down, viz. that each of these varieties should constitute a distinct position, because the different relation of each to the superior strait affects its mechanism. Naegele, however, says that it does not, and in this consists the essential difference between him and Dr. Millar: another of whose objections to Naegele's classification is, that it "allows no place for those positions of the nates in which the back of the foetus looks directly forward or backward (third and fourth of Dugès), which ought not to be excluded, because they are undoubtedly of sufficiently frequent occurrence to be entitled to a place, and their mechanism is not always the same as that of lateral positions." (p. 223.) We rather imagine that these direct antero-posterior positions are of very rare recurrence, and by no means of sufficient frequency to disturb Naegele's principle of classification. It is true that the back of the child, like the occiput, may be expelled in the antero-posterior direction; but this is not so because of this being its position, but because of the rotation of the body from the lateral position, during its descent. The simplicity of Naegele's classification Dr. Millar does not allow, because, "admitting that shades of difference only exist among its varieties, in regard to their mechanism, still we are compelled, for precision's sake, to refer to them, and this makes the nomenclature of positions cumbersome." (p. 223.) Not more so, certainly, than it was

previously; for we do not perceive that the prefixes "anterior," "posterior," "transverse," to the left and right positions, make a more inconvenient compound than those to which we have been accustomed; "left occipito-(iliac) anterior," or "left occipito-anterior," not being more cumbersome than "left occipito-cotyloid." The former term, one sufficiently perspicuous, is adopted by Dugès, and might readily be applied to Naegele's classification, thus making an approach to an uniform nomenclature; but Dr. Millar seems to think otherwise; for with a strange inconsistency, the nomenclature is the only part of Dugès's classification that is rejected, whilst, as if terms were not sufficiently numerous, he introduces new ones, that differ in nothing except their novelty, from those previously in use. We cannot find, for instance, any real difference between the terms "left occipito-acetabular," and "left occipito-cotyloid;" the former is proposed by Dr. Millar, the latter is used by authors generally. It appears to us, therefore, that while Dr. Millar has failed in his objections to Naegele's classification, which, at least, has the merit of simplicity, he has only succeeded in adding to the confusion of tongues of the obstetric Babel builders. In speaking of face presentations, Dr. Millar rejects the term "*mento-iliac*," used by Cazeaux, Naegele, Dubois, and Moreau; and adopts "*fronto-iliac*," the term used by Dugès. This again appears to us unfortunate, because the term "*mento-iliac*" can never be used for any other than a face presentation, while "*fronto-iliac*," like "*fronto-cotyloid*," may be applied to those of the vertex. Had Dr. Millar adhered to Dugès throughout, we should not have thought it necessary to allude to this; but, as on this point he does not follow his authority, we do not think that he has been happy either in the terms that he rejects, or in those he selects from him. Consequently, we greatly doubt his success in setting up "a national standard, under which all our practitioners may arrange themselves, and be cemented by a common bond of union." (Preface, p. vii.)

We shall now examine the most important topic connected with this subject, the mechanism of vertex presentations. Dr. Millar very correctly lays down the principle that must govern all the movements of the head during its transition, viz.: "The axis of the head must be brought parallel successively with the axis of the superior and inferior strait of the pelvis, that is, its poles; the occiput and chin must be placed, as nearly as may be, in these imaginary lines." (p. 237.) He comments with much justice on Dewees's mistaken assertion, that the great diameter (occipito-frontal) of the child's head must correspond with the great diameter of the pelvis; but we were rather surprised to read that this error is "an idea, which, as I shall hereafter show, pervades and vitiates the account which nearly all British writers give of the mechanism of labour." (p. 238.) In support of his assertion, Dr. Millar quotes Smellie's description at length, who says, "When the head first presents itself at the brim of the pelvis, the forehead is to one side and the hind-head to the other, and sometimes it is placed diagonal in the cavity." This account of the head's passage through the pelvis, as Dr. Millar justly remarks, certainly "turns upon the supposition that the head is perfectly inflexible, and is *squeezed along* until its longitudinal (occipito-frontal) diameter takes up its position alongside, with the transverse and oblique diameter of the inferior strait;" and we regret to perceive, that it is too closely followed by Ramsbotham, whose work has such general circulation. But an equally popular author, a more immediate successor of Smellie, and one whose work has been for years a text-

book in midwifery, gives a different account of the mechanism of the head's progress. Burns not only describes the oblique position of the head, the vertex being lower than the forehead, but also alludes to another obliquity of the head, the lateral, which is altogether unnoticed by Dr. Millar. Burns says :

"The position is oblique, in a two-fold way, *the vertex is lower than the forehead*, and the head also enters *somewhat sideways* into the brim of the pelvis, the one parietal, that which is toward the pubis, being the lowest, so that the diameter of the entering part is not so great as the space between one protuberance and the other by about a quarter of an inch. The head continues to descend obliquely, with the vertex lower than the forehead, and the chin directed toward or pressed on the breast or throat of the child, and it is not until the act of expulsion, that it rises or departs from that position." (Burns's 'Midwifery,' p. 357.)

This description is sufficiently precise, showing that the head, when entering the pelvic cavity, presents, not only its shortest longitudinal measurement (the occipito-bregmatic), but its shortest transverse axis, to the brim ; the bi-parietal measurement not being in the plane of the brim, but intersecting it at an acute angle, by which about a quarter of an inch of space is gained. Another, and very recent writer, gives a nearly similar account of the motions of the head, but rather more in detail. Dr. Murphy thus describes its passage through the pelvis :

"1. When the head is above the brim of the pelvis, the forehead and occiput are nearly on the same level ; but when *the head enters the brim, the occiput descends lower than the sinciput*, and glides a certain distance along the plane of the ischium, against which it rests. The forehead then advances more rapidly at the opposite side of the pelvis, until it is arrested by the convergence of the ischium, and shorter sacro-ischiatic ligament. The occiput again descends obliquely along the ischio-pubic ramus, and emerges with part of the parietal bone beneath the pubic arch. . . . 2. Simultaneous with this motion, there is a very slight rotation on the longitudinal axis of the head, by which that side of the head which is next the pubis descends lower than that near the sacrum, so that the parietal protuberance of the pelvic side becomes the presented part." (Murphy's 'Lectures on Parturition,' p. 62.)

These British writers do not adopt Dewees's principle, that "the great diameter of the child's head must constantly correspond to the great diameter of the pelvis ;" but assert exactly the opposite, viz., that the least axes of the head adapt themselves to the greatest of the pelvis. Rigby and Churchill follow Naegele, whose view of this subject, we agree with Dr. Millar, will claim a larger share of our attention than either of the preceding, because it is the latest fashion, and on that very account, if no other, attracting many votaries.

The chief questions raised in Naegele's description are : 1st. That "the head experiences but a slight rotation in the pelvis ; so slight, that it makes its escape at the inferior strait in nearly the *same oblique position* which it had on entering the pelvis." 2d. That it arrives at the inferior strait "without having undergone the least flexion—for, he says, sometimes the posterior fontanelle is lowest, sometimes the anterior." Dr. Millar strongly combats the doctrine, that the head very slightly rotates ; and feels bound to declare his own unshaken faith in the reality of such a rotation as he has ascribed to the head in the account already given of the mechanism of labour. On this point Dr. Millar does not content himself with merely theoretical reasonings, but engages Naegele on his own ground—experience.

“This faith does not repose upon authority only, but, as I flatter myself, upon careful and repeated observation. Like Naegele, I have, in numerous instances, watched the different evolutions of labour with my finger for hours, now on a fontanel, and anon on the commissures, and *if I have not almost uniformly verified the complete rotation of the head, I have as yet learned nothing of midwifery.*” (p. 267.)

Dr. Millar then describes the first position of the head as he has felt it, and concludes, “When I say all this is plainly and nearly invariably felt, the conviction must be riveted upon the mind of the observer, that *the head does verily completely rotate. Such is my experience, or if it prove unreal, my dream, for more than twenty years.*” His account of it is this :

“In a first position of the vertex, the posterior fontanelle is, at a certain stage of labour, plainly felt behind the left obturator foramen, while the lambdoidal commissure is to the left of the symphysis pubis, and the sagittal suture crosses the vagina obliquely from above downward, and from left to right; but in the further progress of the labour, the lambdoidal is brought completely forward, so that its branches cross the rami of the pubis equidistantly from the symphysis, and the sagittal suture runs vertically parallel with the genital fissure, while the posterior fontanelle is felt in the same line.” (p. 267.)

This is certainly diametrically opposed to Naegele’s observation; and although we are little disposed to question the accuracy of that distinguished authority, we are, nevertheless, inclined to think, that Dr. Millar is, in many instances, perfectly correct, and to believe that a complete rotation of the head much more frequently takes place, than those who adopt Naegele’s opinion are inclined to admit. We do not in the least assent to Dr. Rigby’s assertion, that “The head *never* advances with the occiput forwards under the pubic arch, as is stated in works on midwifery, still less with the sagittal suture parallel to the antero-posterior diameter of the pelvis.” (Rigby’s ‘Midwifery,’ p. 126.) We have certainly observed the complete rotation of the head in several instances; but at the same time admit that the oblique direction is that in which we have found the head most commonly expelled. Dr. Millar’s observations on this point are particularly valuable, because they are based upon personal experience, as contrasted with mere authority, which many authors too implicitly follow without judging for themselves. He cannot account for the conflicting experience of Naegele: “It would be presumptuous in me to charge him with having mistaken the exception for the rule; but the fact is unquestionable, that the head is sometimes expelled obliquely; how often, is not, perhaps, well ascertained.” (p. 268.) Neither can *we* reconcile the contradiction, unless we suppose that the female pelvis in Germany is not of the same form as that of Louisville; but if we were to assume for a moment that those pelves observed by Dr. Millar had a narrower pubic arch than in Naegele’s cases, an explanation might be attempted. The foetal head must descend in the oblique direction until it arrives at the arch of the pubis; if there be sufficient room beneath the arch, the occipito-parietal portion may fill it without the head altering its direction; but if not, the head must descend lower along the ischio-pubic ramus, pressing more and more on the perinæum, until the occiput can escape under the arch, the head rotating gradually forwards for this purpose. We shall not, however, venture upon this solution of the difficulty; but shall leave the opposing experiences in conflict, in the hope that this may lead to further observation. When we find authors less disposed to marshal themselves in the ranks of



favorite authorities, and more inclined to observe these phenomena for themselves, we may anticipate a greater uniformity in their descriptions; and although we can only partially agree with Dr. Millar on this disputed question, we give him the fullest credit for having discussed it in the only way that it can be treated,—on the basis of actual observation.

Unfortunately, however, we have more difficulty in reconciling Dr. Millar with himself, than with Naegele. His description of the rotation of the head is sufficiently perspicuous—the posterior fontanelle that was applied to the obturator space rotates under the arch of the pubis; but he does not explain how the fontanelle descended to the obturator space; and we can only arrive at that indirectly. Previous to entering the pelvis, “the occipito-frontal diameter is parallel with the left oblique diameter, and the bi-parietal is parallel with the right oblique diameter.” The left oblique diameter is a right line, “extending from the left acetabulum to the right sacro-iliac symphysis;” or, in other words, from the left ischial surface, which lies above, and posterior to, the obturator space. Now, if the posterior fontanelle is first applied to that surface, and is found afterwards lower down and more forwards, corresponding to the obturator space, some rotation must have occurred to cause this change of position; the further advance of the fontanelle can only be a continuance of the same rotation. We cannot, therefore, understand Dr. Millar, when he says “no such rotation takes place, in fact, *during the descent of the head.*” That rotation which he so much insists upon, occurs *during the descent of the head*; and that which he denies may be demonstrated from the data he has afforded us. When Naegele asserts that the head does not rotate at all, or only very slightly, his statement is perfectly intelligible; but, when Dr. Millar insists strongly upon the rotation of the head from the obturator space, and yet denies that any rotation takes place during the previous descent of the head, we confess that we cannot understand him.—With regard to the second question, whether the head enters the pelvic cavity in the occipito-frontal measurement, and arrives at the inferior strait without having undergone the least flexion, we quite agree with Dr. Millar, that it is an incorrect description of what occurs, and fully assent to his reasoning to prove that it is impossible it could occur.

“The flexion of the head is, doubtless, caused by the resistance of the superior strait, aided by the cervix uteri, which, although it be considerably dilated, does not at once allow the head to engage in its orifice. To understand how this resistance produces flexion, it is necessary to observe that the head forms a lever, by its articulation with the spine, and that, *in consequence of the articulation being nearer the occipital protuberance than the chin, the occipital arm of the lever is shorter than the mental.* Let it be observed, moreover, that the power exerted upon the body of the child by the contractions of the uterus, is transmitted to the head through the medium of the spine; and it is easy to see that *the resistance being equal at both extremities of the lever, the occiput must descend, because it is nearest the spine.*” (Millar, pp. 240-1.)

When the head is, therefore, pressed down by the contractions of the uterus, either against the partially dilated cervix uteri, or the superior strait of the pelvis, it is clear that the occiput must descend lower than the forehead; but when this force is removed, and the head is relieved from pressure, the longer arm of the lever (the forehead) must descend for the very same reason that Dr. Millar assigns; so that when the head

advances, the occiput is first when it retreats; the sinciput comes down to, at least, the level of the occiput. When an examination, therefore, is made, if it be during a pain, the occiput and posterior fontanelle will be plainly felt; but if in the interval, the anterior may, as Naegele describes, be traced even lower than the posterior fontanelle. It is only necessary that the head should have a certain degree of mobility, to produce these changes, as happens when it may be felt above the brim of the pelvis, or below the brim if the excavation be sufficiently wide; but, when passing through, or fixed within the brim, from which it cannot retire, the head must remain flexed, having the posterior occipito-parietal portion presenting. The difference, therefore, between Naegele and Dr. Millar may, perhaps, be reconciled by such an assumption. But, if we are to understand that, in the examinations to determine this question, the finger was kept "for hours" on the presenting part, both during a pain and in its absence, and that each author has described what he felt while the uterus was contracting, we must resign the attempt, and only state that the explanation of Dr. Millar appears to us more consistent with reason and our own experience. We cannot, however, at all agree with him as to the difficulty of feeling the anterior fontanelle. "Both Naegele and Rigby," he says, "speak of the equal facility of reaching the two fontanelles. . . . This does not quite agree with my experience, and I must candidly avow that, whether it has been owing to my awkwardness, or the limited reach of my finger, I have not been able to feel both fontanelles in a single instance." (p. 274.) We fear that "the limited reach of the finger" had something to do with this difficulty, because there seems to be the same obstacle as to the ear of the child. Commenting on Dr. Blundell's directions to feel the ear, Dr. Millar says,—“I must confess that my endeavours to feel and distinguish the ear have been, so far, quite unavailing.” (p. 275.)

Having disposed of these questions, we shall return to Dr. Millar's description of the "mechanism of the first or left occipito-acetabular position of the vertex," which he divides into "steps," as contradistinguished from "stages," which term is applied to the threefold division of the entire function of parturition. The first step is the flexion of the head; the second, its rotation; the third, its extension, by which the chin departs from the chest of the child; the fourth step, the external rotation of the head after its expulsion; the fifth step, the extrication of the shoulders.—The position of the head, before the *first step*, flexion, takes place, is thus described:

"Previous to the rupture of the membranes, and the occurrence of strong expulsive contractions, the head of the foetus is but slightly flexed upon the breast, and its diameters have nearly the following relations with those of the superior strait, viz.:—The occipito-frontal diameter is parallel with the left oblique diameter, and the bi-parietal is parallel with the right oblique diameter. The great or occipito-frontal circumference is, of course, parallel with the boundary of the superior strait. (p. 239.) . . . . . When the uterus assumes strong expulsive contractions, and the head is urged towards the entrance of the pelvis, the first step in the mechanism of labour commences, which comprises *the flexion and descent of the head to the bottom of the pelvic excavation*. Flexion of the head causes the occiput to descend while the forehead rises, and, consequently, the cervico-bregmatic diameter takes the place of the occipito-frontal, while the bi-parietal diameter remains as before. Two of the small diameters of the head, the cervico-bregmatic and bi-parietal, are thereby brought parallel with the oblique

diameters of the pelvis, and its lesser circumference is parallel with the axis of the superior strait. The essential condition above adverted to is complied with, and the head is prepared to enter the pelvis." (p. 240.)

We must deny the parallelism of the bi-parietal axis of the child's head with the oblique of the pelvis, because we believe nothing has been more clearly established than the accuracy of Burns on this point. Naegele, with nearly every author who has written upon the subject, admits that the parietal bone nearest the pelvis is lower than that next to the sacrum; that the head enters the pelvis *sideways*, in order to gain space, by avoiding the parallelism that Dr. Millar describes. This, however, may be only an oversight of Dr. Millar's; because, when the head descends into the pelvic excavation, he gives an accurate description of its position, although one not easily adapted to his theory. "At this time it is evident that, although the head fully occupies the pelvis, and the right parietal bone, which is anterior, is felt considerably below the symphysis pubis, the vertex is still directed towards the sacrum, and the sagittal suture is placed so far posteriorly, that it can only be reached by introducing the finger deeply, and curving it forwards."

There is certainly no parallelism here, and the position thus described is precisely that in which the head enters the pelvic cavity, which could not be the case if the bi-parietal axis was coincident with the oblique axis of the brim. Of the *second-step*, rotation, Dr. Millar gives the following account:

"Arrived at the bottom of the pelvis, the head is forced to execute a rotatory movement; the occiput advancing from left to right, and forward towards the symphysis, under which it is finally placed. . . . Not unfrequently, when the rotation is carried so far as to bring the occiput behind the left ischio-pubic ramus, it is arrested for a time; the posterior superior part of the right parietal bone projecting, meanwhile, in the pubic arch, the perinæum beginning to be distended. . . . It is under these circumstances that a swelling is apt to form on the liberated portion of the cranial integuments, viz.,—over the posterior superior part of the parietal bone, which may continue for some time after birth. . . . I have said, when the head reaches the inferior strait, it is *forced* to rotate, by which it is to be understood that the head could make no further progress without first undergoing this movement. To escape from the pelvic excavation, the head must move in the direction of its inferior aperture. . . . But this can be brought about only by its rotation, which enables the occiput to emerge first under the symphysis pubis, when the axis of the head is placed parallel with the axis of the inferior strait; and the essential condition being now complied with, the head is prepared for the sortie. *Previous* to this rotation, the occipito-frontal diameter tends toward the left oblique diameter of the inferior strait—the occipito-frontal, or great circumference, tending towards its place; and it is demonstrable, with a pelvis and foetal head, that the dimensions it then offers cannot pass out, except the head be very diminutive, or the pelvis very capacious; that is, *the head cannot, except under peculiar favour, clear the inferior strait in a diagonal or oblique position.*" (p. 243.)

This paragraph contains the essence of Dr. Millar's theory of the mechanism of the expulsive stage of labour, as contrasted with, and opposed to, that of Naegele. The head descends to the bottom of the pelvis without any rotation; it then rotates from the oblique into the antero-posterior axis; so that "*after* rotation, the cervico-bregmatic diameter is parallel with the coccy-pubic." We have already pointed out what appears to us Dr. Millar's error in the *manner* of the head's rotation,

and, we trust, have shown, even from his own descriptions, that if a rotation at all take place, it must have commenced from the moment the head enters the pelvic cavity, having the occiput applied to the surface of ischium. It is our duty, now, to rebut his second position, that "the head cannot, except under peculiar favour, clear the inferior strait in a diagonal or oblique position." We do not seek for support from Naegele, Dubois, Rigby, and others, who have seen the head of the child so frequently thus expelled, as to consider it the regular order of its progress. We are satisfied with Dr. Millar's own method of proof—the pelvis and foetal head,—and venture to assert that it is demonstrable, from an examination of them, that the head *can* pass out in the oblique as well as in the antero-posterior axis of the outlet. The space in the outlet is thus described by Dr. Millar:

"The inferior strait has the same number of diameters as the superior, viz.,—an antero-posterior or coccy-pubic, a transverse or bis-ischiatic, and two oblique diameters. . . . With regard to the measurement of these diameters, the same differences are to be found among authors as in reference to the superior strait. Baudelocque, and most French authors, state that they are equal to one another, and measure four inches; and this is not far from the truth in the dried pelvis, with the ligaments attached; but, in the living body, both the antero-posterior and *the oblique* may be rendered more capacious than the transverse, by the yielding of the *os coccygis* and *the sacro-ischiatic ligaments*—the antero-posterior admitting the greatest enlargement." (p. 7-8.)

From this it appears that the antero-posterior and oblique axes are equal—four inches; that both may be increased by the yielding of the *os coccygis* and *sacro-iliac ligaments* to more than four inches; "the occipito-frontal (the great) diameter of the head measures four and one fourth to one half inches." Hence, then, it is possible for the head, having its great axis in the oblique measurement of the pelvis, to pass through in that direction, not only because it admits an increase of space, as well as the antero-posterior, to more than four inches; but also because, while the head is emerging, the posterior portion glides forward over the ischio-pubic ramus, and thus gives additional room; the antero-posterior axis of the head then being taken from the back of the neck to the forehead, which is less than the occipito-frontal. We confess, therefore, it appears to us, that Dr. Millar has demonstrative evidence as little in his favour as the support of experience; and, while we agree with him that the rotation he describes happens more frequently than is generally admitted, we have just as strong an objection to his assertion, that "the head is *forced* to rotate," as to Dr. Rigby's statement, that it *never* rotates. We think that both modes of expulsion take place, and that the selection is very much governed by the width of the cavity and outlet of the pelvis;—the large cavity and open outlet permitting the head to retain its oblique direction, while the more conical excavation and narrower arch oblige the head to rotate completely, in order to be expelled.

There is nothing in the fourth or fifth steps that require notice, except M. Gerdy's explanation of the cause of the occiput turning to the left, and the face to the right, when the head is expelled:

"This movement of the head was called by Baudelocque its *restitution*, because he considered its first rotation in the cavity of the pelvis a twist of the neck, in which the trunk does not participate, and when the head is free from constraint, it

resumes its natural position in relation to the trunk by the elasticity of the ligaments of the neck. The correctness of this explanation has lately been questioned by M. Gerdy, as we learn from M. Cazeaux, who adopts his views of the matter. According to M. Gerdy, the trunk of the child participates in the first or internal rotation performed by the head, so that the shoulders are simultaneously placed *nearly* transversely in the pelvis, instead of remaining oblique as they were when labour commenced. They arrive at the inferior strait, in this nearly transverse position, the right shoulder being a little anterior, where, encountering resistance on account of their bis-acromial diameter being offered to the smallest diameter of the strait, they undergo another rotation in an opposite direction, viz., from right to left towards the symphysis pubis, and the head, being free, simply follows the movement of the shoulders." (p. 245.)

This explanation of the cause of this movement receives the support of Dr. Millar; who observes:

"One of the arguments adduced by M. Gerdy in favour of his theory of external rotation, it will be found difficult to controvert, viz., the fact, that instead of turning towards the left thigh, the occiput sometimes continues to look towards the pubis for a few moments, or until there is a recurrence of efficient uterine contraction, and then revolves toward the right,—the interval or first rotation being continued, and the child being expelled under a long spiral movement. I have, on several occasions, distinctly observed this phenomenon, and it is not possible, I think, to reconcile it to the theory of Baudelocque." (p. 246.)

The third position deserves attention, because Dr. Millar's views form a kind of commentary on the explanation of its mechanism, which has originated with Naegele:

"Flexion and descent of the head are the same in the third, as in the first and second positions; the peculiarities of it are connected with, or consequent to, the *rotation* of the head, which will, therefore, claim our chief attention. This movement may be accomplished in two modes; the occiput may be thrown into the hollow of the sacrum, or it may be conveyed under the symphysis pubis." (p. 249.)

With regard to the rotation of the occiput into the hollow of the sacrum, Dr. Millar makes this a complete rotation; the forehead being behind the symphysis, the head in this position could not possibly escape; therefore Dr. Millar makes it take an *extra-step*:

"An extra-flexion of the head now commences, under which the occiput is depressed while the forehead mounts higher behind the symphysis pubis, until the occiput emerges before the anterior edge of the perinæum. This extra-flexion establishes such relations as allow the head to be delivered, for the cervico-bregmatic diameter is now nearly parallel with the coccy-pubic, and the bi-parietal with the bis-ischiatic." (p. 250.)

We cannot perceive how this extra-step relieves the difficulty of this position; on the contrary, it seems rather to increase it, because in place of the forehead getting under the symphysis pubis and becoming a fixed point for the occiput to pass round while it is moving along the perinæum, it rises higher behind the symphysis, so that the greater part of the head, and "thickness of child-neck (to say nothing of the trunk necessarily drawn into the excavation before the occiput is disengaged), intervenes between its chin and the sacrum;" besides this the bi-parietal axis of the head is forced to pass through the transverse axis of the outlet. To have witnessed such an expulsion of the head in this position, may have been within the range of Dr. Millar's experience; we have not been so fortunate as to notice it, and for the above reasons it appears to us ex-



ceedingly difficult to accomplish. A frightful rupture of the perinæum must be the consequence, because even when the occiput has passed it, the head must still descend in the same position some distance before the forehead could clear the arch of the pubis ; all which time the back of the neck is pressing strongly against the perinæum, so as greatly to increase any slight laceration that the occiput might cause. We do not believe Nature so improvident ; and are, therefore, more disposed to adopt the opinion, that as a rule the head in this position passes out of the pelvis in the direction that we have seen it, viz., in the oblique axis of the outlet ; the forehead resting, not against the symphysis pubis, but the ischio-pubic ramus, along which it descends, so that sometimes both forehead and occiput are expelled together. The bi-parietal axis of the head is not in any manner applied to the bis-ischiatic measurement of the pelvis, on the contrary, it cuts it from above downwards at an acute angle. Dr. Millar's explanation of the "rotation of the occiput towards the pubes" is more intelligible, although it is obliged to take *a stride* in order to perform the complete rotation that he so much insists upon. "In this anterior rotation of the occiput, the head takes *a stride* instead of the corresponding *step* belonging to the first and second vertex positions, for the occiput is brought from the right sacro-iliac junction and deposited under the symphysis pubis." (p. 252.) With regard to the comparative frequency of this anterior-rotation, Dr. Millar observes :

"My own observation has fully satisfied me, that the anterior rotation of the occiput is more common than the posterior, although, like M. Moreau, I once thought differently—too credulously relying on the authority of others, particularly of Baudelocque and Dewees.

"While under the influence of this erroneous opinion, I met a good many cases of occipito-posterior positions, in which anterior-rotation was effected ; but the efficiency, I believed, belong to me and not to Nature, because I laboured assiduously to promote it after the manner recommended by Baudelocque and Dewees. . . . I have since experimentally allowed Nature to take her course in a considerable number of such cases, and *I find that the desired mutation is generally accomplished about as well without as with my assistance* ; and that when the occiput comes forward during the pains and retreats in the intervals, it will be vain to attempt to turn the head round by the pressure of one or two fingers. After wearying myself by fruitless efforts of this kind, I have sometimes altogether desisted, and Nature, though she would not be hastened, has done her work in such time as was most pleasing to herself." (pp. 255-6.)

These observations of Dr. Millar are of the greatest value, because they are a practical illustration of the importance of studying and knowing what Nature really does and can do ; they are also a very significant commentary on the rule laid down by Baudelocque, J. Clark, and, especially, Dewees, who holds a man "incompetent to practice midwifery in its best manner, who cannot detect and change this malposition of the head, and thus abridge, by several hours, the misery and pain of his patient." (Dewees's 'Midwifery,' p. 255.) Dr. Blundell observed, "that a meddling midwifery is a bad one;" we believe that a better illustration cannot be afforded, than what may be derived from these practical remarks of Dr. Millar.

Another question of some importance is the proportionate frequency of these anterior-rotations of the third position. Dr. Millar's experience does not agree with Naegele and Rigby :

"In subscribing to the doctrine of the greater frequency of anterior rotation, I must not be understood to agree with Naegele and Rigby in the opinion, that it has really taken place in almost every case where the vertex is found in the second position; that it is, in fact, as the latter states, the regular commencement of labour in third positions, which are, according to them, so much more common than original second positions. I have expressed my doubts of the accuracy of this statement, in the preceding chapter, and I may here add, that I cannot believe with Dr. Rigby, that the third positions have been so generally overlooked or mistaken as he imagines. At all events, I cannot suppose that they have so strangely eluded my observation and imposed themselves upon me as second positions; and yet this must have happened, *if I have not met with many more cases of second than third positions.*" (p. 256-7.)

Although we do not place unlimited confidence in obstetrical statistics, being just that branch of the science that may be made to prove anything, we think it would have been desirable to have had the advantage of Dr. Millar's experience embodied in a statistical table of positions. It would be valuable as adding to those already published, and, by increasing numbers, might bring the conclusions nearer to truth. There is at present a great discrepancy, on this point, between Dugès, La Chapelle, Boivin, and others on the one side, and Naegele, Dubois, and Simpson on the other, as the following table will show :

	1st Position.	2d Position.	3d Position.	4th Position.	Total.
Dugès.....	27,443	7512	276	144	35,375
Dubois ...	1355	55	491	12	1913
Naegele ...	850	1	359	—	1210
Simpson ...	335	256	1	76	668

We regret this deficiency the more, because Dr. Millar is fully aware of the confusion in which the subject is at present placed :

"It is obvious, that between these imposing authorities, Naegele, Dubois, &c., on the one hand, and Dugès, seconded by Baudelocque, Gardien, Capuron, Boivin, La Chapelle, &c., on the other, there is irreconcilable variance. Future experience must settle the question of accuracy, I say not of veracity, between them, for neither can be supposed intentionally to misrepresent. The sense of touch is confessedly fallacious; but I should have no confidence in it whatever, could I suppose that MM. Naegele, Dubois, &c., are right in this matter, for my touch continues to this day to make contradictory revelations to me, nay, I find by reference to my note-book, that the fourth position has occurred in my practice oftener than third, but both together less frequently than the second." (p. 229.)

We could have wished that the results of the note-book had been given to us in a more precise form, in order that we might judge more accurately as to the comparative frequency of these positions.

We have dwelt at some length upon the mechanism of vertex presentations, because the view of this subject founded on practical results constitutes one of the chief novelties of the work before us. Dr. Millar's description of this mechanism is not original; it is a revival of that propounded by Baudelocque and adopted by the French school, and stands in opposition to the later doctrines taught by Naegele in Germany; nevertheless Dr. Millar must not be deprived of his claim to originality. He has not adopted the opinions of the French authorities, merely because he regards them with respect; but he evidently has judged for himself (rather

original at the present time), and decides in favour of the explanation that his experience tells him is correct. We are glad that Dr. Millar has published views, which, in this sense, are new; because we trust that they may induce further inquiries in the only way that investigation should be made—at the bedside. The great value of Naegele's descriptions consist in their being founded upon accurate and faithful observations; but, like Baudelocque, he has been rather too tamely followed by authors, who take for granted that which he has described, without confirming what he has said. We are pleased, therefore, to see in Dr. Millar an opponent who may rouse the energies of the Naegele school to a more strict and practical examination of their opinions.

*Face presentations* are the most nearly allied to those we have been considering, and may be conveniently discussed after vertex positions. Dr. Millar, however, adopts a different arrangement; and we have to turn over several pages, passing by Nates Positions, to arrive at his chapter on "Face Presentations, Mechanism, Diagnosis, and Prognosis." We cannot congratulate him on his success in simplifying this subject; on the contrary, his account of it seems to us exceedingly obscure. He divides these positions into *primitive and secondary*, "that is, the head may be completely retroverted, causing the face to offer fully at the superior strait when labour begins, or it may be only partially retroverted, in which case the anterior fontanel is found presenting at first, but in the progress of labour this is replaced by the face." (p. 252.) We confess never to have met with these primitive positions, and we find authors nearly all agreeing in the partial character of face positions; they almost describe Dr. Millar's secondary position as being the face presentation. The division, therefore, appears to us quite unnecessary. This, however, is not of so much importance as the description which Dr. Millar gives of the face presentation itself:

"It will be remembered that we admit but two positions of the face, namely, the *left fronto-iliac* and the *right fronto-iliac*. In the first, the forehead corresponds to the left iliac fossa, and the chin to the right, the fronto-mental diameter is parallel with the transverse diameter of the pelvis, and the bimalar diameter is parallel with the sacro-pubic; the back of the child looks toward the left side of the mother, and its breast toward the right; its right side is forward, and its left backward." (p. 353.)

We have already alluded to the term "*fronto-iliac*," as being likely to be confounded with those vertex positions described by authors under the same name; but Dr. Millar's description of these "*fronto-iliac*" face positions seems to us much more objectionable than the term itself. Dr. Millar's name represents Dr. M.'s face-presentation, as accurately as a shadow does the substance, but it also portrays its imperfections. We do not believe that the face occupies the transverse axis of the brim of the pelvis; and a little reflection, we think, would convince Dr. Millar that it could not do so. The mento-frontal measurement of the head is quite as long as the occipito-bregmatic, and requires as much space; if the latter, therefore, is obliged to seek the oblique axis of the brim to obtain room to advance, so must the former; the face must lie in the oblique axis of the brim; and the only question to determine is, whether the chin inclines forward towards the pubis, or backwards towards the sacrum. Burns, Rigby, Murphy, adopt the former, Churchill the latter view. If we assume that the chin is inclined forwards, or, to speak more accurately, that it corre-

sponds to the anterior plane of the ischium, there is no difficulty in perceiving that as it descends it takes the same course as the occiput in vertex positions; it glides in a spiral course beneath the arch of the pubis, and the face may be delivered, when so placed, as easily as the vertex. Dr. Millar, however, keeps the face in the transverse axis until it arrives at the bottom of the pelvis, when rotation commences, by which the chin is brought under the symphysis pubis, and the vertex is thrown into the hollow of the sacrum. Taking this view of its mechanism, we do not wonder that Dr. Millar should find face positions liable to several anomalies; one of these he thus describes:

“First. *Rotation may take place before the face has completely descended in the pelvis.* To understand the reason of this, it is necessary to observe that the length of the child’s neck is not sufficient to allow the face, in any case, to reach the inferior strait in a transverse position, so as to have the chin upon a level with one ischiatic tuber, and the forehead upon a level with the other; for the depth of the lateral walls of the pelvis exceeds the length of the neck. In order, therefore, that the face may complete its descent *regularly*, flexion must take place in a slight degree, that is, the chin remaining as low as the neck will permit, the forehead must be pushed down to the floor of the pelvis. . . . Now, instead of thus flexing to reach the inferior strait, the head may rotate the chin forward, behind the symphysis pubis, and then the anterior part of the neck being opposite the short or pubic wall of the pelvis, there is no obstacle to the speedy completion of its descent. When the face traverses the pelvis in this manner, there is first descent, as far as the neck will allow, then rotation, and finally descent resumed and completed. These anomalous movements, as I regard them, are described by M. Cazeaux as the regular march of natural face presentation.” (p. 356.)

We quite agree with M. Cazeaux, and think that Dr. Millar has made the exception the rule, and the rule the exception. We cannot help expressing our surprise that not only Dr. Millar, but other authors, should insist upon making the face descend into the pelvic cavity in a direction which they admit the vertex cannot. The vertex selects the oblique (the longest) axis of the brim, but the face prefers the transverse which is shorter, descends in that direction as far as the neck will let it, and then finding it cannot go farther, suddenly turns as if it mistook its road into the proper path, and is delivered. We believe that Nature very seldom makes such a mistake; she obliges the face as well as the vertex to descend in the oblique axis, and if the chin be anterior, its course, until it arrives at the pubic arch, is obvious. When the chin is posterior, its rotation under the arch of the pubis is not so easily accomplished. This, then, becomes Dr. Millar’s second anomaly:

“Second. The head may rotate so as to throw the chin into the hollow of the sacrum, or the chin, being directed towards one of the sacro-iliac symphyses from the beginning, may retain its posterior look from default of rotation. If there have been no interference with the regular progress of the labour, it is exceedingly rare that rotation fails to carry the chin forward, and place it under the symphysis pubis. This occurs in the diagonal position of the face, where the chin is opposite one of the sacro-iliac symphyses, with even greater uniformity than does the revolving of the occiput forward in posterior positions of the vertex.” (p. 357.)

We give our cordial assent to this proposition, and believe that when the chin is posterior it still describes the longer rotation that is necessary to bring it under the arch of the pubis, but it does not do so always, and then the question arises:—Does the chin, when opposite the sacro-iliac symphysis, descend into the hollow of the sacrum, and so pass out over the perinæum to be delivered? Dr. Millar very properly observes:

"This is physically impossible, where the child is fully developed, as Madame La Chapelle has irrefutably demonstrated. It is impossible, because either the sternum and clavicles must abide at the sacro-vertebral angle, until the chin passes out before the perinæum, which would require the neck to be so enormously stretched, as to measure the whole length of the sacrum, coccyx, and perinæum (at least eight inches), or the thorax must be drawn into the excavation between the head and sacrum, and be so flattened as to occupy not more than two inches of the antero-posterior diameter of the excavation, leaving three inches for the cervico-bregmatic diameter of the head. The head, therefore, cannot be expelled by the natural efforts, or extracted by art in such cases, unless the position be first changed to one more favorable, or transmuted into a vertex position." (pp. 358-9.)

Thus may be explained the source of contradiction amongst authors respecting face presentations. Some, as Boer of Vienna, Madame La Chapelle of Paris, Collins of Dublin, state that face presentations are delivered by Nature as easily as the vertex: while others, like Dewees, represent the difficulties of a face presentation as always causing a prolonged labour, and generally requiring the assistance of the forceps. The latter may have fixed their attention upon those exceptional cases that are above referred to, in which the face, having taken a wrong direction, could not be delivered until the forceps were used, or some transmutation of the position had taken place. Dr. Millar, however, admits that—

"Although presentations of the face were for a long time regarded as essentially preternatural, it may be easily demonstrated that they do not necessarily offer any obstacle to parturition, which the natural resources cannot surmount, nay, that so far as the *passage of the head merely* is concerned, *there is no material difference between them and vertex presentations.*" (p. 362.)

The only difficulties, beside that just alluded to, which Dr. Millar notices, appear to us to be those of his own creation; for instance, secondary positions he supposes are unfavorable, because "the forehead is at the centre of the superior strait." We do not think that the forehead is generally so situated, on the contrary, we have found it usually resting on the margin of the brim; but even if we assume such to be its position, unless it descend and be fixed there, its mobility above the brim is such as to offer no impediment to the advance of the face, otherwise it becomes a presentation of the forehead, such as sometimes happens. Another difficulty of Dr. Millar's is thus described:

"The face may unquestionably engage in the pelvis as readily as the vertex, nor is there any reason to believe that it may not descend as readily until its free progress is arrested by the shortness of the neck; afterward, as it can continue to advance only by becoming flexed, unless it rotate, the head's axis is made to decline from the axis of the pelvis, as the chin moves towards, and is pressed against, the pelvic parietes, consequently the greater diameters of the head are brought into the pelvis, which must impede the completion of its descent, if it be large, or the pelvis contracted, or the soft parts resistive." (p. 365.)

Dr. Millar's theory about rotation is the whole cause of this difficulty; if he would allow the face to rotate in the first instance, the shortness of the neck would not be in his way; but as he insists upon its not doing so until the face arrives at the bottom of the pelvis, it is necessary to introduce a new movement, an additional flexion of the head, to relieve the difficulty of a failure in this respect.—We shall only allude to one other point connected with Dr. Millar's description of a face presentation. He makes "the bi-malar diameter of the face parallel with the sacro-pubic of the pelvis." We cannot give our assent to this, but must express our



surprise that a practical writer like Dr. Millar should allow himself to be thus led away by the too-ingenious mechanism of the French school; we believe that nothing is more palpable than the accuracy of Naegele in this respect, viz. that the cheek next to the pubis descends lower than that next the sacrum, and that consequently the bi-malar axis is never coincident with the sacro-pubic or any other axis of the brim of the pelvis.

We shall not dwell on the mechanism of the *Nates presentation*, as we agree with Madame La Chapelle, that "on account of the softness of the parts constituting them, nates presentations accommodate themselves more easily to the different forms of the straits; they are readily moulded, and have, consequently, less occasion to change their direction to acquire the most advantageous relations with the great diameters of the straits and excavation." We believe that no matter how the pelvic extremity of the child enters the pelvis, it will ultimately be expelled in the proper direction; because in obedience to the law first propounded by Professor Naegele, and now confirmed by Dr. Millar, there is "*a strong tendency of the back parts of the child, in the dorso-posterior position, to revolve forwards so as to bring the occiput towards the pubis as the head engages in the pelvic cavity.*" But, in order to accomplish the salutary rotation, there must be no meddling interference; we believe half the difficulties of breech presentations are caused by the precipitancy of over-anxious practitioners, who in their efforts to assist Nature, prevent this rotation, by forcibly bringing down the body in the direction that it first presents itself, and, consequently, confirm an unfavorable position. We cannot, however, omit some allusion to Dr. Millar's observations on the diagnosis of breech presentations. Having alluded to the point of the os coccygis, the anus, and the genital organs, as the chief diagnostic marks, he comments on the difficulty that occasionally arises in recognising even these:

"Let none imagine, however, that it is always an easy matter to ascertain how the breech is situated, or even to recognise itself when it is presenting. Previous to the rupture of the membranes, it may be placed too high, or be too obscurely felt, and after the escape of the waters, it may be so disfigured by tumefaction, from long detention in the pelvis, that its natural features are obliterated. In this latter condition, Baudelocque informs us, the best-instructed practitioners have mistaken it for one part and another, even for the head of the child, the integuments of which were supposed to be engorged and swollen. A very celebrated accoucheur, he states, having mistaken the breech under such circumstances for locked head, applied the forceps successfully, and considered the mistake fortunate, as it taught him a new resource in difficult breech presentations.

"But, it may be said, how much soever the breech may be deformed by swelling, the anus is so characteristic, it ought to be sufficient to prevent such mistakes. Aye, so it ought, if it were always found a closed and puckered orifice; but if the child be dead, and a curious examiner have been poking at it before we are called, it may be gaping and tumid, and feel like the mouth, while the buttocks, to the touch alone, are not unlike the cheeks. No wonder, then, if it should be mistaken for a face presentation, one instance of which is within the compass of my own knowledge—in sooth, *magna pars fui.*" (p. 332.)

Dr. Millar honestly confesses the mistake, that we are sure has been made by many more than him and the veteran professor of *l'Ecole de Médecine*. It is sufficient proof of the extreme difficulty of diagnosis by touch, even with the most experienced practitioners.

With regard to *Shoulder presentations*, we shall only allude to Dr.

Millar's observations on their natural delivery, called by Dr. Denman "spontaneous evolution ; by Dr. Douglas "spontaneous expulsion ;" and by Dr. Millar, who wishes to remodel the nomenclature, "duplication" of the foetus. "Considered as a description of what occurs in the great majority of instances of natural expulsion in shoulder presentations, Dr. Douglas's narration must be reckoned to be in the main faithful ; but his reasoning against Dr. Denman's hypothesis is not entitled to much weight." Like Burns, Dr. M. does not agree with Douglas, that Denman's evolution is impossible ; on the contrary, he believes, that "in a certain number of cases (the proportion being probably small) nature does proceed after this manner, performing a genuine version of the child. Still it undoubtedly is according to the other manner, described by Dr. Douglas, that Nature usually operates." (p. 384.) Although this spontaneous delivery of a shoulder position may take place, Dr. Millar very properly objects to the doctrine founded upon it, that the woman would not die undelivered if no assistance were afforded to her ; there is abundant evidence to prove the danger of an expectant treatment under such circumstances :

"There are, nevertheless," he observes, "cases in which we shall be justifiable in deferring to act, in hope that the child may be expelled by the process of duplication ; as, for example, in premature labour, where the small size of the child warrants the expectation that it may easily pass, doubled upon itself ; or where in labour at the full period of pregnancy, the pains are usually powerful and frequent, and the child is already forced so low down in the pelvis as to distend the external parts. A remarkable instance of the latter kind, which came under my own observation, I may be permitted to relate. February 27th, 1846, I was requested by Dr. Donne, now one of the professors in the Memphis Medical College, to accompany him to the house of Mrs. B—, in Market Street, who was in labour with her second child, under the disadvantages of a shoulder presentation. I found Dr. Lewis Rogers at the house, who had made an effort to turn, but was defeated by the strength of the uterine contractions. It was plain, from the patient's behaviour, that the pains were still exerted with unusual vehemence as well as frequency, and I proceeded, as soon as possible, to make an examination ; when it was discovered that the left shoulder presented in the second (scapulo-sacral) position, with the arm extended, and the hand protruding through the vulva. Before the examination was completed, however, the perinæum began to be distended, and I remarked to the medical gentlemen, that the child would probably be expelled by duplication, which did accordingly occur in a few moments afterward, in the manner already described. The child, which appeared to be fully developed and of average size, was born dead ; its left arm being considerably swollen, and the left side of the neck, with the corresponding cheek, retaining marks of the contusion they had suffered. The labour was not unusually protracted, and the mother recovered without any unfavorable consequences. It should, perhaps, be observed, that the time when this case happened seemed to be propitious for *independent* child-bearing, as no fewer than four women, to whom I was called, were delivered by dame Nature (by duplication?) before I could reach their domiciles, albeit, I made as much haste as is consistent with obstetric dignity." (p. 392.)

Here, for the present, we shall quit the subject, having thus fully and critically examined Dr. Millar's account of the mechanism of the second stage of labour in connection with the obstetric properties of the pelvis. On another occasion we shall resume the subject, and review those portions of our author's doctrines, which want of space obliges us, for the present, to pass by.

## PART SECOND.

**Bibliographical Notices.**

- ART. I.—1. *The Profession of Medicine, its Study and Practice; its Duties and Rewards. An Address, delivered at St. Bartholomew's Hospital on the Opening of the Medical Session of 1850-51.* By CHARLES WEST, M.D. F.R.C.P., &c. &c.—London, 1850. 8vo, pp. 32.
2. *The Studies of the Medical Man; being the Introductory Address, delivered at the Theatre of Anatomy and Medicine adjoining St. George's Hospital, at the opening of the Session of 1850-51.* By EDWIN LANKESTER, M.D. F.R.S. F.L.S., &c. &c.—London, 1850. 12mo, pp. 24.
3. *An Introductory Address, delivered at the London Hospital Medical School at the Opening of the Session 1850-51.* By NATHANIEL WARD, F.R.C.S., &c. &c.—London, 1850. 8vo, pp. 31.
4. *Address to the Medical Students of London, Session 1850-51.* By J. STEVENSON BUSHNAN, M.D. M.R.C.S., &c.—London, 1850. 8vo, pp. 16.
5. *Our Institution and its Studies. An Introductory Lecture, delivered at the Bristol Institution for the Advancement of Science, Literature, and the Arts, on Monday, Sept. 23, 1850.* By JOHN ADDINGTON SYMONDS, M.D.—London, 1850. 8vo, pp. 38.
6. *Happiness in its Relations to Work and Knowledge. An Introductory Lecture, delivered before the Members of the Chichester Literary Society and Mechanics' Institute, Oct. 25, 1850; and published at their request.* By JOHN FORBES, M.D. F.R.S., &c.—London, 1850. Fcap. 8vo, pp. 75.

To all those who may have thought it worth while to glance from time to time at the Introductory Lectures which it is now the ordinary practice to deliver at the commencement of the medical session in each school, it must have become apparent that a great improvement has taken place in their general tone. An "Introductory" was formerly but too commonly a dry summary of the history of medicine, or of the special department of the science professed by the lecturer; or it consisted of a hasty sketch of the curriculum of study on which the auditors were supposed to be entering, the special department professed by the lecturer being here also raised into extraordinary prominence, and too often magnified at the expense of the rest; and in either case, the latter part of the lecture was pretty sure to contain a glorification of the school at which it was delivered, with a few common-place exhortations designed to teach the art of rising in the world by the attainment of success in practice.

Whether such lectures are still delivered, or not, we do not profess to know; for if they are, their authors wisely abstain from publishing them.

In those now before us, as in nearly all that we have seen of late years, a much higher tone is taken. The student is led to consider the true dignity of the profession he has embraced, the highest objects of his study, the mode of pursuing it which will be most likely to promote, not only his own worldly success, but the credit of the body of which he forms a part; and to secure not merely his own self-approbation, which may spring from a most erroneous view of his character and position, but the favour and approval of the omniscient Judge who "seeth not as man seeth." There is seldom any attempt to lead the student to any partial exercise of his time and industry, by urging his attention to any one particular study; the mutual bearing and essential utility of all being duly recognised. And of the offensive laudation of the institutions with which they happened to be associated, in which so many lecturers were wont to indulge, we do not find a word.—Our limits do not permit us to do more than make a short extract from each of the medical "Introductory" before us, by way of sample of their contents.

I. The general scope and character of Dr. West's lecture are indicated by its title; and all who have learned to estimate his worth from his previous writings, as we trust that most of our readers have done, will be prepared to expect a rare combination of dignity and philosophic discrimination, with taste and elegance, and the earnestness of true and noble feeling, in such a composition. Nor will they be disappointed. Our extract will show what we believe to be the truest appreciation of those collateral studies, on which the teachers of the "more practical" subjects have too often thought it becoming to throw the stigma of inutility. The *cui bono* of these pursuits has nowhere been to our minds so tersely or happily expressed, as in the following passage:

"It is a mistake to suppose that the sole object of your sojourn here is the gaining of the greatest possible amount of practical, technical, acquaintance with diseases and their remedies. It is true, indeed, that everything you do must be made subservient to this as your ultimate aim, and that the chief difference between the man of great experience and the man of little consists in the more abundant possession of this knowledge by the former. But there are certain prerequisites, without which all the experience in the world will profit you but little; since, without them, you will be as likely to collect the husk as the precious grain, and to store up things valueless as things of greatest worth. One grand intention of the course of study you are about to pass through here is, to furnish you with these requisites, by such training of your intellectual powers as may best bring forth their vigour, may strengthen your memory, quicken your observation, beget in you the habit of noting and arranging facts, of estimating probabilities, and of reasoning rightly on that peculiar kind of evidence, on which you will have to found all your decisions, and which falls short in every case of actual demonstration or mathematical certainty. In all your studies there is a twofold object: partly disciplinal, to bring out those qualities of mind of which you will have most need; partly practical, to obtain that knowledge in the application of which your future life is to be spent.

"Viewed in this light, chemistry, botany, comparative anatomy,—subjects, the bare enumeration of which now almost alarms you,—will cease to appear as merely superfluous additions to your other studies, keeping you back from more important pursuits; and you will see that, like the young soldier's exercises at drill, they are means by which you both increase your familiarity with the weapons that you will have to wield, and develope and strengthen those powers by which you will be

enabled to wield them with efficiency. Still, 'Lyfe is short, and scyence is full long:' and some of these studies you will but just have time to glance at. You are not to be botanists, nor comparative anatomists, nor chemists,—but doctors. With each of those sciences you may discipline your minds; and of the grand facts of each may be held fast even by the least retentive memory. It is neither expected nor desired that you should all become botanists,—but it is wished that none of you should be ignorant of the correspondence between the intimate structure of plants and animals; nor comparative anatomists,—but that you should know how the archetypal forms of various animals are but slightly different expressions of the same idea, all pointing to a unity of design which runs throughout the whole; nor chemists,—but yet that you should be aware that within the body, as without, the same great laws which govern inorganic nature still obtain; but modified, and made subservient to the support of life, and to the maintenance of health.

"It is the possession of knowledge such as this which marks the difference between the man who follows his profession in the spirit of a philosopher, and him who pursues it as a handicraft, limiting his endeavours to the acquisition of just that kind and amount of knowledge for which there will be most demand in the duties of every-day life. The latter, indeed, if for a moment he raises his thoughts beyond this narrow circle, looks out upon irreconcilable difficulties and contradictions; but the same things which seem to him to 'clash and interfere,' are discerned by the former to be all 'concentric and harmonious;' full of that wisdom with which 'God, the divine Harmostes, has ordered them in such tunable and methodical proportions.'" (pp. 9—11.)

We should gladly have added an extract from the latter part of the lecture, on the rewards which the medical practitioner should keep in view as his *true* objects; but we would not deprive the passage of any of its eloquence by an attempt at condensation, and it is too long for admission as a whole.

II. Dr. Lankester has made the relations of medicine to other departments of science the chief subject of his lecture; which, though written (as he informs us) *currente calamo*, bears evidence of that philosophical *habit* of thought, which might be expected from the enlightened interest he has shown in Natural Science. We are glad to find him candidly confessing the imperfections of our existing system of therapeutics, as a reason for urging upon his auditors to refrain from implicit faith in dogmatic teaching, but to apply themselves to personal observation, and thus to qualify themselves for contributing to its improvement:

"Although the treatment of disease to the unobservant seems the most perfected part of our art, in reality it is that of which we know least. The history of the reputation of various remedies would serve as the best commentary on this observation. Very limited indeed is the list of agents which the universal experience of medical men has shown to be of value in the treatment of disease, but these few are among the most valuable we can employ. In order to place therapeutics on a sounder basis, we must still accumulate evidence of the action of remedies in health and disease. To do this we must know the natural progress of disease, as well as the natural action of the remedy, before we can draw satisfactory conclusions with regard to the merits of particular remedies, or sets of remedies, in particular diseases. Although we are improving, our system of reporting cases is still inadequate to meet the demands of the scientific investigator of the action of remedies. An inference may be correct on the action of a remedy in a single case, but the probability of its truth becomes constantly increased by numbers; but in the majority of instances single cases, nay, even hundreds of cases, are insufficient to establish the utility of one system of treatment as com-



pared with another ; hence the necessity of reporting accurately large numbers of cases, and grouping them so as to admit of comparison. I know how figures may mislead, and the objections that may be urged against entirely relying on the results of counting, but the fault lies with ourselves and not with the method. Figures cannot err, but the facts which they represent may be badly observed, and their results of no value. In the use of figures, then, our first care should be to secure that the facts represented by any series are all of the same kind, or our figures will only mislead." (p. 21.)

The concluding paragraphs are admirable, both in feeling and expression :

"I would not say, gentlemen, that it is a matter of indifference who you have for your teachers ; but this I am prepared to say, that it is a matter of very secondary influence on your career, compared with the control you possess over your own characters. You have entered on an arduous undertaking, and you must never forget, that in order to be successful, you must make up your minds to work. All enjoyment and relaxation will be doubly relished when earned by due attention to study. Be not disheartened by difficulties ; what others have surmounted before you, be assured that you can surmount. Persevere ; no step in life is more difficult than the next, and each one is bringing you to your goal. Some will go faster than you ; but compare not yourself with others in your weakest points ; look at your strongest. Each man has qualities of his own which recommend him in society ; these are your gifts to use for your own and others' advantage. Find these out, and be thankful that you have so many. Above all, avoid idleness. Take formal leave of leisure for the rest of your life. Always have some object, some purpose, in spending your time. Even your hours of relaxation from severe studies may be made to contribute to your stores of knowledge, to the refinement of your taste, and the elevation of your heart and mind. Avoid the smoking-room and the gaming-house. Yield not to the first temptations to vice. Maintain the purity of your youth, and let not the pleasure of ministering to the need of the afflicted be marred by the thought that you have ever been its cause.

"We are often told, gentlemen, that ours is a noble profession ; but, if men did but know it, every human occupation may be ennobled, and there is none so exalted that may not be made mean and degraded. It depends on yourselves as to whether the profession you have chosen be noble or mean. Unless you possess the noble heart and noble mind, the exercise of the art of medicine will be to you as complete a piece of drudgery as the blacking shoes or sweeping streets. In order to make the exercise of your profession noble, your aims must be high, and your motives pure. I cannot suppose you have chosen it because it is the road to wealth or honour, for it is not. Few, indeed, are the members of our profession who acquire wealth ; and fewer still are the positions of honour awarded to us by society or by the state. If, then, you have been led to join our ranks upon the supposition that wealth and worldly distinction will attend your path, the sooner you give up your pursuit the less of pain and disappointment you will have. But if you have embraced the medical profession in the cheerful faith, that by 'the sweat of your brow you shall earn your bread,' and that, satisfied with this as your physical lot, you will seek your reward in the exercise it affords to your intellectual and moral faculties, then you will not be disappointed. Cast away from you all anxiety about worldly success as a thing that must always follow all earnest labour, and fix your mind upon the great work you have before you. You are called to the divine task of alleviating the sufferings of your fellow-creatures ; let the thought constantly inspire you in your studies, that the more you know the better fitted will you be for this great mission. Early learn to seek, in your own consciousness of rectitude, that satisfaction which you will in vain look for without. It is in the serene sense of having done their duty to God and their fellowman, that martyrs and heroes have found a happiness that the smile and applause of the world could never afford. Do not imagine that I am addressing you in the language of hyperbole ;

you will be constantly called upon to exercise your profession where only the spirit of the hero and the martyr can sustain you. The dangers you will have to meet amidst pestilence, are greater than those of the soldier in the battle-field, and you will have to meet them alone. Should you fall, as the members of our profession so constantly do, whilst fighting with deadly disease, no funereal honours will attend you, no public provision awaits your family; but in the midst of all this, the feeling that you are suffering for the good of others, will make you partakers of the same joy that He felt who died for the world; and yours will be the honour to be co-workers with that glorious host of men, by whose toils, and labours, and self-denials, the world has been ever blessed." (pp. 23-4.)

III. The chief objects of Mr. Ward's address will be gathered from the following extracts from his exordium. He supposes a youth, just awaking to a sense of the responsibilities of life, and pondering upon the choice of a profession.

"The result of his meditations, however they may deviate from the path of candid inquiry and careful argument, however they may be intruded on by the fanciful visions of unearned happiness and prosperity, should be the practical conclusion, the irresistible conviction, that his position in life, as a useful and responsible agent, must depend on the correct and assiduous application of his intellectual endowments. Now the requirements of society at large, in all their varied and complicated relations, call as much for the exercise of these capabilities, as the wishes of the individual should prompt him to employ them. These demands are so numerous and diversified in character, that ample scope is given for the application of talent the most varied, and it becomes merely a matter of decision on his part into what channel he should direct his energies. The selection should mainly depend on the nature and extent of his preliminary knowledge, the power and inclination of his mind, his capability for sustained exertion, and the opportunity which the circumstances in which he is placed afford for the further advantageous development of them all. The different steps in this important inquiry should be conducted with the utmost care and discrimination, the most rigid self-examination should be instituted, and the opinions of those calculated to bring about or to interfere with a correct estimate should be carefully canvassed." (pp. 5-6.)

But the advice of friends, however well meant, is only really valuable on such a point, as Mr. Ward justly remarks, when the individual is "firmly convinced that such advice is consonant with his own conclusions, provided they have been worked out according to the rules of sound reason and common sense." It is, we feel assured, one of the evils attending the too early choice of a profession, that many are induced to enter upon the course of preparation for a special object in life, before their general training has advanced sufficiently far, to develop their minds to that point at which the individual is capable of forming a judgment for himself; and thus that mutual harmony between the mind of the individual and the pursuits wherein he is engaged, which is essential to his own happiness and success, and to the best use of the endowments which the Creator has given him, is too frequently turned into a discord that makes the whole future course one of unhappy and profitless labour. To succeed in medicine, or in any other profession, a man must *love* it, and *feel his aptitude* for it. Nothing is so paralysing to mental energy, as aversion and self-distrust. As Mr. Ward justly remarks:

"Were determinations based on such considerations as these more frequently arrived at, the world at large would materially benefit, by the proper adjustment and exercise of the human intellect, and numbers who now wander heedlessly about, haunted as it were, by the spirit of their prostituted powers, would have found a

congenial soil for the cultivation of their abilities, and taken their active part in the onward movement of civilization, instead of acting as a dead weight to its progress." (p. 8.)

And after an admirable quotation from Sydney Smith's 'Sketches of Moral Philosophy,' on the real worth of knowledge, and the value of the discipline requisite for its attainment, he continues :

"It is by the continued spirit, if not by the letter of such discipline as this, that we trust you, gentlemen, who are about to undertake the responsible duties involved in a qualification for the medical profession, will be actuated ;—that you will be urged forward in your onward course by the permanent impulse of industry and enthusiasm, dedicating to the attainment of your object an amount of energy and rational devotion, commensurate with its importance and extent. It is not a mere worldly speculation in which you are about to be engaged, and from which you may expect to derive the maximum of proceeds from the minimum of labour. It is, on the contrary, an undertaking of deep and earnest interest ; not only fully capable of gaining for you a respectable position in life, but of exalting the mind, and enlarging the boundaries of intellectual enjoyment. To pursue it with a due sense of its grandeur and nobility may well be looked on as an ennobling occupation ; but to come to its investigation with a wayward feeling of flippancy, is virtually to cast an insult on nature and her Author, to trifle with reason, and to tamper with the best and purest feelings of the heart." (pp. 11-12.)

The remainder of the lecture is marked by the same earnest and elevated spirit ; and we cannot but believe that the influence of such addresses as those to which we have thus briefly referred, will not pass away from the minds of all who may have heard or perused them, but will take root in some minds at least, and will fructify in due season. Many are the instances we have known, in which wholesome advice, that seemed for a time utterly disregarded, has exerted a most beneficial influence at a remote period, when bitter experience has shown how truthful and judicious were the kindly counsels that were at first neglected or even scorned. It is refreshing to perceive, in so many of the oral teachers of our time, an elevation of thought and feeling, that will tend, we trust, to emancipate the rising generation from the thralldom of that petty *trading* spirit, which is the greatest bane of our profession at the present time, and which is fostered, for their own selfish purposes, by the writings of some of those whose special duty it is to elevate instead of to degrade the character of our body, and to raise instead of lowering it in public esteem.

IV. The "Address" of Dr. Bushnan, which appeared as the "leading article" of the 'Medical Times,' just before the commencement of the session, is more strictly practical in its character ; being a judicious and well-timed exhortation to medical students, on the mode of pursuing their studies, the relaxations in which they indulge, the company they keep, &c. &c. ; proceeding from one who intimately knows their habits and feelings, and whose freedom of style and earnestness of purpose are well calculated to arrest their attention. There is nothing in it, however, that calls for more special notice.

V. The other two Lectures, whose titles we have placed at the head of this notice, although not delivered on occasions similar to those which called forth the preceding, yet deserve special mention, alike for their own merits, and as the manifestations of that sympathy which some of the

most distinguished members of our profession are ever ready to show, in all efforts for the elevation of the intellectual and moral character of their fellow-men. The Scientific and Literary Institution of Bristol, established some thirty years ago, has shared the fate of many similar establishments, in the gradual diminution of public support, and of its pecuniary resources, consequent upon the progressive removal by death, or change of residence of nearly all the influential individuals to whom it owed its origin, and the absence of that novelty which with many is the greatest incentive to the promotion of the objects to which it is devoted; and this, notwithstanding that its Museum contains an assemblage of specimens, which, for scientific value, can probably not be equalled in this country, save in collections supported out of the public purse. An effort has been recently made, however, for its revival; and to Dr. Symonds was assigned the honorable post of introducing the series of Lectures which is now in course of delivery. "Our Institution and its Studies," was the theme of his discourse; and although the notice of its history, and of those among its most-valued supporters who have passed from this scene of activity, which constitutes the first portion of it, must be chiefly interesting to those to whose local associations it appeals, yet we would hold up even this portion as a model for imitation, combining, as it does, in no ordinary degree, vigour of thought, and cultivated elegance of expression, with admirable tact in the selection and exposition of his materials. From the second part, which consists of a masterly sketch of the studies which such an Institution should foster and promote, we select a passage, as a specimen of Dr. Symonds's style, in the perusal of which we are sure that our readers will not think their time mis-spent. Our quotation is connected with the inquiry into the specific unity or diversity of the human races; as to which Dr. Symonds expresses his conviction,—which he considers that all investigations into the physical characters of races, all consideration of languages, all analogies from the dispersion of plants and animals, tend to support, "that all the nations of the Earth are of one blood; and that in the lowest, weakest, ugliest, and most stupid and besotted race yet discovered, there are the elements, however undeveloped, of the greatest and wisest, and bravest and fairest." He then continues:

"We cannot doubt that this great apparent inequality in the attributes and endowments of mankind, in different times and countries, is a necessary result of that arrangement of Providence, whereby man has been so constituted as to flourish in all climates, to struggle with all outward difficulties, and to obtain universal dominion. Other living beings have their peculiar haunts and habitats. Man's home and dwelling place is the whole earth. Wheresoever he roams, he carries with him his power of self-accommodation, and his faculty of subduing all things to his purposes. But for the variety of elements, both physical and moral, in his composition, capable of being educed and developed indefinitely, in correspondence with the diversity of the external conditions of existence, his range would have been limited indeed. Moreover, there would not have been those extraordinary differences in the characteristics of successive ages which are shown by history. Man's development would have been uniform, unvaried, and far less interesting than it has been. But it is plain that the human destinies were not to be brought out in one, nor in many generations. It has been the plan of Providence, so far as our finite powers can follow it, that civilization should come in instalments; and that different ages and different nations should contribute their respective amounts. All history points to the gradual and variable unfolding of the elements of human nature in

particular nations, and as truly under the direction of Providence for ultimate good, as any other of the perplexed phenomena submitted to mortal survey. We, of the latest birth of Time, are experiencing some taste of this good. For us, the Hellenic nations produced the finest embodiments of the beautiful in art, and the purest models of literature. For us, the Romans laboured and struggled in gaining and improving and preserving dominion. For us, the Crusaders learned the benefit of mingling with other nations. For us, our heroic ancestors, on many a hard-fought field, and in many a lonesome dungeon, and on many a bloody scaffold, taught the great lessons of civil and religious liberty. And above all, and apart from all else, for us and for all mankind, a particular people was for a time kept separate, for the intensest development of the religious sense,—for the profoundest education in religious truths—and inspired to be the teachers, through their records, of religious hopes and duties, to the farthest end of time; and still more, to be the earthly source of the transcendantly most momentous event in history,—the sublimest and most pathetic manifestation of Divine goodness.

“We are the fortunate heirs of Time; and having so rich an inheritance of wisdom and knowledge, we are bound to hand it down to our successors, not only unimpaired, but also enlarged and improved.” (pp. 35-7.)

One word we would say in conclusion.—Why does not Dr. Symonds allow the Profession to derive some benefit from his rare combination of high literary attainments, with practical sagacity and ample experience? He has shown, on various occasions, what he could do, if so minded. And we would that we could lead him to reflect, that he, too, as one of “the fortunate heirs of Time,” who has shared largely in the inheritance of wisdom bequeathed by our ancestors, and attained, at a comparatively early period of life, a professional position which falls to the lot of few, owes to it, to his brethren, and to his successors, to impart to them of the store which he has accumulated, and not to allow his acquirements to share the fate of those which are limited to the individual, instead of being diffused amongst the profession at large, elevating the standard of medical literature, and exerting their beneficial influence upon successive generations.

VI. The request made to Dr. Forbes that he would deliver the Inaugural Lecture at a literary and scientific Institution, which has recently been opened in the city in which he long resided, has afforded him an opportunity of bringing forward his views on a topic, which seems to have been much before his mind even from a very early period, having been the subject of his graduation Thesis at Edinburgh, in 1817, the title of which was “*Tentamen Inaugurale de Mentis Exercitatione et Felicitate exinde derivanda.*” The mode in which he has now treated it may be gathered from the following enumeration of the principal points adverted to. His purpose being to inquire how such a society as that which he addresses, whose object is to diffuse a taste for scientific and literary pursuits, and to provide materials for the gratification of this taste, is calculated to minister to the comfort and enjoyment, in other words, to the *happiness* of its members, he begins by considering what constitutes Happiness; and excluding, as topics unsuited to the occasion, those aspects of the question which concern man’s social and political condition, and his relations to another state of existence, he looks at man chiefly in his intellectual capacity, and lays it down as his fundamental proposition, that the most constant, copious, and universal source of happiness, of which Man,



regarded simply as an individual, is susceptible, is the due exercise of those faculties with which he has been endowed by his Creator. A brief review of the faculties, bodily and mental, succeeds ; in which the analogy of the former is brought to bear upon the latter, and it is shown that the health of the mind cannot be maintained, any more than that of the body, without *exercise* ; and Dr. F. then appeals to the experience of his hearers, whether much of the happiness which it has been their lot to enjoy, or which they have witnessed in others, has not been the result of that kind and degree of exercise of the faculties, which is usually termed *work* or occupation (in which, however, he includes all such *play*, or relaxation, as keeps the mind in activity) ; and whether the amount of such happiness has not been proportioned, *cæteris paribus*, to the activity of the exercise (under due limitation), and the number of the faculties so engaged. We doubt not that the experience of every one of our readers will afford him ample evidence of the truth of Dr. Forbes's position ; for ourselves we can only say that the most unhappy individuals of our acquaintance, are those who have no occasion to earn their own livelihood, and have not strength of resolution to direct their minds to any steady pursuit, either for their own benefit, or for that of others. As Dr. Forbes justly remarks,—

“If a man in the average condition of mental tranquillity, possessing the knowledge of the best mode of exercising his faculties, and with a conscience approving the same, has also the WILL of sufficient energy to enforce the exercise,—that man may be said to be master of his own happiness. And I believe the most prolific cause of unhappiness in the world is misapprehension or neglect of these simple principles. Many men are unhappy because they do not know that occupation would make them otherwise ; but more are so because they lack the resolution to use the remedy which they know would cure them. It is in this respect that the obligations of a trade or profession, compelling the exercise of the faculties, constitute the means of happiness to a large proportion of mankind who, deprived of such obligations, would be miserable.” (pp. 44-5.)

He warns his hearers against the mistakes as to the mode of exercise, which may prevent it from exerting its most favorable influence, or which may even lead to prolonged and confirmed unhappiness ; and concludes with pointing out the value of literature and science, as supplying wholesome materials for mental exercise. From this latter portion we shall quote a passage which is pregnant with the sound sense, philosophical spirit, and genial feeling, that manifest themselves in all Dr. Forbes's writings ; and with this we must bring to a close our notice of a discourse, which deserves to be widely read and carefully studied, and which our readers will do well to bring under the attention of such of their patients as are suffering from those very common causes of disordered health,—too little, or too much, WORK.

“I will state broadly at once, that I think it is just as much a part of man's business in this world, to exercise and gratify the faculties subservient to these pleasurable feelings, as it is to exercise and gratify those which have mainly for their office the acquisition of Knowledge. If it had not been God's will that these faculties should be used, he would not have bestowed them ; and surely we are only speaking in accordance with the voice of universal nature, when we say, that He did not make any of his creatures—least of all the sovereign creature Man—to be mere passive machines for work only, but sentient organisms, to enjoy as well as to work.

“It is manifest that there are both in the body and mind of man, faculties

whose principal office is to convey gratification to the individual; although undoubtedly in accordance with the general providence of nature they do more than this,—‘touching some spring, or verging to some goal,’ to aid in the general scheme of the whole organism. Clearly of this sort are those mental endowments which fit us to receive the varied impressions capable of exciting in us the feelings which have been usually classed under the head of Taste: as ministering to these, we may reckon the sense of the beautiful, the grand, the sublime; the emotions derived from painting, sculpture, and the fine arts generally; the feelings which are gratified by wit, humour, by the ludicrous, &c.; likewise the taste for music. Filled as this outer world of ours is with objects to gratify many of these feelings, and stored with objects to charm others as are the writings of the great men who have most dignified our race, it would be, in my mind, no less foolish and absurd—not to say ungrateful and wicked—to shut the doors of the mind against the enchantments last named, than it would be to refuse to gaze on the mountains, the ocean, or the sky—to see and smell the flowers—to taste the fruits—to listen to the songs of birds, or to eschew any of the other innocent delights which are spread so profusely and so benignantly over and around our path of life.

“Here, as in all our actions, we have Reason to teach us how we are to use, not abuse, Nature’s gifts; and so long as we follow this guide, let us rest assured that we are not acting in opposition to God’s will. When I formerly mentioned to you the injurious effects of light, desultory reading, and over-indulgence in fictitious narrative, I was speaking of an abuse; and now, when I not merely sanction but recommend the perusal of such narratives, as well as of poetry, plays, and other works of wit and fancy, I mean, of course, that such perusal is to be enforced under the proper limits and restrictions.

“Looking at the plan of nature throughout, and at the organisation of the human mind, more particularly, it appears to me as evident that the great works of Imagination, Fancy, and Wit, produced by man, were as much preordained by the framer of the mind to gratify certain of our faculties, as many of the works of his own material creation were planned for the gratification of others; and whether we go to Homer, to Shakspeare, to Milton, to Cervantes, to Moliere, Scott, or Dickens, or to “the Alps and Appenines, the Pyrenæan and the river Po,” in order that we may enjoy some of our highest and purest emotions, we are equally following the wise promptings of nature, and equally conforming to that benignant law which ever binds man’s best pleasures with his duties. The master minds of this class, according to my judgment, should be regarded by us not merely with respect and love, but with the utmost reverence; seeing that the Father of all has been pleased to constitute them—so to speak—his delegates to his other children; endowing them with “the vision and the faculty divine,” the power to create materials for the exercise of faculties for the full gratification of which He has not himself chosen to provide in his own immediate creation.” (pp. 52-6.)

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ART. II.—*Elements of Chemistry; including the Applications of the Science in the Arts*. By THOMAS GRAHAM, F.R.S., Corresponding Member of the Institute of France, Professor of Chemistry in University College, London, &c. &c. Second Edition, entirely revised and greatly enlarged. Part IV.—*London*, 1850. 8vo, pp. 88.

THIS part completes the First Volume of the new edition of Professor Graham’s admirable Treatise, which we look upon as by far the most philosophical of all the works on this subject that our country has yet produced. It is not surprising that, in the interval which has elapsed since the publication of the former edition, the accumulation of new materials should have imposed upon the author the necessity of extending his work; and we think that he has been judicious in reproducing it in

the form of two moderate-sized volumes, instead of attempting to confine it within the compass of a single one of overgrown bulk. A considerable portion of the first volume is devoted to an exposition of the general doctrines of chemistry; and it is in this portion of Professor Graham's work, that we think that its superiority consists. Our readers must be well aware that in the domain of Chemical Philosophy the author has himself been among the most successful labourers; but we nowhere find that tendency to exalt the value of his own researches, and to make *self* the standard by which everything is to be judged, which deforms the writings of some chemists we could name. On the contrary, every side is fairly and (so far as the author's plan would permit) fully represented; and his own doctrines are put forth for confirmation or rejection by future researches, with no more dogmatism than is used in the exposition of those of others.—Our readers may like to know Professor Graham's general opinion respecting the present aspect of chemical science; and we, therefore, subjoin the following extract from his preface:

"The advance recently observed in inorganic chemistry is partly in the old direction of enlarging the list of elements, partly and more conspicuously in supplying deficient members to familiar series of compounds, and in thus enlarging these series. But the most important feature in its recent progress has been the rigorous verification which numerical data of all kinds have received, whether relating to physical laws, such as the specific heat of substances, or to chemical properties and composition. The statement of properties and relations has thus acquired a fulness and precision for many substances, which contrasts strongly with the history that could be offered of the same substances even but a very few years ago. This correction and revision of every minute branch of the science was never, indeed, more general and rapid than at the present time. The enlarged means of practical instruction in chemistry, now everywhere provided for the student, and the consequent increase in the number of able investigators, have no doubt contributed much to this result. Progress of this description cannot fail to effect the theoretical views of chemists, and to promote sound conclusions by affording an extended and safe foundation for reasoning, in a body of well-established facts. It must be admitted that the fundamental views respecting the constitution of salts are at present in a state of transition; but the great questions of chemical theory, if not yet solved, have at least been correctly enunciated, and a general assent obtained to the facts on which they rest." (Preface, p. vi.)

In this volume, Inorganic Chemistry is brought to a conclusion; and we have only to express the hope that the appearance of the second volume, which will embrace Organic Chemistry, will not be so long delayed as to render the one before us already antiquated before its fellow shall have been completed.

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ART. III.—*A Manual of Dissection*. By LUTHER HOLDEN. Part III.—  
London, 1850. Post 8vo, pp. 174.

THE third part of Mr. Holden's Manual embraces the dissection of the abdomen and pelvis, with their viscera; and of the lower extremity. We must again repeat that we cannot conceive any sufficient reason for the publication of such a work piecemeal, at distant intervals; and shall reserve the expression of our opinion as to its character, until it shall have been completed.

ART. IV.—1. *Pathology of the Human Eye*. By JOHN DALRYMPLE, F.R.C.S. Part VI.

2. *Surgical Anatomy*. By JOSEPH MACLISE, F.R.C.S. Fasciculus VII.

IN the present part of Mr. Dalrymple's work, he draws attention to the diseases of the choroid coat; a class of maladies which, in their incipient stage, have scarcely received from ophthalmologists the attention they merit, but which are among the most frequent precursors of serious functional derangements and disorganising changes of structure. The first of these maladies which he describes, is simple congestion of the choroid; which he looks upon as one of the most common sources of impaired vision, especially in persons of sedentary habits, languid circulation, and lymphatic temperament, who are forced to make excessive use of their visual organs, and particularly when this happens under circumstances which tend to induce a general cachexia. It is not difficult to understand how an over-full condition of the choroidal vessels, from whatever cause proceeding, may and must produce impairment of the functions of the retina, even though that tissue itself be free from organic mischief. As Mr. Dalrymple remarks:

"The choroid coat, the most vascular of all the membranes of the eye, when in a state of plethora, produces a degree of pressure upon the expansion of the optic nerve, which excites undue irritability or blunts its sensibility; for while the external case or sclerotica is tough and unyielding, the fluids within are little compressible, and the retina, concentrically placed, suffers from the increased bulk which the dilated capillaries of the Ruyschian membrane assume. If this condition of congestion remains long unrelieved, further changes occur,—the minute vessels themselves become permanently dilated, lose their tone and elasticity; the nutrient functions are ill performed, the pigment becomes scanty and pale, and an effusion of fluid takes place between the choroidal and retinal membranes. Impaired vision is the consequence of this condition, and may be traced, in all degrees, from simple mistiness, *mucæ volitantes*, and dark fixed spots, to complete and permanent amaurosis. If the pressure be considerable, there is felt pain of a dull aching character in the globe of the eye, principally referred to the back of the organ, the lids become heavy, and weigh down, and neuralgia more or less affects the frontal division of the fifth pair of nerves. This latter phenomenon I believe to be due to pressure, similar to that which the retina undergoes, affecting the ciliary nerves so plentifully traversing the exterior of the choroid coat, on their way to supply the iris. After a time, the innervation of this latter membrane becomes disturbed, and the pupil is either wholly or partially dilated, presenting at times some irregularities not due to adhesions, but to unequal pressure upon one or more branches of the ciliary nerves."

No external change marks the character of this disease, save that the veins emerging from the interior of the globe, at the anterior part of the sclerotic, are sometimes seen to be distended and semi-varicose; whilst the openings through which they pass are stained by brown pigmentous molecules or patches. The malady must be chiefly diagnosed by the characters of the affection of vision experienced by the patient, of which Mr. Dalrymple gives full details. Occasionally he has seen, in persons labouring under general, as well as ocular, venous congestion, actual rupture of the blood-vessels occur, by which sight has been wholly effaced in a few minutes; vision being partially recovered by the subsequent gradual absorption of the effusion.

The first illustrative figure, given by Mr. Dalrymple in the *First Plate*,

represents a very marked case of the varicose veins and pigmental spots just adverted to. The second is a delineation of the external appearances presented, when the stage of congestion passes, as it is very prone to do from any exciting cause, into one of acute inflammation of the internal tunics; the sclerotic vessels are seen greatly distended, radiating from all parts of the globe towards the corneal circumference, and there forming a pink or red vascular zone, very similar to what is seen in common iritis. This disorder runs a rapid course, and terminates in extensive organic mischief and loss of vision. The appearances presented by a subacute form of the same malady, which, without a careful inquiry into the previous symptoms, might be considered as indicative of simple chronic iritis, are delineated in a third figure. The other three figures of the first plate represent the lesions which these diseases tend to produce; the pupil becomes fixed and contracted, adhesions take place between the iris and the capsule, and effusions take place in various situations; or the pupil may become completely closed, and the iris pushed forward so as almost to come in contact with the cornea, whilst it is adherent behind to a dull and opaque capsule; and, in addition to all this, the lens itself may lose its transparency.—Few writers, as Mr. Dalrymple justly remarks, have paid so much attention to, or taken so philosophical a view of, the affections of the choroid, as the late Mr. Tyrrell; but we consider that Mr. Dalrymple has done good service in thus directing attention more minutely to the subject, and in pointing out the close connection between disordered circulation in this tunic, and various derangements of the visual function.

The *Second* Plate illustrates that form of choroiditis which rapidly extends to the other tunics, and is commonly known as ophthalmitis interna. The first three figures delineate the appearances presented by the successive stages of this destructive disease; whilst the last three give an idea of the disorganising changes to which it leads. In all these, the principal seat of the effusion had been the internal surface of the choroid, so that the retina was detached from it, and pressed, with the shrunken vitreous humour, into the centre of the globe. One of these specimens was an eye, which had been removed under the idea that it was the seat of malignant disease; but, in Mr. Dalrymple's opinion, the supposed malignant growth was nothing else than a fibrinous deposit; probably of a scrofulous nature. The disorganising changes had been very extensive; the vitreous body having been wholly absorbed, and the opaque lens having been pushed against the posterior surface of the cornea, carrying with it the remains of the iris. "The still clearly traceable choroid coat lining the sclerotica, the integrity of the latter fibrous tissue, the still recognisable retina, and the fact of the non-enlargement of the globe, and the healthy state of the optic nerve," are, in Mr. Dalrymple's opinion, conclusive against the supposition of the case having been one of malignant tumour; and it ought no longer to rank, as it has done, as one of the supposed examples of the successful removal of cancerous disease of the eye. We should have imagined that the history of such a case would make the diagnosis sufficiently clear; since no such extensive disorganisation could have taken place, except by a malignant growth, without such an attack of inflammation as must have forced itself on the attention of the patient.

The *Third* Plate illustrates the various forms and phases of *Glaucoma*, which Mr. Dalrymple does not think entitled to rank as a distinct disease,



since he regards it rather as a result of inflammation of the retina, either primarily set up in this membrane, or, which is more common, extending to it from the choroid. Here, also, he is in accordance with Mr. Tyrrell, whose views he adopts and makes more complete. In the *Fourth Plate* are shown other results of choroiditis, namely hydrophthalmia, and staphyloma in its various stages and degrees; and in the commentary on this plate, we find Mr. Dalrymple's views of the treatment of this disease, which are eminently judicious. He lays great stress on the *entire disuse* of the visual organ, as soon as the symptoms indicate that congestion of the choroid has developed itself: recommends cold applications in preference to warm, as more likely to restore a healthy tone of the circulation; and indicates the kind of constitutional treatment which each class of cases will require. When an outburst of acute inflammation takes place, he considers that the chief hope of saving the eye from destruction depends upon the patient's being brought under the influence of mercury as quickly as possible.

In regard to the subjects embraced in it, and the mode in which they are treated, we consider this a peculiarly valuable fasciculus of Mr. Dalrymple's admirable work; and of the illustrations we have only to say, that they fully maintain the high character which the plates of the preceding parts have acquired, in the estimation of all who are competent to judge of their merits.

The First Plate of Mr. MacLise's "*Seventh Fasciculus*" is intended to illustrate "*The Surgical Dissection of the Principal Blood-vessels and Nerves of the Iliac and Femoral Regions*;" the great artery of the lower extremity being exposed along its entire course, from its commencement in the bifurcation of the aorta to its entrance into the popliteal space. Mr. MacLise adopts this plan of demonstration, in preference to the more usual division into "*regions*," because, as he states, "*on examining any one of those parts irrespective of the others, many facts of chief surgical importance are thereby obscured or overlooked.*" No doubt he is quite right in considering that a general knowledge of the course and relations of the vessel should be gained, before these are more particularly investigated in the several regions; but the former cannot supersede, to the operating surgeon, the necessity of the latter; and we by no means think that this one plate conveys all the information, which, on such an important department of surgical anatomy, might be reasonably looked for in a work of this kind. In particular, we would say, that the complete removal of all the parts which overlie the artery, prevents those relations to the superficial parts, which constitute the operator's most important guide, from being displayed; thus, the sartorius muscle is cut across and turned away, so that the point where it crosses over the femoral artery can only be guessed. We trust that Mr. MacLise may see reason to fill up this part of his work by a few additional figures, which might all be included in a single plate.

The Second Plate is devoted to "*The Relative Anatomy of the Male Pelvic Organs*;" and though the two figures which it contains are somewhat hard and diagrammatic, yet they serve well to illustrate the subject. The commentary on this plate contains some very judicious observations on the influence of the abdominal muscles on the movements of the pelvic

viscera. The Third and Fourth Plates contain no fewer than nine Figures illustrative of the "Surgical Dissection of the Male Perinæum, and the Lateral Operation of Lithotomy;" and it must be the student's fault if he does not gain from these figures as good a knowledge of the region as can be acquired without that actual dissection, which alone can prepare him to operate upon it. We cannot but regret that Mr. Maclise should have given to these plates so unsightly an appearance, by crowding his figures together; and think that his subscribers would have been well pleased to pay for distributing them over another plate or two. The appearance of the two latter plates, as works of art, is not enhanced, in our estimation, by their extreme blackness; for in attempting to gain depth of effect, Mr. Maclise has sometimes only reached a sort of sootiness. We should not have thought it worth while to point out these comparatively trivial blemishes, if it had not been that the general excellence of the work makes us desire to see its character well sustained.

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ART. V.—*De Colica Scortorum Disquisitio*. Autore MARTINO HASSING, Dr. Med., Medico Secundario Nosocomii Communis et Legionis Civilis Hauniensis, Regiæ Societatis Medicæ Hauniensis Locio.—*Hauniæ*, 1848.

*On the Colic of Prostitutes*. By Dr. MARTIN HASSING.—*Copenhagen*, 1848. 8vo, pp. 100.

AMONG the various writers who have treated of prostitution and the diseases specially incident to those who practise it, none, save Lippert, who has written on prostitution in Hamburg, appear to have made a special mention of colic; to which disorder, according to Dr. Hassing, the class of "unfortunate females" appears peculiarly liable. In the dissertation before us he gives a summary of observations made by himself and other physicians upon no fewer than 92 cases, that have presented themselves within nine years in the Hospital at Copenhagen, into which most prostitutes are received when ill, unless affected with syphilis. We do not see that either in its symptoms or in the mode in which it was influenced by remedies, the colica scortorum can be said to constitute even a special variety of disease; but its peculiarity, according to our author, chiefly lies in its tendency to simulate metro-peritonitis, and in its etiology. This is carefully investigated by him; and every circumstance that seems likely to exert a causative influence is duly considered. It would seem that the disorder is most frequent during the spring and summer months; the number of cases from March to August inclusive being 55, whilst in the six autumn and winter months they were only 37. In no fewer than 49 cases, the disorder showed itself in the patients already in the hospital under treatment for some other malady, and not unfrequently at an interval of some weeks after their admission; a circumstance which would seem to point to some cause specially acting upon them in that establishment. We are not informed whether *other* patients who have been some weeks in the hospital are liable to the same complaint; but we presume that this must have struck Dr. Hassing had it occurred. He inquires whether the vaginal injections frequently ordered for the prostitutes in the hospital can have given rise to the attacks of colic; and seems to consider that they may have a special influence, more particularly as there is reason to believe that they are not unfrequently used by

prostitutes for the suppression of the catamenial flow. It is well known, he remarks, that intra-uterine injections have been often found to give rise to phenomena simulating local peritonitis, metritis, or colic, but always easily subdued. Some have imagined that the liquid injected finds its way through the Fallopian tubes into the peritoneal cavity; but from this opinion he dissents (and we think quite justly), regarding the phenomena as sympathetic, and as proceeding especially from irritation of the neck of the uterus. In some of the cases related, it appeared pretty evident that the injections were the exciting cause; whilst in others the attack of colic followed immediately on the application of the speculum. On the whole it seemed to the author that the tendency to the disease was chiefly to be laid to the account of the general habits of life of the patients; their irregularities in diet, exposure to vicissitudes of temperature, and the continual excitement of the genital system, acting sympathetically upon the digestive. He endeavoured to ascertain whether sexual coitus was a frequent cause of the attacks of colic; but upon this point he could not obtain any satisfactory information. In some of the cases which he relates, the tendency disappeared as soon as the patients married and led a regular life. We have seen cases, however, very much resembling Dr. Hassing's *colica scortorum*, in married ladies of hysterical temperament.

The disorder was in no case fatal, so that its pathological anatomy cannot be positively determined; but from the examination of the bodies of patients who have died from other diseases, but have been subject to this form of colic, it does not appear that it leaves behind it any traces of peritoneal inflammation. Various methods of treatment appear to have been employed. General bleeding, which we should not have thought at all applicable in such cases, was practised in 21 instances, with the effect of giving immediate relief from the pains to only 1 patient, more or less benefit to 12, and none whatever to 8. Local bleeding by leeches was not much more successful; out of 41 cases, it caused the entire abatement of the pains in only 5, gave more or less relief in 18, and was of no benefit in 15. But, as the author justly observes, it is doubtful whether the emollient cataplasm used after the application of the leeches, had not a good deal to do with the benefit which was ascribed to them. We are surprised to find so little use made of anodyne remedies, which we should have looked upon as much more appropriate than the antiphlogistic treatment that seems to have been generally employed. In the cases to which we just now referred, warm fomentations and anodynes internally administered seemed more useful than any other remedy; but the attacks seemed to have a spontaneous tendency to a favorable termination.

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ART. VI.—*Conspectus Medicinæ Theoreticæ, Capita Quadraginta tria universa complectens, ad Usus Academicum*. Auctore JACOBO GREGORY, M.D. Editio Undecima, omni parte perfecta, prioribus emendatior.—*Edinburgi*, 1851. 8vo, pp. 433.

THIS reprint of a well-known and justly-valued medical classic, deserves commendation for its compactness and typographical elegance; and we learn from one of our northern contemporaries, that it "has been revised by an excellent scholar, who has cleared the text of a number of errors which had crept into it."

ART. VII.—1. *Report by the General Board of Health on the Supply of Water to the Metropolis.*—London, 1850. 8vo, pp. 325.

2. *Further Examination of the "Gathering Grounds" for the proposed Government Water Supply to the Metropolis, as contained in a Report to the General Board of Health, Gwydyr House, Whitehall.* By the Hon. WILLIAM NAPIER.

THERE is a general agreement as to the necessity of an adequate supply of pure water to the healthy condition of towns; and those who have been charged with the special duty of carrying into effect the provisions of the legislature, under the Public Health Act, for the improvement of the sanitary condition of towns, uniformly represent that without such a supply no other material improvement can be effected. Though water is required in towns for many purposes besides domestic use, yet the domestic use is so much more important than any other, that the quality of water which best adapts it for that object, should govern the nature of the supply. But it happens that the quality, that is softness, which renders water best adapted for domestic use, is the most advantageous for every other purpose to which it can be applied; at least where the quality is of any consequence at all. River water, which varies in hardness from  $5^{\circ}$  to  $16^{\circ}$ , and upwards, is the only source from which, until recently, the supply has been sought; and it is remarkable that every plan for the supply of the metropolis, with the single exception of the Artesian well scheme, proposes to derive it from some river; either the Thames, the Lea, the Wandle, the Colne, the Darent, the Mole, or the Medway; and the two latest schemes brought before Parliament propose to take the supply, the one from the Thames at Henley, and the other from the Thames at Maple-durham. But the two objections to river water being its hardness and impurity, the General Board of Health, in the execution of the duty confided to them, of maturing a scheme for the better supply of the two million inhabitants of London, sought a supply from springs instead of from rivers; assuming that the nearer the source, the softer and purer would be the water. Experience had shown that the softest and purest water is obtained from the immediate rainfall flowing directly off primitive rocks. On searching for a similar condition for the collection of water in the neighbourhood of London, the attention of the board was directed to the Bagshot district, the distinctive character of which is that it consists of insoluble sands resting on clay. After an extended and careful examination of this district, it has been ascertained that no less than fifty-one millions of gallons of water per day are obtainable from springs alone, rising within an area of a few miles, the water being almost as soft as distilled water; perfect as to aeration; brilliant in colour; of a grateful temperature, about  $50^{\circ}$ ; and almost free from all mineral, animal and vegetable impregnation; while the springs themselves, wholly uninfluenced by the immediate rainfall, afford a perennial flow, contributing to the formation of the river Wey, and other tributaries of the Thames; the streams, though of only from one to two degrees of hardness at their source, acquiring a progressively increasing hardness as they reach their outfall in the rivers. It is proposed to collect this pure and soft water, by laying earthenware pipes up to the very sources of these deep springs; to convey it to London by a covered conduit; to distribute it directly, without storing it and thereby

spoiling it in large open reservoirs, exposed to smoke and dirt, and to introduce it into every house in the metropolis, on the constant, as distinguished from the intermittent, system of supply. It is estimated that this can be effected without the addition of a single shilling to the present water-rate raised by the existing water-companies, and paid by the consumers for their present impure and imperfect supply. We have had no opportunity of considering the details and calculations on which this estimate is based, on the correctness of which must of course mainly depend the realization of this magnificent promise; but it is clearly the bounden duty of the government, the legislature, and the public, to give to this scheme an honest and earnest consideration.

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ART. VIII.—*A Practical Treatise on the Therapeutic Uses of Terebinthinate Medicines; with Observations on Tubercular Consumption, Gout, Mineral Waters, &c.* By THOMAS SMITH, M.D., late Physician to the Cheltenham General Hospital and Dispensary, &c. &c.—London, 1850. 8vo, pp. 95.

Dr. Smith has been led, in the course of his clinical experience, to form a higher estimate of the value of terebinthinate medicines, than he believes to be generally entertained in the profession, notwithstanding the praises which they have received from Dr. Copland and various authors of eminence; and he has now reproduced, in a collected form, a series of papers which he has already laid before the medical public through the periodical press, with the view of placing the results of his observations in the hands of his brethren. We regard them as well deserving of perusal; as they are altogether free from any of that offensive parade of the success of particular modes of practice, which in too many such cases diminishes the confidence of the wisely-judging reader, and exhibit throughout the single aim of improving the resources of the therapeutic art, by the attempt to determine the true value of a powerful class of medicaments. The observations on a somewhat miscellaneous set of subjects, which are thrown together in the Appendix, are also well worthy of attention. One of these relates to the subject of lead medicines, as to which the author expresses an apprehension that does not appear altogether ill-founded, that their administration in cachectic subjects, as in hæmoptysis dependent upon tubercular disease, may be productive of remotely injurious consequences. He dwells strongly on the admitted results of the prolonged introduction of lead in small quantities into the system; and expresses the opinion, in which we fully agree with him, that the contamination of well-water with lead is a more frequent cause of disordered health than is generally suspected. If there be any reason to fear such results, it is surely better, he argues, to place our chief reliance on the powerful astringents we possess, which are not subject to the same objections. Allowing the full force of his argument, we would still remark, that it is not from the ordinary medicinal doses of acetate of lead, repeated a few times and then discontinued, that we should expect the effects of lead poisoning to be developed. These seem to require, either a very large dose in the first instance, or the continual introduction of small doses continued over a long period, so that the nutritive operations are affected, and the lead becomes incorporated with the tissues and organs generally.



ART. IX.—*Gout ; its Causes, Cure, and Prevention, by an Original and most Successful Treatment, founded on the Organic Changes in the Human Solids, and on the Functions of the Skin ; without the Use of Colchicum.* By ABRAHAM TOULMIN, M.D. M.R.C.S.—London, 1850. 12mo, pp. 112.

THIS book is one of a class which, we fear, is considerably on the increase, to the great detriment of the character of our professional literature. It wears the outward garb of a medical treatise, designed to inform the profession of the success that has attended a plan of treating gout, which the author claims as original ; but it is really addressed to the public, and is intended to impress all who are able to afford to pay for Dr. Toulmin's treatment, with the idea of its vast superiority to all other systems, and of its almost certain efficacy. The cat is let out of the bag in the very first sentence of the Preface.

"In submitting to the *public* what, I flatter myself, will be found to be a greatly improved method of treating gout and its congeners, it is not with a view of superseding medicine, but as an auxiliary to it, and to its application in a way, which has of late years been almost, if not altogether, lost sight of."

This quotation will further serve as a sample, that will enable our readers to form their own judgment of the degree of clearness of expression and elegance of construction, which characterise the author's style. On the Pathology of Gout he has nothing new to say ; for his doctrines are all borrowed, with very insufficient acknowledgment, from Prout, Liebig, Garrod, and other original investigators ; though often presented in so distorted a form, that their real parents would scarcely recognise them. And in what do our readers suppose that Dr. Toulmin's great novelty in treatment consists ? Simply in the use of a hot-air bath, which he dignifies by the name of a *thermaclin*, whereby he conceives that he draws off the *materies morbi* through the skin ; and in the employment of a shower-bath immediately afterwards, when the patient is supposed capable of bearing the revulsion. In this there is no novelty whatever ; to our certain knowledge, the hot-air bath has long been successfully employed as a *sudatorium* by many practitioners, in gout as well as in other diseases in which free diaphoresis is desirable ; and we have known even the cold plunge-bath employed after it, which is going a considerable step beyond Dr. Toulmin. We are inclined to think, with him, that more use may be advantageously made of the skin as an excreting organ, than is the custom in ordinary practice ; and that hot dry air is usually a most effectual diaphoretic. But if Dr. Toulmin wished to inform his professional brethren of his successful employment of this method of treatment in gout, why did he not send a concise statement of it to the Medical Journals, by whose agency it would have been placed before a wide circle of professional readers ? And why did he, in preference, make a book, which, we venture to say, not half a dozen medical men, save the unfortunate critics whose duty requires them to do so, will ever read ; but which will, no doubt, be eagerly purchased by those gouty subjects who have more money than brains, and will leave them under the impression that they are certain of an easy and pleasant cure, by placing themselves in Dr. Toulmin's *thermaclinic* establishment ? These are the questions which the author has

to answer at the bar of professional honour. Glad should we be to allow such productions to pass *sub silentio*. We know that we make ourselves enemies, and do ourselves no benefit whatever, by thus noticing them. But while we continue in our present office, we are determined to pass by no such offence against the credit of medical literature; since every repetition of it is calculated to damage the profession in the eyes of the enlightened portion of the public, as well as to lower its own self-respect.

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ART. X.—1. *The Physicians', Surgeons', and General Practitioners' Visiting List, Diary, Almanac, and Book of Engagements for 1851.*—London, 1850.

2. *A Medical Almanac and Companion to the Visiting List for 1851.*—London, 1850.

IN accordance with suggestions which have been conveyed to him, Mr. Smith, in this the fifth edition of his useful work, has divided the "Visiting List" from the "Companion," and has thus been able to add to the contents of both, without rendering them liable to objection in point of size. Together or separated they are extremely compact; the "Visiting List" has been arranged in such a manner as to answer every purpose that experience has hitherto pointed out as desirable; and the "Companion" contains a large body of very useful information. When we add, that the "Visiting List" is made up in no fewer than fourteen different sizes and forms, we think that the most fastidious of our readers must be able to find one amongst them to suit him,—to his own great comfort, and possibly, also, to his patients' advantage.

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ART. XI.—*The Surgeon's Vade Mecum*. By ROBERT DRUITT, F.R.C.S. Fifth Edition, much improved. With 175 Wood-engravings.—London, 1850. Fcap. 8vo, pp. 660.

THIS little volume, published in Manual size, has so well established itself in public favour, that it almost seems superfluous for us to say a word in its commendation. We know no work of its kind, in which the author's plan has been so successfully wrought out, and none which may be so justly recommended to the student. The new matter introduced in this edition has been very aptly incorporated with the old, without materially adding to its bulk; and a large number of references have been appended, to the best and most recent sources of information.

## PART THIRD.

## Periscope.

## ANATOMY AND PHYSIOLOGY.

*On the Functions of the Liver.* By M. BERNARD.

M. BERNARD, in a recent course of lectures delivered at the "College of France," reiterated his views respecting the functions of the liver, and endeavoured to confirm them by additional experiments. Although we have already adverted to the various papers he has from time to time published on the subject, a brief recapitulation of the points he believes proved may not be uninteresting, and will be best managed by stating them in the form of propositions.

A. *On the Formation of Sugar by the Liver.*

1. In an animal that has been prevented access to saccharine and amylaceous food, the blood entering the liver contains no sugar, but that which leaves it always contains it. Dogs fed for six weeks exclusively on meat present large quantities.

2. It is not only the blood of the organ, but its tissue also, that contains it in abundance.

3. It is found in the livers of all the domestic animals, in birds, fish, reptiles, and even oysters and snails. Towards the fifth month it is found in the foetal liver, and continues increasing; and it exists even in the embryos of oviparous animals.

4. The quantity calculated from actual measurement to be contained in the liver of a healthy adult, who was guillotined while fasting, was 23 grammes 267 milligrammes. In a diabetic subject, the liver contained 57 grammes. In the liver of an ox the total quantity was calculated at 243 gr.

5. As a general rule, there is most sugar in the liver of those animals which consume aliments containing sugar. The longer the abstinence the less the quantity.

6. There is more found in adult animals than in young.

7. Although repeated experiments constantly confirm M. Bernard's original assertion that irritation of the olivary bodies of the medulla oblongata induces an almost immediate increase in the quantity of sugar, yet the supposition that it did so by irritating the origins of the eighth pair of nerves is erroneous; for if these nerves be divided, the increased secretion still goes on.

8. Whether the left or right olivary body, or the interval between them, be pierced, the quantity of sugar is alike increased in the urine; but in the first two cases the animal turns continually to the left or the right, and in the last progression occurs in a straight line. The quantity produced, within certain limits, is increased with the size of the instrument used. In rabbits the sugar-secretion continues for forty-eight hours after, and in dogs for four or even seven days. The same result follows, whether the animal is fasting or not.

9. During this increased secretion, the animal is in constant agitation; its respiration is accelerated, but its temperature is diminished some degrees. The quantity of urine is increased, besides becoming saccharine.

10. The sugar once produced, whether by the liver or by means of aliment, undergoes destruction in the lungs; but its disappearance is not, like its production,

under nervous influence, but is a chemical phenomenon which may take place in contact with air externally to the lungs. The destruction in the lungs gives rise to the production of carbonic acid, which is liberated from the air passages. In animals whose olivary processes are pierced, this is given out in larger quantities, and their blood becomes blacker.

11. This diabetic sugar is especially distinguished by the large quantities which can be destroyed by the lungs, being as 15 to 2½ as compared with grape sugar. Cane sugar introduced into the blood does not disappear by the lungs, but escapes by the urine.

12. Various circumstances may impede or prevent the secretion of sugar by the liver, as severe pain or lesion of the nervous system (except of the olivary bodies). Diseases may produce the same effect. Thus sugar has been found to cease being secreted in diabetes during the paroxysm of ague, in pneumonia, &c.

13. Additional experiments have shown M. Bernard the error of the hypothesis he advanced, that glycosuria was due to an affection of the pneumogastric nerves; and he is now disposed to regard it as due to a special although unknown alteration of the liver itself, which is indeed generally hypertrophied in this disease.

#### 2. Formation of Fatty Matters by the Liver.

1. In spite of the great variety of fatty substances, animal and vegetable, the animal that consumes them always produces the same description of fat, owing to the elaboration they have undergone in the economy.

2. To become absorbed, fatty matter must previously have become emulsified, and the pancreatic juice is the fluid by which this is accomplished. Its power of effecting this depends upon an organic matter analogous to ferments.

3. The amount of fatty matter introduced as food is far from explaining the quantity possessed or produced by the individual. There is no fat in the blood which enters the liver, but there is abundance in the blood that leaves it, and therefore its formation within this organ is certain.

4. It is, as in the case of sugar, during digestion that fat is produced in the liver, and after abstinence it disappears. Sometimes it is very abundant, especially in suckling women; whence probably arise the fatty matters of the milk, for the fat of the liver offers most analogy to butter. In such women, the blood itself contains much fat.

5. The fat received in the food after decomposition by the pancreatic juice, and the fat from the liver, both enter the blood, and are not entirely destroyed in the lungs, inasmuch as the arterial blood still contains abundance. As the venous blood contains hardly any, and the vena cava none, we must conclude that the greater part is destroyed in the general capillary system.

6. The production of fat in the liver seems, like that of the production of sugar, to be under the influence of the nervous system; and if this undergoes violent lesion or perturbation, its production ceases. It is remarkable, too, that in proportion as the quantity of sugar increases after puncture of the medulla oblongata, that of the fat diminishes. The same is observed in diabetes; for from the livers which are loaded with sugar, not an atom of fat can be extracted.

7. Although healthy urine has been shown to contain some fat, yet this sometimes constitutes a disease under the name of *fatty or chylous urine*, with a similar state of the blood. This is probably due to fat secreted by the liver, and constitutes a (so to say) *fatty excess* of sugar does a saccharine one. [digestion may thus be said to induce three principal diseases; the fat, the so called chylous urine; and albumen, the albu-

#### the Liver.

ing the liver contains little fibrin, and coagulates with difficulty [is fed on flesh. The fibrin of the aliments is dissolved by [is converted into a matter analogous to albumen, termed by But the blood which quits the liver contains much fibrin, and

therefore the albuminose of the blood of the abdominal veins has become transformed into fibrinous matter.

2. It is during digestion that the blood, traversing the liver, becomes loaded with this abundance of fibrin.

D. *Secretion of Bile.*

1. The secretion of bile differs from those already named, as it is continuous, while they occur only during digestion.

2. The bile is not a mere excrementitious fluid, but influences digestion usefully, contributing with the gastric and pancreatic juices to constitute that most powerful solvent—the intestinal liquid.

3. The bile seems to be essentially endowed with anti-putrefactive properties. It regulates the chemical reactions occurring during digestion, prevents fermentation, and opposes the formation of the gases which result from the decomposition of azotised and non-azotised aliments.

4. When bile is prevented reaching the intestine, putrefactive fermentation, no longer opposed by the acids of this fluid, induces diarrhoea; which may also be induced by a predominance of alkali in the intestines, and may be under such circumstances advantageously treated by acids.

D. *Résumé.* From what has preceded, it is evident that *sugar, fat, and fibrin* are fabricated in the liver, and that whatever the alimentation may be, this organ transforms it into appropriate matter of nutrition; so that the great variety of food taken does not derange the composition of the blood and prevent its being identical. In a chemical point of view the organ is then of great consequence, and is properly regarded as one of *sanguification*. While in the herbivora, whose aliments furnish much sugar, this secretory organ furnishes least sugar, in the carnivora, who ingest much fatty matter, it secretes least fat. So, too, the liver forms fibrin abundantly, in proportion as the vena portæ contains less. The liver, therefore, while it is an organ of sanguification, is also one for *adjusting the equilibrium*.

If the above products furnished by the liver serve to keep the circulating fluid in a composition essentially fit for nutrition, the *bile*, acting in opposite direction, contributes to the same end by removing the principles that are in excess, especially carbon; so that the liver must be considered not only as an organ of sanguification and equilibrium, but also one exerting a *depurative action on the blood*.

E. *Circulation of the Liver.*

1. Two powerful causes—the pressure of the abdominal viscera and the venous aspiration—contribute to this; but other contrivances are required to meet the varying degrees of plenitude to which the organ is liable.

2. Certain vessels, seen easiest in the horse, communicate directly between the vena portæ and vena cava, conveying a portion of the blood without its having undergone modification in the capillary circulation of the liver, and operating as a kind of *diverticulum*, preventing the organ from being too greatly and too suddenly engorged.

3. Congestion of the liver and heart, in the case of unusual afflux of fluids, is further provided against by the agency of a special *hepato-renal circulation*, not so distinct in man, in whom the liver is liable to become congested by over-exertion, as in the horse, in whose liver this does not produce the same effect. [We have already (No. X, p. 246), described the apparatus by which this is supposed to be carried on, and expressed our conviction of the fancifulness of the suggestion.]

4. The active and abundant circulation through the liver is an important source of *animal heat*, the blood leaving the organ having acquired an additional temperature of about 2° Fahr.—*L'Union Médicale*, 1850, Nos. 82, 85, 88, 91, 98, 103, 106, 113, 115.

[We think that there is quite enough of probability in some of M. Bernard's assertions, to encourage further inquiry; but we must enter our protest against the hasty and dogmatic manner in which he builds up his conclusions on a very slender basis of evidence. It will be seen, even from his own admissions, that he



has done this in regard to the influence which he supposed to be exerted through the eighth pair of nerves on the production of sugar; and the recent researches of Frerichs and Lehmann on the Chemistry of Digestion (of which we shall give an account in our next Number), have shown him to have been no less hasty in his deductions on the agency of the pancreatic fluid.]

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*On the Shape of the Thyroid Foramen of the Os Innominatum in Male and Female.*  
By Dr. NEILL.

Dr. Neill believes great errors prevail as to the shape of this foramen in the two sexes. He finds that many anatomists and obstetrical writers are at issue on the point, or are altogether silent upon it. He has examined 32 skeletons, with a result so contrary to the prevailing statements on the subject, that he has had a chart prepared exhibiting diagrams of the male pelves in one column, and of the female ones in another,—and from an inspection of this, it shows that the foramen in the *male* is *oval*, in the *female*, *triangular*. “The male foramen, too, is longer and narrower, the line representing its long axis being more vertical, and nearly parallel to the rami of the pubes and ischium; whereas in the female the foramen is not only smaller and triangular, but the apex of the triangle is downwards, its internal side nearly parallel to the rami of the ischium and pubes, and the base of the triangle is proportional to the chord of the arch of the pubes.”

The establishment of the fact is only valuable as affording an additional mark of distinction between the male and female skeletons.—*Am. Journ. Med. Sc.*, N. S., No. xl, p. 558.

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*Electro-Physiological Researches*. Seventh Series. By Signor C. MATTEUCCI.

In this memoir, Professor Matteucci, after recapitulating the results of his previous researches on Electro-physiology, published in the ‘Philosophical Transactions,’ proceeds to the relation of new experiments. He first shows that nervous filaments made to conduct an electric current in a liquid are not capable, like metallic wires, of acting as electroids, and giving rise to electro-chemical decomposition. The solution employed was that of iodide of potassium; the nerves, two large ones, taken from a living animal, each of which was separately attached to the metallic extremities of a pile of fifteen couples. No trace of decomposition followed; and he concludes from hence, that the conductivity of nervous matter is due to the liquid part of the matter itself.

He then gives further experiments on the relative conductivity of muscles and nerves, with a view to ascertain whether, when a current was impelled through a mass of muscle, any part of the current might have passed through the nervous filaments spread through that muscle. For this purpose he inserted the nerve of a galvanoscopic frog into a hole made in a piece of dead muscle, through which he then passed a very powerful current: no contraction followed in the galvanoscopic frog. When muscles still retaining their irritability were substituted for the dead muscle, induced contractions occurred in the galvanoscopic frog during the passage of the current. He concludes that when the poles of a pile of twenty-five or thirty elements are applied to the surface of the muscles of a living animal, the phenomena produced by the passage of the current must depend either on the *direct* action of the current on the muscular fibre, or on the *indirect* action or *influence* of the electric current transmitted by the muscular fibre to its own nervous filaments, or rather to the nervous force existing in those filaments.

Referring then to an experiment related in a preceding paper, in which the lower limbs of a frog, united to the spine only by the lumbar nerves, are placed astride two glasses containing water, with each foot immersed, and in which a current, after traversing the two limbs, and consequently the two nerves, in opposite directions, so modifies at length the excitability of the nerves, that, on opening the

circuit, only the limb in which the current has been passing inversely contracts, he shows that if in this state what may be called the "inverse" nerve be touched by a piece of muscle, although the circuit is continued, yet the limb contracts as though the circuit had been broken. In fact, the muscle, by its greater conductivity, becomes traversed by the current in place of the nerve. Again, if after the former part of the experiment has been performed, the portions of nerve which had hitherto been buried among the crural muscles be dissected out, it is easily seen that their excitability has not been affected like that of the lumbar nerves, because the current, in place of traversing them, has traversed only the crural muscles. The nerve has had its excitability modified in only that part of its course in which, being laid bare and isolated, it has necessarily conducted the current.

M. Du Bois Reymond ('Comptes Rendus') has related an experiment seeming to lead to the inference that section of the spinal marrow increases the excitability of the lumbar nerves, at least during a certain period of time. In order to test the accuracy of this conclusion on so important a point, M. Matteucci institutes a number of very accurate experiments, in which he measures the excitability of the lumbar nerves after section of the spinal marrow, by means of the apparatus of Breguet, used and described by him in a former paper. His first results show that "the contraction excited in the muscles of a frog, of which the spinal marrow has been divided from twelve to eighteen hours, is *stronger* than that obtained under the same circumstances from the muscles of a frog just killed, without having been previously subjected to any injury to its nervous system." But subsequent experiments have satisfied him that this result depends not on the separation from the spinal marrow, but rather on the repose in which the muscle has been permitted to remain; for without division of the medulla, nearly the same force of contraction existed after the same interval of time. He finds indeed that the only alteration which the excitability of a nerve undergoes by separation from the nervous centres, consists in its being more readily exhausted under the action of stimulants, the longer the period that has elapsed since its detachment.

The author then proceeds to relate the nature of the strict analogy existing between electricity and nervous force. As electricity is developed under the influence of the nervous current in the organs of electrical fishes, so, as a converse of this phenomenon, electricity may develop the nervous force. After adverting to the well-known analogy subsisting in every particular between the phenomena of the electrical organ and those of muscles, he adverts to the old experiment of passing a current through the muscles of the thighs of a living animal, the positive pole being placed now above, now below, so that it may be supposed that the current passes in the two cases in opposite directions as regards the nervous filaments distributed in the muscles. He then points out that the effects of a current directed downwards, in the direct course of the nerves, are a strong contraction of the muscle traversed, and also of the *muscles of the leg below*; while the effect of a current in the opposite or inverse direction is *pain*, together with contractions less violent and always confined to the muscles traversed. The *contractions* (especially of the parts below) indicate a current of nervous force propagated towards the muscles, while the *pain* indicates a current towards the nervous centre. Now, bearing in mind that it has been proved by direct experiments that an electric current traversing a muscle never quits the muscular fibre to enter the nervous filaments, it seems clear that the phenomena just spoken of are exclusively owing to the *influence* exerted by the electricity passing through the muscles on the nervous force contained in the nerves; and also that this nervous force acts peripherad or centrad, according to the direction of the electric current which excites it. The great importance of the conclusions drawn from these experiments consists in this, that they lead to the same law which establishes the analogy between nervous force and the electrical discharge of fishes. The paper concludes with some further considerations intended to confirm this law.—*Philosophical Transactions*, 1850.

*Observations on the Freezing of the Albumen of Eggs.* By JAMES PAGET, Esq.

THE object of this paper is to illustrate a peculiar property of the albumen of the eggs of birds, a property which seems to have its purpose in preserving them from the injurious effects of very low temperatures.

Mr. Hunter observed that a fresh egg will resist freezing longer than one which has been previously frozen and thawed; and he referred this fact to the "vital power" of the egg in the first case, and the destruction of that power by freezing in the second. The author's experiments confirm those of Mr. Hunter, and prove, also, that when fresh eggs are exposed to very low temperatures, and also in the case of eggs which are decayed, or putrid, or the contents of which have been much altered by mechanical force or by electricity, a shorter time is sufficient for the freezing of such eggs, than is necessary for the freezing of those which are uninjured.

An examination of the rates at which heat was lost by the several eggs, exposed to temperatures varying from zero to 10° Fahr., showed that fresh eggs, though they resist freezing longer than any others, yet lose heat more quickly; and that their resistance to freezing is due to the peculiar property of their albumen, the temperature of which may be reduced to 16° Fahr., or much lower, without freezing, although its proper freezing point is at or just below 32°. Other than fresh eggs lose heat comparatively slowly, but freeze as soon as their temperature is reduced to 32°; fresh eggs lose heat more quickly, but may be reduced to 16° or lower; then, at the instant of beginning to freeze, their temperature rises to 32°.

That this peculiarity of fresh eggs is not due to vital properties, is proved by experiments which show that certain injuries, such as mechanical violence, addition of water, and others, which spoil their powers of resisting freezing, do not prevent eggs from being developed in incubation. By the same and other experiments, which are related, it is made probable that the peculiarity depends on the mechanical properties of the albumen; for whatever makes the albumen more liquid than it is naturally in the fresh egg, destroys the power of resisting freezing.

The author could find no other substance possessing this property; and in evidence of its adaptation to the purpose of preserving eggs from the loss of their capacity of development, which they would suffer in being frozen, he relates experiments in which eggs were kept for a considerable time at temperatures ranging from zero to 10° Fahr., yet were afterwards developed in incubation. By the same series of experiments it was shown, that although freezing renders the effectual development of the germ impossible, yet the intensest cold, if freezing does not take place, has no similar result.—*Philosophical Transactions*, 1850.

*On the Structure and Use of the Ligamentum rotundum Uteri, with some observations upon the change which takes place in the Structure of the Uterus during Utero-gestation.* By G. RAINEY, Esq., M.B.C.S.E.

THE author first refers to the discovery of the difference which exists between the two classes of muscles; the voluntary, or those with striped fibres, and the involuntary, or those with unstriped fibres. He then notices that the opinion which is entertained respecting the round ligaments being composed of the unstriped variety of muscular fibre is incorrect, these organs consisting chiefly of the striped muscular fibre.

In support of the accuracy of this assertion, the author alleges the following facts:

First, that the round ligament arises by three tendinous and fleshy fasciculi; one, from the tendon of the internal oblique, near the symphysis pubis, a middle one from the superior column of the external abdominal ring, the third from the inferior column of the same: from these points the fibres pass backwards and outwards, and uniting form a rounded cord—the round ligament; after which, traversing the broad ligament, they go to be inserted into the angle of the uterus.

The striped fibres are principally situated in its centre, and extend from its origin

to within an inch or two of the fundus uteri; as they approach which, the fibres gradually lose the striated character and degenerate into fasciculi of granular fibres of the same kind as those of the Dartos muscle; both these fibres presenting similar microscopic characters when acted upon by glycerine.

The author then states, that the round ligament does not pass through the external ring to be lost in the labia and mons veneris; and argues from the fact of their consisting mainly of striped fibres, &c., that their use cannot be merely mechanical or subservient to the process of utero-gestation, and therefore he concludes that its function must be connected in some way with the process of copulation.

He also adverts to the necessity of examining the round ligament by the microscope in glycerine in preference to any other fluid; as this substance renders the cellular tissue mixed with the fibres more transparent without diminishing the distinctness of their characteristic markings. The author next states his views on the changes which take place in the uterus during utero-gestation, and observes, first, that there is no similarity between the fibres of the round ligament and those of the unimpregnated uterus, the latter being made up of spindle-shaped nucleated fibres, contained in a matrix of exceedingly coherent granular matter; that these fibres are best examined in portions which have been broken up by needles, in preference to thin sections; and that this tissue is well seen in the larger mammals, as in the cow, &c. In the impregnated uterus the fibres are found much increased in size and distinctness, but devoid of nuclei and comparatively loosely connected; and the enlargement of these fibres is of itself sufficient to account for the increased volume of the gravid uterus, without supposing that a set of muscular fibres are formed in it *de novo*.

Hence he reasons that the unimpregnated uterus consists probably of little more than an assemblage of embryonic nucleated fibres, inactive until the ovum is received into it, after which their development commences and continues simultaneously and progressively with that of the foetus; so that when this last has arrived at a state requiring to be expelled, the uterus has acquired its greatest expulsive power. Lastly, the author observes, since the fully-developed fibres cannot return to their former embryonic condition, they necessarily become absorbed, and a new set of embryonic fibres are formed for the next ovum, so that each foetus is furnished with its own set of expulsive fibres; which view is in perfect accordance with the statements of Drs. Sharpey and Weber, with regard to the membrana decidua.—*Philosophical Transactions*, 1850.

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## PATHOLOGY AND PRACTICE OF MEDICINE.

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### *On Elastic Fibres found in the Sputa of Phthisis.*

By Professor SCHROEDER VAN DER KOLK.

THE learned Utrecht professor, so well known for his researches into the structure of the lungs, declares that the microscope offers an infallible means of detecting the existence of cavities, by exhibiting in the sputa the presence of the *elastic fibres* which surround the cells of the lungs; and this the more certainly as the cavity is in an early stage of formation, consequently at the very period when such a sign, if to be depended upon, is most wanted. They can be examined under a magnifying power of 200. They are of an arched form, very thin, with sharp borders, and are sometimes covered with fat, which is removable by ether. They must not be mistaken for a species of *conserva*; which very rapidly appears in the expectoration, especially when this contains fat, but which is recognisable by its ramifications terminating in tumefied cells.—*Rev. Med.-Chir.*, vol. viii, p. 226.

*On the Treatment of Itch.* By MM. BAZIN and BOURGUIGNON.

M. Bazin, physician to the St. Louis, in a recent report furnishes an account of the trials he has made of the various means of treating the itch, and of the definitive results he has arrived at. He states that at the time of his appointment in 1847, the medium time occupied in treating the disease by the *sulphuro-alkaline ointment* applied to the wrists and insteps, together with sulphureous baths and fumigations, was fourteen days; but that since he has caused the *entire body* to be well rubbed with it (rubbing with extra force those parts the acari specially infest), the patients are dismissed cured in two or three days. Some practitioners who have adopted the plan have erred in its application by leaving some portions of the body unrubbed, or by continuing the friction as long as any itching was perceived. In the first case the disease re-appears; while in the other, by prolonging the frictions beyond the time necessary to destroy the acari, and the vitality of their ova, other eruptions are induced which give rise to great itching. This itching and eruption, occurring after the employment of two complete frictions, furnish an indication to desist instead of to continue; and if they do not then disappear, they may be relieved by tepid baths.

There are cases, however, in which, from the existence of great abundance of pustules, the sulphuro-alkaline ointment would excite too much pain, or in which the patients have such an invincible repugnance to its smell, that we should resort to some other substance; and M. Bazin finds, by numerous trials, that the *lard* and *oil*, which form the base of all antipsoric preparations, if employed in general frictions, either together or separate, will effect a cure, only from four to six, instead of two, frictions being required. Another ointment recently tried, containing *chamomile*, cures in three frictions, soothes the itching instantly, and does not give rise to any secondary eruption. It is composed of equal parts of fresh chamomile, olive oil, and lard.

M. Bourguignon, in his recent prize essay, prefers the *staphysagria* to any other remedy. He adds 300 parts to 500 of lard, stirring the powder into the boiling lard, and keeping up a temperature of 100°, C. for twenty-four hours. After straining, a little essence may be added. Baths should be taken before and during the treatment, and the frictions should be made four times a day, the cure being completed by the fourth day.—*L'Union Médicale*, 1850, Nos. 82, 135. *Journal de Chimie Méd.*, No. xi, p. 671.

*Disease of the Heart and Chorea.*

M. TROUSSEAU frequently points out to his class the correlation so often observed between disease of the heart and chorea; so that examination of this organ, and inquiry into the history of the case as regards rheumatism, should never be neglected. Such correlation supports the views of those who regard the disease as rheumatic or rheumatoid; and indeed if this were not its nature, how can we explain that a disease which induces such marked disturbance of different parts of the nervous system, even to the intellect itself, should be so completely curable.—*Gaz. des Hôp.*, No. 86.

*On Lupus.* By M. CAZENAVE.

M. Cazenave rejects the term *lupus exedens* of Rayer, inasmuch as every lupus is *exedens*, this being the essential character of all forms of it. What has given rise to the term is the fact that there are two principal varieties of lupus, the *ulcerated*, and another in which instead of ulceration there is *hypertrophy*; but this latter form is no less destructive, and after a cure, though no solution of continuity has taken place, there is yet loss of substance and a cicatrix. With Bielt, Cazenave admits the form of lupus which destroys the surface, that which penetrates deeply, and lupus with hypertrophy,—forms indeed which are sufficiently well known. There is, however, another form more rare, hitherto described incompletely and classed improperly, it being the *erythema centrifuga* of Bielt, and



since his time placed among the erythemata by all writers, although in fact it is a lupus. It especially attacks young women when apparently in good health, and affects the face in preference, commencing by a point, and enlarging excentrically. It generally exists in the form of red, round patches, of two or three centimetres in diameter, the edges of which are much raised, and the centre depressed; the redness is of different shades, and disappears under pressure. When cured, each patch becomes a depression (the depression of the cicatrix being proportionate to the given hypertrophy of the spot) implying a prior loss of substance, though no breach of surface had existed; and it is on this ground that M. Cazenave terms the disease *Lupus erythematosus*. The disease is to be *treated* by sudorifics, as guaiacum, and locally by the tar ointment, or by more or less irritating resolvents, in proportion to the depth of the central depression. One of the best applications is from 1 to 4 of the iodide of mercury to 30 of oil, applied to the part every or every other day with a pencil. The pain caused by it is great, but the resolution of the tubercle is frequently soon accomplished.—*Gaz. des Hôp.*, No. 91.

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*On a Species of Atrophy of the Spinal Marrow.* By Dr. NAMIAS.

A woman, æt. 48, was admitted into the Venice hospital, who had suffered from paralysis of the lower extremities during three years, of the origin of which she could give no account. She possessed no voluntary power over them, and the upper extremities were also in a state of semi-paralysis and constant flexure. She was submitted to electricity during three months, and recovered power sufficient to enable her to walk slowly about the ward, the flexure of her fingers continuing. Some months after, her power again declined, but she was again improving under the use of electricity, when she was eventually carried off by diarrhoea in the cholera season.

At the *post-mortem* examination, the spinal marrow was found flaccid; and hollowed out in its substance was a cavity extending from the first cervical vertebra to within two fingers' breadth of the *cauda equina*. A catheter of medium size could enter this cavity, which occupied the centre of the medullary cord, the absence of the central gray matter giving rise to its production. Cruveilhier has noticed this absence of gray matter in children dying with spina bifida; and Morgagni relates the case of an adult, in which the cavity was large enough to admit the end of the little finger, but extended for a far less distance than in the present case.

Dr. Giacinto Namias observes, in reference to the improvement of the paralysed limbs which resulted from the use of *electricity* in this case, that it is only one out of many examples of such improvement being produced even in cases wherein serious organic lesion existed; so much so, that by its patient and *prolonged* employment, voluntary movement has often been procured, in cases in which the causes of the paralysis were absolutely irremediable.—*Rev. Med.-Chir.*, viii, p. 133.

The important resources derivable from the persevering use of galvanism, in cases of paralysis which have resisted all other means, have been frequently demonstrated by M. Duchenne, who, having had the principal hospitals of Paris freely thrown open to him during the last five or six years, has acquired a remarkable dexterity in its application. As he is about to publish a work upon the subject, detailing the results he has attained in respect to the diagnosis and treatment of the different forms of paralysis, we will only state here, that his plan consists in *localizing* the galvanic action upon the different muscles, which, by the employment of moistened excitors of a great variety of form and size, he believes he can do with the greatest nicety. He has derived few satisfactory results from *indirect muscular galvanization*, or acting upon the muscles through their nervous trunks. When he wishes to localize galvanic action upon the *skin*, he previously dries it with an absorbent powder, and passes over the surface dry metallic excitors, the sole result being the various degrees of sensation according to the intensity of the currents and duration of the intermissions.—*Archiv. Génér.*, tom. xxiii, p. 420.

*Case of Ossification and Obliteration of the Vena Cava Ascendens.* By Dr. COLLINS.

THIS case occurred in the person of a man æt. 34. He first perceived a small vascular tumour on the left side of the coccyx in 1847, which by the beginning of 1849 had reached a very large size, the abdominal veins also having become greatly enlarged. The tumour burst and gave rise to alarming hæmorrhage, and after this had again recurred, a "young physician, somewhat irregular in his practice," punctured it with hot needles, the effect of which was the coagulation of the blood of the tumour, and after a while its conversion into a suppurating one; the patient by hæmorrhage and discharge having become, when admitted into Providence Hospital, July 1849, exceedingly reduced. The superficial veins of the abdomen were now enormously enlarged, many of them being nearly, and some quite, as large as the small intestines, some of them inosculating with the mammary and axillary veins. Soon after his admission some of the smaller of the veins, situated on the left side of the thorax, commenced undergoing obliteration by spontaneous coagulation, and daily this process went on, additional masses of the varicose veins being thus closed up, the process being attended with considerable local inflammation. Although the tumour diminished in size, and the discharge in quantity, he gradually sank, and died at the beginning of November.

At the *post-mortem* examination, the external veins of the abdomen were found mostly obliterated, the coagula being quite absorbed in some and partly in others, and still imperfect in others. The femoral veins and the upper part of the saphenas were firmly plugged. Nothing very particular was observed in the other parts of the vascular system or the heart, until the *vena cava ascendens* was traced downwards to a little below the right renal vein, when it became impervious, terminating abruptly in a cul-de-sac. Below this the vein for  $2\frac{1}{2}$  inches was converted into a firm cord, about 2 lines in diameter, in the substance of which was a deposit of compact bone. From this osseous deposit to its bifurcation, the vena cava continued a dense, impervious cord, and the common iliacs would only admit a small probe being passed through them. The external iliac veins gradually enlarged, so that while under Poupart's ligament they were of a normal size, though still closed by a firm coagulum. The internal iliacs were reduced to the size of a crow quill, closed by old coagula, and lost in the remains of the tumour. The remains of the tumour, reduced to the size of a foetal head, lay to the left of the rectum, a part of the sacrum having been absorbed by its pressure. It was compact, and liver-coloured, and had lost all appearance of vascularity.—*Am. Journ. Med. Sc.*, N. S., No. xl, p. 395.

*On Cyanosis from Transposition of the Orifices of the Aorta and Pulmonary Artery.*

By Professor JOHNSON.

THIS occurred in the person of a mulatto child who lived for two months, the cyanosis being permanent, but liable to increase during unusual exertion. The child's body was plump and well nourished, though rather undersized. The lungs were but imperfectly inflated, though they floated upon water; and except in a few spots, where more air gained access, they were of the dark colour of a highly congested adult lung. The heart was of its usual form, size, and direction; but the pulmonary artery and aorta lay side by side from their origin to the division of the former, the aorta on the *right* and somewhat in front, the pulmonary on the *left* a little behind. The *foramen ovale* was *patulous*, the opening being an oval of  $4\frac{1}{2}$  lines by  $2\frac{1}{2}$ . The *musculi pectinati* of the right auricle were much developed, passing down from the upper to the posterior wall, and resembling the *columnæ carneæ*. The auriculo-ventricular orifice was natural. The walls of the right ventricle were nearly as thick as those of the left, and its *columnæ carneæ* were more developed and larger though less red. From the upper and anterior portion of the *right* ventricle, the *aorta* took its origin. The *left* auricle (with the exception of the opening of the *for. ovale*), the auriculo-ventricular opening, and the left ventricle were normal in structure; but from the upper and posterior portion of

this last, just behind the posterior fold of the valve, the *pulmonary artery* took its origin by a *free* and *patulous orifice*. From its origin it proceeded upwards and to the right for about an inch, when it divided into three branches, the right and left taking the usual course of the pulmonary arteries, and the middle one (the previous ductus arteriosus) piercing the concavity of the arch of the aorta.

Thus the right ventricle received, as usual, only venous blood, and propelled it into the aorta and through the whole systemic circulation. The poisonous effect of this unchanged blood was mitigated by the arrival of arterial blood through the *ductus arteriosus* from the left ventricle, the different organs of the body being supplied, as in the foetus, with a mixture of arterial and venous blood.

"The pulmonary artery communicating with the left ventricle which received the blood from the left auricle, which in its turn received arterial blood from the pulmonary veins, how was the venous blood of the *right* side carried into the lungs, and how was hæmatosis accomplished at all? This could only have been effected by the passage of a portion of the venous blood in the right auricle, through the open *for. ovale* into the left auricle, from whence, passing into the left ventricle, it would be propelled by the latter, in common with the arterial blood, into the pulmonary artery,—a portion of this mixed blood finding its way through the divisions of the pulmonary artery into the lungs, and the other portion passing through the *ductus arteriosus* into the aorta. Would not this fact militate against the doctrine advocated of late years, that after birth and the full establishment of the true circulation, no current would pass through the *foramen ovale* even were it open?"—*Amer. Journal Med. Sc.*, N. S., No. 40, p. 371.

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*On Morbid Changes in the Papillæ of the Tongue.* By Dr. DUBINI.

Dr. Dubini, while noticing the disappearance of the prominence of the papillæ of the tongue in *muguet*, so that to the eye and touch it becomes as smooth as a mucous membrane, takes the opportunity of observing that the morbid changes of the papillary body of the tongue have been very insufficiently studied and described in books, although a large volume might be written on the subject. Dr. Gamberini first noticed the *atrophy* the papillæ undergo in many diseases, and showed that tongues considered as good may be the subjects of such changes in the papillary body—appearing polished and shining, and being defective in the customary slight mucous secretion. They are too destitute of mucus, and denote by their abrasion a severe disease of the intestinal mucous membrane, and a diarrhœa that cannot be controlled, except after the papillæ become redeveloped.

Dr. Dubini has for several years carefully examined the state of the papillæ, and has observed in several diseases, as in certain cases of chlorosis, of enteritis, the early stages of scarlatina, &c., the coniform papillæ of the anterior half of the tongue become *hypertrophied*, appearing like so many fleshy projections of a uniform red colour, with an especially red point at their centres. This condition he thinks might be designated by the term *dotinoglossia*. A still more serious alteration of the papillæ is found in many tongues which are habitually chapped on the surface, or are traversed in various directions by deep furrows, resembling in their disposition a tree and its branches; but the change is by no means confined to such tongues, and may be met with in those otherwise normal. At the apex or edges of the tongue there form small, white, elevated, incomplete circles or irregular semicircles, which gradually increase in size until they occupy a full third of the organ, and by becoming confluent with those of the opposite side, circumscribe an area having a very smooth surface, with here and there a minute red papilla upon it. Within this area other semicircles form, which in like manner enlarge so as to give rise to a third or fourth production of incomplete concentric circles. When the disease is disappearing, the semicircles are less white, less raised, and less distinct, the enclosed area loses its smoothness, and after the isolated red papillæ have disappeared, those of a natural appearance present themselves covered with mucus, under which the normal colour of the tongue may be seen. This condition,

which Dr. Dubini calls the *festooned* tongue, is developed in many persons, sometimes even in one day, upon the occurrence of the slightest disorder of the digestive organs, and is in his opinion an indication of a chronic phlegmasia of the gastrointestinal mucous membrane, especially the intestinal, of which, with disorder in the digestive functions and in defecation, it is sometimes the only sign. As long as the festooned tongue continues, a convalescence from these disorders is not an assured one,—a slight improvement of diet at once increasing the semicircles, and aggravating the abdominal affection.—*Annali Omedei*, vol. cxxxiii, p. 374.

[In making our selections from foreign journals, we are always scrupulously exact in supplying references to the sources of the articles chosen, in order that these may be referred to their proper authors, and the correctness of the rendering verified; and we wish to suggest to those who transfer them to their columns, to state the medium through which they have obtained them, as we frequently find them reproduced without any such designation. In the 'Annali Universali' is a verbatim translation of a long article which appeared in these pages, furnishing a *resumé* of the prolonged discussion at the French Academy on *Engorgement of the Uterus*; and although we are pleased to find that the remarks we made upon the exemption of this country from the Lisfranc views on uterine disease, so exactly apply to Italy, as to require no alteration in their expression than the substitution of the word "Italia" for "England," yet we think their origin should have been indicated. We should not have noticed this, however, but from the fact of the publication of the Italian journals taking place at so much later a date than is professed to be the case. Thus, the volume containing the article in question extracted from our Number of April, 1850, bears date of March, while it was really published in August; so that any person taking up the two journals might suppose that *we* had committed the very fault we are deprecating.]

#### *Results of Revaccination in the Prussian Army, during 1849.*

DURING the year 1849 there were 51,637 individuals revaccinated, of whom 39,116 had distinct cicatrices of the former vaccination, 8706 had these in an imperfect condition, and 3815 were destitute of them. The vaccination

Pursued a regular course in . . . . .	30,457
An irregular one in . . . . .	8467
And failed in . . . . .	12,713
Succeeded on repetition in . . . . .	2862

Thus, then, of the 51,637 vaccinations, 33,319 were quite successful; vesicles running a normal course being produced. This proportion, amounting to 64 per cent., is nearly 1 per cent. more than was obtained in 1847 and 1848.

Of the different forms of variolous disease which appeared in the army throughout 1849, only 62 cases occurred, and were distributed as follows:

	In persons not revaccinated.		Revaccinated without success.		Revaccinated with success.		Total.
Varicella . . . . .	2	....	6	....	5	....	13
Varioloid . . . . .	14	....	17	....	9	....	40
Variola . . . . .	6	....	2	....	1	....	9
	<hr/> 22		<hr/> 25		<hr/> 15		<hr/> 62

In almost every case the disease ran a mild course, and frequently was quite insignificant. One case only was fatal. A recruit vaccinated when a child had not been yet revaccinated, and died on the 10th day.—*Med. Zeitung*, 1850, No. 19.

[We have been careful to record abstracts of these annual official returns, testifying as they do most strikingly to the value of the practice of re-vaccination, through the agency of which smallpox has now for many years been well-nigh extinguished in the immense Prussian standing army. To the greater prevalence of this practice may in part, also, be attributed the much fewer deaths from variola which take place in Berlin as compared with Prussia at large. Thus, while in 1840, 1 death in 227·2

was due to smallpox in Berlin, 1 in 173·7 was the proportion for Prussia. In 1845, the numbers were 1 in 4,562 to 1 in 171, and in 1848, 1 in 1022·6 to 1 in 244·7. While the average of 9 years, 1840-8, gave 1 death from smallpox in 289·2 deaths in Berlin, and 1 in 159·7 in the Prussian States.—See *Med. Zeitung*, No. 10.

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*On Vaccination and Revaccination.* By M. CRANINX.

AN interesting discussion has lately taken place at the Belgian Academy of Medicine, upon the subject of vaccination. The following were the conclusions of M. Craninx, the reporter, which were affirmed by the academy:—1. Variola and varioloid are but degrees of the same affection. 2. Simple variola may attack the same individual twice, but scarcely ever in rapid succession (*coup sur coup*). 3. It may also attack persons who have been properly vaccinated, but it is then generally mitigated. (M. Lombard observed that the word “generally” must be dwelt upon; for subjects who, to all appearance, have been well vaccinated, at a distant period occasionally die from variola. He added, that in the dreadful epidemic which has just devastated Liege, this was the case, while none of those who underwent revaccination took the disease.) 4. Variola, after vaccination, is almost without example within the next ten years; but it is observed from time to time in those who have been vaccinated for more than twenty years. It is, however, very rare after forty. 5. It is of more frequent occurrence, but, at the same time, milder, in the vaccinated, than in persons who have already undergone an attack of it. 6. Varioloid is observed oftener than variola after vaccination, and is not infrequently observed in children; but it increases both in frequency and severity from ten to twenty-five or thirty years. 7. Variola and varioloid in the vaccinated, not following the same course in respect to frequency or intensity as in the non-vaccinated, the cases in which they are seen among the former cannot all be explained upon the supposition of a faulty vaccination, but upon that of a diminution of the preservative action of vaccination. 8. Perhaps we should admit incomplete vaccinations possessed of a less degree of preservative power, and capable of becoming sooner exhausted. In this point of view, sufficient importance is not attached to the general reaction which should accompany the vaccine eruption, indicating the action of the virus upon the general economy. 9. If the protective power of vaccination has become enfeebled by time, if not in all, at least in several individuals, there is not sufficient evidence to show that the vaccine, considered in itself, has lost its efficacy since the first years of its discovery. While there is doubt, it is better to revert to the cow-pox whenever the opportunity presents itself. (Upon this resolution M. Seutin remarked that, believing as he did, that the vaccine lymph which existed is efficient, and that it fails either from not having been taken well, or owing to individual susceptibilities, he considered this conclusion would spread needless alarms. M. Lombard observed, however, that the new cow-pox, recently imported from England, certainly exhibited a more certain and more active effect.) 10. As the immunity conferred by vaccination is not indefinitely absolute, revaccination, at least for a great number of individuals, is rationally indicated. 11. Observation shows, that when it succeeds, the second vaccination produces phenomena very nearly like the first, so that we would, *à priori*, anticipate the same effects from it. 12. Experience has determined this point: it has proved that a recent revaccination preserves from variola and varioloid, and that, practised on a sufficient scale, conjointly with vaccination, it constitutes a sure means of arresting the progress of this malady when it appears epidemically. 13. It succeeds best in proportion as it is most required, that is, the more remote the period is since the individual has had variola, or has been vaccinated. 14. If it does not succeed at a first attempt, it should, if necessary, be repeated several times. 15. During the prevalence of an epidemic of variola or varioloid, it is prudent to revaccinate all those whose first vaccination dates ten years back, and all those whose first vaccination gives rise to any doubt. 16. Revaccination may be performed almost indiffer-



ently with the lymph of a primary or a secondary vaccination. 17. It is imprudent to inoculate with the lymph of spontaneous varioloid; nevertheless, in the time of an epidemic, if vaccine lymph could not be possibly obtained, we should be authorised in the employment of this fluid, and to transmit it as we do vaccine lymph. 18. If revaccination is so useful a thing, at least for a certain number of persons, vaccination loses none of its importance; and the government and the profession should exert all their influence to enable the entire population to participate in its benefits.—*Gazette Médicale*, No. 27.

*Cases of the Termination of Acute Rheumatism in Suppuration.*

By MM. FLEURY, ANDRAL, and TROUSSEAU.

A prolonged and interesting discussion has recently arisen at the Académie de Médecine, upon the occasion of the presentation of a report by M. Martin Solon, upon the proposition of a M. Dechilly to treat acute rheumatism by covering all the affected joints with large blisters, a plan, as might be supposed, that met with little favour. The opportunity was taken to enter into the question of the nature and treatment of the disease at large, and several sittings were occupied in discussing this. We have no intention of following the various speakers, since much that was said consisted of a reiteration of what is already known, or has been sufficiently refuted. M. Bouillaud defended his doctrines with his accustomed ability and want of success; and M. Piorry disputed with him the parentage of the practice of bleeding *coup sur coup*, which every one else has abandoned as mischievous. We will briefly notice a portion of the address of M. Malgaigne.

M. Malgaigne, after expressing his conviction that physicians would be less discrepant in their opinions concerning the disease, if they studied first its monarticular form, as found in the surgical wards; and, adverting to the fact of the utter ignorance which prevailed concerning inflammation, until Hunter studied its phenomena, at the surface of the body, and scattered the former hypotheses of physicians to the winds; stated that suppuration is by no means an uncommon termination of the disease, but that it then comes under the care of the surgeon. He believes that physicians are too easily contented with the removal of the general symptoms, and allow a patient to leave the hospital when a considerable amount of pain or debility of the joint continues. Under favorable circumstances, Nature herself will complete what is insufficient; but, at other times, the patients resort to the surgical wards, "to have their cures cured." He thinks, too, that in appreciating the employment of different remedies, we are too ready to yield them praise as they successively appear, without reflecting upon the natural tendency of this, and of all articular inflammations, to a cure, provided the parts are kept in quietude, and free from the irritation of external agents. He believes that amidst the multiplicity of formulæ, the natural indications are often neglected; one of the principal of which he considers to be, to protect the perspiring surface from the effects of cold, which, in most cases, has indeed been the original cause of the disease. The perusal of all modern cases proves the great inattention showed to this point, the patient being allowed to uncover himself to satisfy every want, or undergo any application; and in this way the disease may be rendered obstinate and relapsing.

Some of the speakers laid considerable stress upon the rarity of the termination of rheumatism in suppuration, and this has led to the publication in the medical journals of some examples of it. Professor Fleury, of Clermont, states that, up to 1848, he had always believed with M. Chomel, that rheumatism never so terminated; for, practising in a part of the country wherein the disease is of frequent occurrence, he had never met with a case of such before, although he had frequently had cases transferred to his surgical wards from the physicians', under the idea that such was the case. The present case occurred in a youth, æt. 18, of sound constitution, and hitherto of good health. He was admitted into the Hôtel-Dieu, at Clermont, 2d October, on account of acute rheumatism of the shoulder and knee, induced by sleeping in a damp chamber. Suppuration was set up, notwithstanding

active antiphlogistic treatment, in both joints, the abscesses being left to discharge themselves; and he died on the 12th of November, of purulent infection. On examination, the articular surfaces were found denuded, a metastatic abscess existed in the right lung, but no other disease of any of the viscera was found.

M. Andral read the next case to the Academy. A woman, æt. 67, of feeble constitution, was admitted into La Charité early in July last, suffering from pneumonia, from which she rapidly recovered by bleeding and tartar emetic, and was in a state of convalescence, when, from the effect of a current of air, she became attacked with acute rheumatism of the shoulders, unattended with complication, the attendant fever being great. She was bled, which so enfeebled her as to forbid the repetition; and next day twelve grains of quinine in the twenty-four hours were prescribed. The disease proceeded on to a fatal termination with fearful rapidity, without any complication occurring, any anormal sound of the heart, or any other joint participating. She died on the eighth or ninth day, having exhibited no other symptom than intense pain in both shoulders, a pulse of constantly increasing frequency, and a state of general anguish and rapid sinking, resembling that observed in acute peritonitis. The most careful examination of every organ failed to elicit any explanation of the issue; nor were there any signs of phlebitis or purulent infection. Both shoulder-joints, however, were found filled with white, homogeneous pus, and the synovial membrane was of the intensest red, the articular cartilage retaining its normal colour. Some of the bursæ in communication with the joints were also filled with pus; but external to the joints all was normal, bones, periosteum, and muscles having undergone no change whatever.

M. Trousseau relates a case also, which occurred in a child æt. 9, who, having always enjoyed good health, and been unexposed to privations, was attacked with intense scarlatina, and, during its prevalence, various of the joints became the seat of acute rheumatism. She died on the fifth day of the scarlatina, and third of the rheumatism. On examination, the various organs were found in a healthy condition, with the exception of the pleura, which contained a considerable quantity of serosity. Both shoulder-joints, and those of the elbows, knees, and ankles, were filled with a considerable quantity of greenish, well-formed pus, accompanied by considerable vascularity of the subsynovial cellular tissue.

M. Trousseau observes, that all practitioners who have much observed disease in children, must be aware what a powerful effect the exanthemata exert in engendering a *purulent diathesis*. If this child had contracted the rheumatism unconnectedly with the scarlatina, it would have probably determined the synovial effusion usually found; but the scarlatina having changed the *crasis* of the blood, developed the suppurative diathesis, and converted a trifling affection into one of an irremediable character. Was not the old woman, whose case is related by M. Andral, reduced, M. Trousseau inquires, by the debilitating treatment to which she was subjected, to a condition analogous to that into which scarlatina so often places children?—*Bulletin de l'Académie*, xv, pp. 774-785; *Gazette Médicale*, Nos. 26 and 32; *L'Union Médicale*, No. 102.

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*On the Treatment of Insanity.* By M. MOREL.

M. MOREL concludes an interesting Report upon the Asylum of Mareville (Meurthe), which during 1849 contained 890 patients, with the following propositions respecting the treatment of the insane:

1. An asylum is one of the elements necessary for the treatment of the insane; but on the conditions that order and discipline, the essential basis of moral treatment, are maintained, and that medicinal therapeutics have not to be exercised amidst faulty hygienic conditions.
2. Prolonged warm-baths have been employed with success in mania accompanied by great agitation; but it is to be feared that, if used in excess, they may lead to cerebral congestion.
3. The douche has been almost entirely replaced by irrigation of the head through the rose of a watering-pot.
4. Cold water irrigations along the spinal column, and especially over the

abdomen, are of service in cases of atony. They have been employed with success in onanism, and in hypochondriacal or melancholic patients having difficult digestion. 5. Hydrosudotherapeia is an excellent means in melancholia complicated with stupidity, accelerating the circulation, and improving the digestion. 6. General bleeding is scarcely ever employed, save in the complication of insanity with incident acute disease, (M. Morel states, he was at first much surprised to find so many patients brought to the asylum in a state of hopeless dementia, though still young, until he learned that they had already been submitted to copious and frequent bleeding,) and in epilepsy with great congestion of the brain or lungs. 7. Leeches behind the ears are often indicated, especially in cases of general paralysis. 8. The administration of tartar emetic by an enema is useful, when a saburral condition of the alimentary canal is present. 9. When loss of appetite and a disposition to vomit depend, as is often the case, upon a state of congestion of the brain or its membranes, a few leeches behind the ears relieve these symptoms. 10. The great irritability of the alimentary canal in many of the insane, should teach great caution in the employment of drastics. 11. M. Falret's practice of applying a seton to the nape in chronic mania, has been followed with success. 12. Ether and chloroform have furnished no successful results in the treatment of delirium. Patients while in a state of agitation have been put under their influence, to become just as bad when this has passed off. Most patients strongly resist its application, and one was nearly lost by consequent asphyxia. M. Morel has abandoned inhalation as a means of treatment, save for special complications. Thus, in a patient affected with tetanoid convulsions, and in another who kept his teeth closed over his bitten tongue, it was resorted to with complete success. 13. Opium has been given even to the extent of nine grains in the twenty-four hours, but no sufficiently satisfactory results have followed, and it is never employed in the plethoric. Camphor has seemed of utility in some cases of hysterical mania. 14. Great advantage has resulted from the destruction of a number of strong separate cells, within which refractory patients were locked, as in prisons. Since these patients have been allowed to be with the others, the asylum has been much less noisy, and the employment of the strait-waistcoat much less frequent.

*Annales Méd. Psych.*, N.S., vol. ii, p. 384.

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## SURGERY.

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### *On Hydrocele of the Neck.* By Professor MÜTTER.

Professor Müttter relates four cases of this affection, and subjoins a short account of its symptoms and treatment. The first case was a congenital one, and consisted of numerous sacs not having intercommunication, some of which reached a very large size, and encircling the throat and chin embarrassed the respiration. When they did so to a great extent, one of them was punctured with great relief, and that on two or three occasions. The child, however, died from oedema of the glottis, probably brought on by the compression of the distended sacs, which were found to be both large and numerous. The three others were acquired, and were all, as is the case with the bulk of recorded instances, situated on the *left* side. In one, in which a radical treatment was declined, 44 oz. of a chocolate-coloured fluid was drawn off, and several months elapsed before reaccumulation. In another case a seton was introduced, which produced no bad symptom, and was followed by a radical cure. In the last case this was obtained by dissecting out the cyst, which was only the size of a walnut.

Speaking of the *treatment* of this disease in general, Dr. Müttter observes, that the operation of *excision* should be confined to cases in which the tumour is small, circumscribed, and superficial, and when a small scar is not considered of importance, under which circumstances it is the preferable plan. *Repeated tapplings*, care being

taken to empty the sac of all fluid lest it infiltrate into the cellular tissue, may be employed as a *palliative* in congenital hydrocele, when severer measures might be undesirable, and may sometimes effect a radical cure by keeping the sac constantly empty. In old cases it may be resorted to to relieve cogent symptoms, or when the patient will not submit to other means. The chance of a radical cure is increased by scratching the interior of the sac with the trocar before withdrawing it; and after the operation, moderate pressure should be kept up over the sac. The shortest plan of curing one of these tumours, if superficial, is *free incision of the sac*, following it by moderate compression; but the long and unsightly scar it leaves would be with many an insuperable objection. In order to convert the surface into a granulating one, several surgeons have combined with incision the *application of stimulating substances to the sac*, as lint, iodine, wine, &c. This, however, excites great inflammation, is followed by a large irregular cicatrix, and requires much time. *Injections*, Professor Mütter states, are abandoned, on account of the inefficiency of weak and the danger of strong ones. (Such would seem the very cases for iodine injections, which indeed have been used with success in this affection.) The *seton* is the best means for cases in which extirpation, repeated tapping, or incision are inappropriate or have failed. No means is so promising, when the disease is unilocular and of long standing. The treatment occupies several weeks, perhaps, but the method is safe, easy, slightly painful, certain, and followed by scarcely any scar.—*Philad. Med. Ex.*, N.S., vol. vii, pp. 257-274.

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*Operation for Ingrowing Toe-nail.* By M. BAUDENS.

M. Baudens states, that he has performed the following operation more than 200 times without accident of any kind, little pain being caused, and that only for a few seconds. The right hand is armed with a narrow, straight bistoury, held like a knife while cutting a pen. The free extremity of the toe is firmly fixed by the thumb and index finger of the left hand, so as to render the diseased part prominent. The operator now carries the edge of his knife (on the outer or inner side of the phalanx according to the situation of the disease, and equidistant from the root of the nail and the next phalanx) perpendicularly down to the bone, and then inclines it towards himself, shaving the phalanx, and carrying off at one stroke the degenerated soft parts, the diseased portion of the nail, and the corresponding portion of the *matrix*,—the removal of this last being indispensable, in order to render the reproduction of the disease impossible. For two or three days he keeps the parts surrounded by ice (which, however, is no essential part of the plan, but his mode of treating recent wounds in general), when granulations spring up, and eventually a horny description of cicatrix, forms an excellent substitute for the nail.—*Gaz. des Hôpitaux*, No. 77.

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*Cholesterine in Sparkling Synchysis.*

It has long been supposed that the sparkling appearance in this form of synchysis was due to the presence of *cholesterine* in the fluids of the eye; and M. Lebert has recently been able—by microscopical examination of certain bodies, which in a case of M. Sichel's so obstructed the anterior chamber as to compel extraction,—to show that cholesterine constituted the principal portion of these.—*Annales d'Oculistique*, tom. xxiv, p. 54.

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*Case of Hernia of the Bladder.* By Dr. MARKOE.

This occurred in a female child, about three years of age, during tenesmus and prolapsus ani from diarrhoea. The author found a dark red tense tumour, the size of a pigeon's egg, protruding from the upper part of the vagina. It became rapidly tense under the intense straining of the child, but this was relieved by the use of chloroform. A catheter could be passed into the bladder, but not so directed as to evacuate the protruded portion; and while the author was considering how best

to remove the strangulation of the part, the tumour burst and discharged its contents, and then admitted of replacement. The hole in the vaginal parietes, through which the protrusion had occurred, was about a third of an inch in diameter. The parts were supported by lint stuffing, and the tenesmus relieved. The child perfectly recovered, without any signs of a recurrence of the protrusion, or the formation of a vesico-vaginal fistula.—*New York Journal of Med.*, N.S. vol. iv, p. 322.

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*Spontaneous Cure of Spina Bifida by Rupture of the Sac.* By Dr. NANCE.

THE subject of this case was a girl *æt.* 13, in whom a tumour, situated at the juncture of the lumbar vertebræ and the sacrum, had increased from the size of the end of the thumb to that of a quart measure. All whom the mother consulted in the child's infancy refused to meddle with it, believing that doing so would hasten a death they regarded inevitable. However, the child, though rather delicate, continued to grow, with the use of all its limbs; the prominence of the tumour being visible externally to all her clothing.

After an attack of measles, the skin over the tumour became inflamed, and eventually sloughed, and an immense quantity of straw-coloured fluid came gradually and constantly away, the patient being exceedingly exhausted, as if from hæmorrhage. External and internal stimuli were freely administered; and after the slough had entirely given way, the whole of the water had been discharged, and a healthy purulent secretion had replaced the gangrenous one, she recovered. The remains of the sac gradually contracted, and there is only left a semi-cartilaginous lump, the size of a walnut, acting as a protective covering where the bone is defective. Two years have elapsed, and the girl has become a healthy and active young woman.—*Am. Journ. Med. Sc.*, N. S., No. xl, p. 552.

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*On a Fatal Case of Ligature of the Internal Iliac.* By Dr. KIMBALL.

IN this case, occurring in a man, *æt.* 35, the internal iliac was taken up, at about an inch from its origin, on account of a pulsating tumour of six years' standing, situated at the posterior part of the thigh. Very little blood was lost during the operation, but on the 16th day profuse hæmorrhage issued from the wound, which never manifested any disposition to heal, and on the 18th day the patient died. At the *post-mortem* examination the wound was found to have remained quite unhealed, and no coagulum existed in any of the iliac vessels, not even between the ligature and the bifurcation. The portion of the vessel embraced by the ligature had begun to give way, explaining the origin of the fatal hæmorrhage. The aneurismal tumour was not examined.—*Amer. Journ. Med. Sc.*, No. 39, p. 92.

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*Case of Fatal Peritonitis from a Perforating Bubo.* By Dr. LENTE.

THE patient, aged twenty-two, was admitted May 30, with a suppurating bubo, situated much nearer the ant. sup. sp. proc. of ilium than usual, the integument being undermined above this point, but not below. The surface of the sore was healthy, except at the lower part, where the ulceration also extended deepest. He seemed to be going on very well until June 8, when he was seized with profuse vomiting and purging, followed by excessive prostration, and rapid small pulse, but unaccompanied by any abdominal tenderness. After several hours this purging abated, and the patient was able even to read, but he soon began to complain of some tenderness near the pubes; his pulse, too, still continued rapid; vomiting again reappeared, the tenderness increased, and he died on the 9th.

At the *post-mortem* examination, a probe could be passed from the bubo into the abdomen with ease; and on opening this cavity, it was found to have been the seat of *acute peritonitis*. The colon, just prior to giving origin to the sigmoid flexure, was found firmly glued to the internal abdominal wall by fibrinous bands, at a point corresponding to the situation of the bubo. The bands were of recent formation, but



bore considerable traction. This attempt of nature to prepare an exit for the contents of the bubo through the intestine was, however, defeated; inasmuch as an opening into the peritoneal cavity itself was found near the left sacro-iliac synchondrosis, whence pus could be seen oozing. (Owing to the true nature of the case being unsuspected, the patient was inefficiently treated by mere palliatives.)—*New York Journal of Med.*, N.S. vol. v, p. 169.

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*On a Case of Dislocation of the Fifth Cervical Vertebra, with Congenital Fusion of the Sixth and Seventh.* By Dr. LENTE.

THE subject of this case fell head-foremost into an area, the head being forcibly bent backwards. He died on the third day, and at the *post-mortem* examination the fifth cervical vertebra was found displaced from the one below it. "The luxation is backwards, the intervertebral substance being torn through without being separated from either vertebra. The lateral laminae of the body of the sixth, which enclase the body of the fifth, are not at all injured. The articular processes are dislocated without fracture, and without much displacement; the latter being prevented, apparently, by the end of the spinous process of the fifth coming in contact with and resting on that of the sixth."

In this case, too, the sixth and seventh cervical vertebrae were found fused into one. In the middle of the body is a transverse line, with a rudiment of intervertebral substance about a line in length. All the lower part of the body is expanded by the deposition of white osseous matter, as compact as that of the cylindrical bones. The spinous process is single, a slight furrow marking the line of junction on one side only. There are two superior articular processes joining the vertebra to the fifth, and two inferior ones joining it to the dorsal. There are four transverse processes, the two on each side being united together.—*New York Journal of Medicine*, N.S. vol. iv, p. 284.

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*Case of a Wound of the Heart, with a commencement of Cicatrization.*  
By Dr. TRUGIEN.

A stout negro, æt. 21, after being stabbed in the cardiac region, fell into a state of extreme collapse, whence he rallied after some hours. By perfect horizontal rest and low diet, he seemed to be doing so well, and his breathing was so normal, that it was believed the heart had escaped. Going out on the fifth day, however, he suddenly expired after employing exertion. The instrument was found to have passed a line or two to the right of the septum into the left ventricle, through which blood had escaped into the pericardium, where it was found partly fluid, partly coagulated. The wound in the pericardium had quite *cicatrized*, as had that of the heart through two thirds of its extent.

Dr. Trugien adduces this case as confirmatory of the views of those who believe that wounds of the heart may be recovered from, providing the patient observe absolute rest sufficiently long. Instant death from this cause is by no means common; for Ollivier has shown, that of twenty-nine cases which he collected, only two proved fatal within forty-eight hours, the others living from four to twenty-eight days. Beck has brought together a great many cases of prolonged survival; and in several of the cases published by Dr. Coxe, in an early number of the '*Amer. Journ. Med. Sc.*,' life was prolonged to the sixteenth or seventeenth day. Dr. Ramsay relates the case of a negro boy ('*Western Journ. Med.*,' vol. i), who survived for sixty-seven days; and at least one example of recovery is on record in the '*Med. Gaz.*,' vol. xvii, p. 82.—*Amer. Journ. Med. Sc.*, No. 39, p. 99.

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*Case of Luxation of the Penis.*

THIS curious case occurred in a lad æt. six, who fell from a cart. He was treated for the resulting contusions for some days, when it was observed that urine passed through a wound near the left buttock, and he was sent to the St. Louis. M.

[We shall be anxious to know the issue of this extraordinary case. Deception of some kind might be suspected: but the idea of this seems negatived by the strict scrutiny the case underwent; the real suffering of the woman, in presence of the reporter; and her general good conduct, which induced her to persevere in the performance of her duties even in the midst of the greatest anguish.]

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### MIDWIFERY, &c.

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#### *On Fatty Degeneration of the Placenta.* By Professor KILIAN.

ALTHOUGH the so-called hepatized state of the placenta, supposed by Rokitansky to depend on *placentitis*, but shown by the researches of Lee, Jäger, Gierse, and others, to be in reality an apoplectic condition of the part, is a frequent cause of abortion; yet there is another diseased condition of the organ, having nothing in common with this, and which it is the object of this paper to illustrate. Dr. Kilian had the opportunity of minutely examining the placenta of an eight-months' child, which had died a fortnight before birth. It was found to be pale and easily torn, the uterine surface being of a bright yellowish-white colour for two or three lines in depth, and of a brittle friable texture, as compared with the red portion of the placenta, from which it was separated by no exact line of demarcation. Examined with the microscope, the *extreme ends of the vessels* of this portion were found to form little knobbed swellings, composed of *fat globules*, strongly reflecting the light. From these ends of the vessels, filled with fat droplets closely packed together, the blood corpuscles of the placental vessels were quite absent; but in proportion as the vessels were traced back from their terminations, the fat-globules were progressively replaced by blood globules, the walls of their vessels, which at their terminations were also loaded with fat, recovering their natural appearance. The remotest ramifications of the healthy portions of the placenta continued entirely normal; and where the yellow portion joined the coloured, many of the terminating vessels were destitute of fat, others contained droplets, but none showed the dense masses observed at the uterine surface.

It is evident, that such an obturation of the terminations of the vessels as this, must give rise to complete interruption of the circulation, and when prevailing to a great extent, cause the faulty development or death of the child; but it may be inquired, whether this fatty formation occurs during the healthy conditions of life and becomes the *causa mortis*, whether it results from decomposition after death, or whether originating during life it may be but the expression of other causes inducing the degeneration of the ovum, and a symptom of retrogressive metamorphosis of the placenta. On examining the foetus, all parts were found apparently normal, except a contused state of its head and face, and a large effusion of blood between the scalp and cranium; so that it might be inferred that the child died from some cause of pressure on the brain, and the described changes of the placenta were only the product of a decomposition consequent on its death. Against this view, however, many objections may be urged. There is no fact known of the conversion of stagnant blood into fat. Virchow and Reinhardt have only seen the formation of fatty corpuscles within the colourless blood corpuscles. The changes which occur in the terminations of the blood-vessels, or other metamorphoses after death, authorise no such view. The author, too, after macerating healthy placentas in warm water until approaching putrefaction, never found this obturation produced, a few droplets here and there being found only in the walls of the vessels. Moreover the limitation of the fat-globules to the extremities of the vessels, and the normal state of a large portion of the placenta, show that it was no *post-mortem* change. The obstruction of the vessels must then either have commenced during the normal life of the child, and by the interruption of the circulation have led to its death, or the formation of fat was contemporary with its decay from other causes, and only

constituted a symptom of another more distant cause. The author believes that many additional observations are required, to enable us to come to any decision upon this point; and any statements he now makes are entirely provisional. He regards the change then as a retrogressive metamorphosis or involution; the primary seat of the disease of the child being in the extreme ends of the vessels, the change apparently depending upon a general lowering of the vital powers of the ovum by the agency of disease or medicine. A sluggish feeble circulation through the remote ends of the vessels, and a resulting defective nutrition of the vessels themselves (perhaps aided by the influence of opiates or other means of weakening the heart's impulse), may be the earliest stage of this retrogressive metamorphosis, leading to fatty formation in the cells of the vessels. Discharged into the channel of the vessels and obstructing their terminations, the mutual action of the blood and vessels become more and more impeded, until by accumulation a large number of the capillaries are completely stopped up—exerting an effect upon the life of the child very similar to that which ligature of the funis would do. The effusion of blood observed in the head of this child was due, the author believes, to the embarrassment of the circulation produced by the fatty obstruction of the small vessels of the placenta.

The *funis* of the child which furnished the above-described placenta, was of a brown-red colour, and so swollen throughout its whole course as to equal a large thumb in thickness. All its meshes were filled with dark red gelatinous substance, the colour of which was due to the transudation of the stagnant blood contained in the vessels at the time of the child's death. It is very common to consider as *signs of the death of the child in utero* a separation of the epidermis, and a flaccid and discoloured state of the funis. But a mere discoloured state of the funis may arise from the imbibition of a yellow or greenish liq. amnii, and may be present in children born alive and well, and the epidermis may become very decidedly loosened even in live children; but this *transudation of the colouring matter through the containing walls* of the vessels, is a very certain sign of death having occurred at least some days.

As an appendix to his communication, Dr. Kilian adds a few notes of a *case of placental apoplexy produced during labour*. The labour was natural, and a first one, the child being born after one hour's excessive and continuous pain. The placenta was normal in structure, and vascular; but contained four cavities, the size of walnuts, filled with blood, which flowed out as they were divided. Besides these, numerous other extravasations, the size of a hazel-nut, were diffused through the substance of the placenta; these were of quite recent occurrence, as shown by the fluid state of the blood, and seem to have been produced by the violence and continuousness of the uterine action.—*Neue Zeits. f. Geburt.*, b. xxvii, pp. 34-62.

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*Cases of Fatal Extra-Uterine Pregnancy.* By Professor JOHNSON and Dr. DENNY.

Professor Johnson's case occurred in a married, healthy, childless, regularly menstruating negress, æt. 30. The catamenia last appeared about Dec. 1, 1848; and on the 18th she had a violent attack of pain in the abdomen, with vomiting, supposed to be uterine pain, and relieved by mild antiphlogistics and sedatives. Repeated similar attacks occurred, and a tumour formed in the hypogastric region, and increased as the uterus ordinarily does in pregnancy, the breasts also becoming lactiferous. In August, 1849, about the time of the expected labour, some sanguineous vaginal discharge occurred; but this, as well as some pain which came on, subsided in a week. Soon after she was confined to her bed for some weeks with *phlegmasia dolens*, and after she got about again complained occasionally of pains in loins and abdomen.

She was admitted May 22, 1850, when the abdomen presented a large uniform tumour, very much like a huge mammary gland, with the umbilicus as its retracted nipple. As the uterus was found to be neither diseased nor impregnated, it was resolved to make an exploratory incision into the tumour, which was eventually carried down from above the umbilicus to near the pubis. This, after the discharge of

about a pint of sero-purulent fluid, brought a full-sized foetus into view, which with little difficulty was removed. The cord, which was flaccid and bloodless, and the placenta, which was atrophied, were attached along the inner side of the sac containing the foetus, and required removal like an uterine adherent placenta the numerous vessels which connected it with the sac being however quite impervious. The sac was formed of very firm plastic lymph, closely connected with the abdominal parietes anteriorly, and separating the foetus completely from the abdominal and pelvic viscera. The child weighed nearly *nine pounds*. The skin was separating in most parts of the body, the eyes were deeply sunk in the orbits; but there was no foetor. For sixteen days the patient seemed to be going on very well, when she became attacked by a prevalent diarrhoea, and died on the 24th day. The *post-mortem* examination was made ten hours after. Although the viscera of the hypogastric and iliac regions were a good deal displaced, there was very little disease. The sigmoid flexure of the colon adhered to the sac, and was found thickened. Its mucous membrane was softened, and that of the rectum congested and slightly ulcerated. The sac presented no communication with the abdominal or pelvic cavities; and its interior was black and soft, emitting a strong gangrenous odour. The unimpregnated uterus and its appendages were found compressed between the sac and the bladder. The right ovary was normal, and the Fallopian canal pervious only to some distance. On the left side, the canal was quite impervious, and the ovary hidden amidst lymph.—*Phil. Med. Ex.*, N.S., vol. vi, pp. 511-522.

Dr. Denny's case, which he believes to be [an example of ventral extra-uterine pregnancy, the foetus not having escaped from the tube, is detailed with almost tedious minuteness, which is, however, excusable, inasmuch as his diagnosis of the case was opposed to that of high authorities.

The menses were suspended after April, 1848, and after October the patient felt the child move as she had done in a former pregnancy, until the end of January, 1849, her size increasing in the usual manner. The movement ceased at the end of January; but at the end of May, pains, apparently parturient, were set up. After this, at two separate times, vaginal discharges, described by the patient exactly as menstrual, occurred. Great constitutional irritation was set up, and the author pronounced the case one of extra-uterine pregnancy, which had given rise to simulated pseudo-labour pains. All doubt was removed on the 12th of October, when, through an ulcerated opening in the vagina, the head of the foetus could be felt, and a fragment of its scalp was detached by the nail.

She died in November; and on opening the abdomen, a smooth, purplish tumour, with walls of about one and a half lines in thickness, was observed, and on opening this a foetus was discovered. The skin and subjacent soft parts were completely converted into adipocire; and at the termination of the funis some shreddy remains of a placenta were observed, which seemed to have occupied a place at the upper and back part of the cavity, where however it was not now adherent. The foetus was not weighed, but all agreed in regarding it as a mature one of not less than seven pounds. The uterus, which was pushed on to the pubes, anterior to the cyst, was unimpregnated, and contained no decidua. Both it and its appendages were normal, except for a small cyst in one ovary. The uterus adhered to the tumour by its sacral aspect; but the tubes and ovaries were free. No rent or cicatrix was discoverable in the uterus.—*Amer. Journ. Med. Sc.*, No. 39, pp. 49-62.

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#### *On Lactation.* By M. TROUSSEAU.

EXHAUSTED as this subject might seem to have become, M. Trousseau nevertheless makes several interesting observations upon it. An infinitely larger number of infants are brought up by wet-nursing in France, especially in the large towns, than with us; for not only are the mothers more occupied in business or amusement, but their medical advisers are accustomed to place more restrictions upon their acting as nurses to their own children, than we think desirable. The mother should be allowed to follow her natural inclination to nurse, M. Trousseau

says, when she is strong, has a good constitution, is unaffected with hereditary disease, and her breasts are of a good configuration, provided she is not so placed that she is able to take little or no exercise, or cannot resign the dissipated habits of gay society.

In deciding beforehand whether a woman is likely to prove a good nurse, the *large size of the breasts* has been erroneously considered a sign of the affirmative. The mammary gland, separated from the adipose matter surrounding it, is always very much of the same size, and does not secrete milk in proportion to the size of the breast. A far better sign is drawn from the veins of the organ being very distinct and its vascularity well marked. Another is the fact of the woman having been accustomed to perceive a vigorous flux towards the *mammæ* just before each menstrual epoch, the breasts then becoming hardened and the lobules of the gland as it were more prominent. When this physiological sign is distinctly present, it is the most important and most certain of all, although not indicated by preceding writers.

When a young woman becomes pregnant, her *nipples* swell and increase in length to about 4 lines, the surrounding papillæ become prominent, and the areola dark. Such a person will have a good nipple for suckling; but in a great number of women the nipple is only on a level with the breast, and, in such, means should be taken to bring it out during the latter months. The best means of effecting this is to wear a nipple-shield under the corset. If this is worn for the two or three last months, the nipple is found to have acquired the necessary projection by reason of the compression that has been exerted at its base alone.

As to the period when the child ought to be applied to the breast, M. Trousseau directs this to be a few hours after delivery, when it will very shortly procure milk, although what is called "the draught" may not take place for two or three days. If suckling be delayed till then, the breast becomes painful, the nipple buried and ill-erected, the lacteal vessels obstructed with colostrum, and suction is both difficult and painful; while engorgement and abscess of the breasts, or fissures of the nipple, may compel an abandonment of suckling altogether. [There can be no doubt of the importance of early suckling here urged by M. Trousseau, especially with the first child; but in women who suffer severely from after-pains, these are dreadfully increased during suckling, so as often to necessitate its postponement.] With respect to the child, M. Trousseau advises an ingenious procedure which we are inclined to think might have saved trouble in some cases we have seen. It is the giving it to suck, before applying it to the breast, a very short hard piece of linen, tied up in the shape of a nipple, and moistened with a little sugar and water. The infant likes the taste, but the body being hard and short, it finds, after its efforts upon it, that the suction of the mother's nipple is a much more pleasant operation. It has already tried its strength against a greater resistance, and without having done so it will often not take the nipple kindly. The fluid employed must not be too sweet, or the mother's milk will appear tasteless afterwards.

As to *regulation of the suckling*, this must not be attempted at first. Children in hospitals lose weight during the first nine days, and in private life during the first two days, and at first they should be allowed to suck as much as they will, which is also of use to the mother, who has then abundance. This may go on during the fortnight or month that she is within doors, and can sleep or rest as she likes; but when she resumes her occupations, suckling should be regulated, and above all things so as to allow rest at night. To this end, during the first two or three months, the child should be suckled at short intervals during the day, but only once between 8 and 9 at night and 7 the next morning. After three or four months, most women require a little assistance; and a meal of milk mixed with any suitable fluid should be given the child by the sucking bottle at the beginning or middle of the night, by which means the mother's repose and the reaccumulation of her milk are aided. During the day artificial food must at first be chiefly formed of milk or farinaceous matters, by far the best of these being, in spite of Rousseau's denuncia-



tions, *pap*, made with the crumbs of well-baked, light, stale bread. It is of importance that the food should not always be the same, but composed of different farinaceous articles, &c., at different meals, and care being taken to observe which is digested best. As the time for weaning approaches, the food must be more and more varied, eggs differently prepared, chicken and other broths, and especially beef-tea should be employed, still however encouraging the infant's taste for milk.

In respect to the *nurse*, of course she should observe all the rules dictated by a sound hygiene, if she wishes to preserve her milk good. The most absurd prejudices prevail in respect to nourishment; and thus it often happens, that a nurse arriving from the country, who had enjoyed excellent health on a vegetable diet, becomes ill and loses her milk through the highly animalised food she is overloaded with. If a nurse menstruates, 99 out of 100 mothers would change her, although the milk is as good, but in a smaller quantity just at the epoch. Most women after 11 or 12 months menstruate without their milk deteriorating. The case may be different if menorrhagia or leucorrhœa is present. M. Trousseau also considers that the milk, though lessened in quantity, is not deteriorated by the occurrence of a new pregnancy. About the sixth month, however, becoming mingled with colostrum, it purges the child a little, and then weaning should be resorted to. He has very frequently seen a nurse able to suckle quite well until her confinement, although he certainly would not choose one if he knew she was pregnant.

In towns *bringing up children by hand* is a most difficult matter, and never to be advised. In the provinces, where abundance of good milk can be got, it is far easier, and in Normandy almost all the children are so brought up. Even there, however, a great number perish. Among all the varieties of sucking bottles, the best is a phial, corked by a piece of sponge, and tied over by a piece of old linen, the fluid always being given at the temperature of the body. It is necessary that the child should make some effort in sucking, or saliva is not educed. Instead of the linen, a piece of chamois leather answers well, if kept constantly well washed.

*Weaning* should be delayed as long as possible, provided neither nurse or child suffer, and in the case of weakly children even for two or three years. This is the epoch of their dentition and various maladies, and it is very convenient to have so digestible an aliment at one's command. It is to be observed, too, that the longer they continue to suck, the less do they entirely depend on the breast. Age should not guide us in determining upon weaning, so much as the evolution of the teeth. This evolution takes place in *groups*. In the first group, the two lower median incisors appear, in the second the four upper incisors, in the third the four first molars and two lower lateral incisors, in the fourth the four canines, and in the fifth the four last molars. During the evolution of these respective groups, weaning should never be attempted; while, after this is completed, an interval of repose favorable to the attempt occurs. It is not until the canines have appeared that we should advise weaning, if avoidable, these being the teeth attended with the most difficulty in their evolution. The child before weaning should have been gradually accustomed to other articles of diet; and in those cases in which it seems to have an aversion to milk, every variety of food must be tried till the suitable one is found. There is, in fact, no rigorous rule upon this subject; but that should be given which is best digested.—*Gazette des Hôpitaux*, Nos. 23 and 24.

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*On the Diminution in Weight of New-Born Infants.* By Dr. HOFFMAN.

It is a popular opinion that new-born children fall away during the early days after birth; and seeing that during this period they evacuate meconium, the epidermis exfoliates, and matters are expired from the lungs, while the little milk they take is little else than a purgative colostrum, there is reason for its entertainment. To test it practically, however, Dr. Hoffman had thirty-six children, who were successively born at the hospital at Würzburg, weighed every morning; and in the present article he gives the tabulated results of the daily examination, from which the general conclusion is drawn, that all children for the first thirty-six or

forty-eight hours after birth become lighter, such diminution in the majority continuing until the third day. From this period they increase again, so that by the fifth or sixth day they have in general recovered the weight they possessed at birth.—*Neue Zeits. f. Geburt.*, b. xxvii, p. 145.

*On Auscultation and the Pneumonia of Infants.* By MM. TROUSSEAU and LASÈGUE.

THE authors preface their Essay by some observations upon auscultation of infants in general, which they consider to be far more difficult than is usually supposed. The restlessness and cries of the child confuse the results; and the small space to be operated upon, with the inconvenient position to be assumed, prevent the prolonged examination which is necessary. When we are in no doubt as to the nature of the disease, its limits often cannot be ascertained, for an abnormal sound, whenever it may arise, is heard all over the chest. Both normal and abnormal sounds are, therefore, best heard where the ear can be most conveniently placed, namely, *opposite the subscapular fossa*. From the *resonance* of the cry little is to be learned, and the *souffle* of the child is indistinct. The comparative exaggeration and feebleness of the respiration in different parts of the chest are seldom to be observed, and the exaggerated sound of one lung when the other is bad,—puerile respiration,—is comparatively wanting in the child. Most is to be learned from the *râles*. The *crepitant* is very rare, and has not the fineness or the dryness of that of the adult, the *subcrepitant* and *sibilant* being those usually present. Their temporary absence does not indicate the absence of lesions, for the subcrepitant *râle* may cease for minutes or even hours, and return again, no apparent cause for the oscillations being observable.

The Pneumonia of Infants is never an original affection, being always preceded for a longer or shorter time by bronchitis; and it is very important to seize the point of transition of the one into the other, as the implication of the parenchyma so much adds to the danger. Bronchitis may exist only for a short time, or for months, before being followed by pneumonia. Indeed, there are two forms of it; one in which *pneumonia necessarily follows*, and the other in which it is a mere *coincidence*. In the first case, there is much more fever present than the local symptoms seem to warrant, and the cough is dry without *râle*, the first sound that is heard being the subcrepitant *râle* of the pneumonia, two or three days after the bronchitis has existed. In the other case, the catarrh passes through its usual stages, having the large mucous or sibilant *râles* present, the subcrepitant *râle* appearing suddenly among these, and not the one gradually becoming fused into the other. In general the sibilant *râle* gives way to a mucous one of short duration, prior to the occurrence of the subcrepitant rhonchus. Whenever bronchitis, characterised by cough and fever, does not manifest its auscultatory signs, pneumonia is imminent; but when mucous or sibilant *râles* have been heard for some time, the subcrepitant *râle*, when it occurs, has no longer the same importance. Sometimes, indeed, the subcrepitant *râle* is never present, the *souffle* alternating with the mucous. In other cases it may accompany the disease in all its stages; and in some that run a rapid course, especially in rickety children, the disease may terminate fatally without any other having become developed. Whenever it ceases completely, without being replaced by any other form of rhonchus, the general symptoms still continuing menacing, the augury is very bad. If, on the other hand, it persists, though the respiration be easier, the fever less intense, and the countenance less anxious, the amelioration cannot be relied on, for it is only a truce. This *râle* is larger, the more superficially the affected lobules are situated, and is smaller when they are more central. When the hepatized tissue is found highly coloured, it has persisted to the last; but when this is pale, it has usually given way, prior to death, to other signs. It is no measure of the more or less advanced stage of the lesion.

The *souffle* in hepatization, at first soft and veiled, becomes eventually almost as distinct as in the adult, being either accompanied by the subcrepitant *râle*, or alter-

nating with it. It may disappear for short intervals, and undergoes some change from posture. It is one of the most certain signs of the disease, and if the patient dies without its being developed, this is because his prior state of health or some other general accident has complicated the case. It indicates that hepatization occupies an extensive portion of the parenchyma; and by practice its intensity becomes a measure of this. Neither it nor the subcrepitant *râle* indicates the degree of the change, though they may its extension,—different parts of the lungs, in fact, undergoing simultaneously a very different degree of change. The *souffle* appears early in that form of the disease, in which contiguous inflamed lobules unite and invade a whole lobe or contiguous lobes; while in the form in which the hepatized lobules are dispersed over almost the entire lung, so as to unite nowhere into such large masses, although constituting a great aggregate, the *souffle* appears later.

As in the adult the sign of improvement is a returning crepitation, so is the returning mucous *râle* in the infant, displacing the *souffle* and subcrepitant *râle*, though it may reappear without the intervention of this last.

All the auscultatory phenomena are never entirely wanting during the whole course of the disease, but any one of them may be so, even in cases of such severity as to lead to the full expectation of their presence.—*Arch. Générales*, xxiv, pp. 130-42.

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## MATERIA MEDICA AND THERAPEUTICS.

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### *Additional Observations upon the Tartrate of Potass and Iron.* By M. MIALHE.

M. Mialhe has already published various observations, showing the therapeutical value of this salt of iron, on account of its tastelessness and the ease with which it is absorbed; and he now adds some additional ones upon the chemical changes it undergoes in the alimentary canal. It results from his own experiments and those of M. Leras, that all ferruginous solutions become entirely or partially precipitated in presence of the gastric juices, and form with the animal matters of these an insoluble compound which passes into the small intestine. Here the alkaline juices decompose the precipitate, and set the oxide of iron at liberty, which then mingles with the *feces*, and is expelled without any advantage to the economy. Thus, martial preparations can only be absorbed by reason of the small quantity which escapes the action of the gastric juices and is absorbed directly, or the very small quantity of the precipitate which may become dissolved in an excess of gastric fluid; for once admitted into the intestinal canal, they lose all efficacy by the decomposition of their principles, and by the absence of the acid element which is indispensable to their solubility and absorption. The *tartrate of potass and iron*, while in the stomach, possesses no pre-eminence over other preparations; but when it arrives in the presence of the alkaline juices of the intestines, it is not decomposed; and as the acid which had given rise to its precipitation becomes united with the alkaline bases found in the intestines, it recovers the solubility it had temporarily lost, and becomes absorbable throughout the entire length of the canal, so that it is administered with equal success by the mouth or rectum. The efficacy of other preparations depends upon the quantity and acidity of the gastric fluids which may exist at the time of their ingestion; than which nothing is more variable.

After their absorption, martial preparations are only fitted to act upon the *cruor* in proportion as they are decomposable by the alkalis of the blood—the ferruginous salt and the albuminate of soda undergoing mutual decomposition, and a new salt of soda and albuminate of iron, the true basis of the *cruor*, resulting. This decomposition effected, we never find iron in the urine, for the oxide of iron then participates in the properties of the organic texture of the albuminous elements with which it is combined, and is prevented passing from the vessels. Certain salts of iron, as the cyanide of iron and potassium, &c., cannot undergo such decomposition by the alkaline substances of the blood, and then they are found entire in the urine. How does it happen, then, that the tartrate, which has the power of resisting the

action of energetic alkalis, is retained in the economy, and never found in the urine? It is because in proportion as the elements of the tartaric acid are transformed into other products by the oxygen of the blood, the oxide of iron is set free, and combines directly with the albuminous elements, so as to reconstitute the blood globule.

Therapeutical observation confirms the view derived from chemical considerations. Several of the older best known formulæ owed their efficacy to the presence of the tartrate; and the employment of the formulæ suggested by M. Mialhe, by some of the first practitioners, has completely confirmed his most sanguine anticipations. The tartrate is nearly tasteless, and can be borne by delicate stomachs when other martial preparations cannot, while it does not give rise to constipation or colic, as some of these do. If it produces diarrhœa, it is impure, as found in commerce, combined with cream of tartar, constituting, in fact, a purgative rather than a ferruginous preparation. To obtain it pure, the chemist should prepare it himself by acting upon cream of tartar, diluted by six or seven times its weight of water, with an excess of the hydrated peroxide of iron. When the saturation is complete, as recognised by the deep-red colour and sweetish taste of the liquor, the solution is filtered and evaporated.

The following are some of M. Mialhe's formulæ. *Pills*.—25 grammes of the tartrate are made with syrup into 100 pills, each thus containing 5 grains of the salt. *Syrup*.—16 parts of the tartrate are dissolved in as much distilled water of any kind, and when filtered, added to 500 parts of syrup. The syrup being so lightly charged with iron, is no-wise disagreeable to the taste, and is taken readily by children. *Lozenges*.—To 1000 parts of sugar, 30 of vanilla-sugar, and 10 of powdered tragacanth, 50 of the tartrate are added, made into a homogeneous paste with 100 of water, and dried, so as to form 1000 lozenges.—*Bulletin de Thérapeutique*, tom. xxxviii, pp. 529-34.

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*On the Sulphuret of Soda as a Depilatory.* By M. BOUDET.

M. Boudet states, as the result of numerous investigations, that the arsenic contained in the sulphuret which forms a part of so many depilatories, is not the essential ingredient, and that an innocuous substance, the *sulphuret of soda*, may be substituted with great advantage. He therefore proposes the following formula:—*sulphuret of sodium*, or *crystallized hydrosulphate of soda*, three parts; *quick lime*, in powder, ten; *starch*, eighteen. Moistened with a little water, this becomes so powerful a depilatory, that if removed, in one or two minutes, with a wooden spatula, it leaves the skin quite hairless. Independently of its use in removing abnormal villosity, it is of great service in preparing hairy parts for blisters or operations. It is applicable to the most delicate and irregular surfaces, and of whatever extent, the hair only reappearing after several days.—*Journ. de Pharmacie*, xviii, p. 119.

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*Abortive Power of Collodion on Smallpox.*

A case has recently occurred in the wards of M. Aran, at the Bon Secours, in which the good effect of collodion was proved to be as decisive in confluent smallpox as it had been found before in the more simple form. It occurred in the person of an unvaccinated young man, and the collodion was applied to all parts of the face but the lips and ears. Through this transparent covering the progress of the pustules was observed to become at once arrested, while those uncovered continued enlarging. Moreover, a part of the covering having been destroyed without being observed for some hours, the pustules thus exposed immediately began to develop themselves until again arrested by a reapplication. The ears, too, were now covered, and the progress of the pustules stopped there. In a few days the collection peeled off, the skin looking as after erysipelas, but no cicatrices were to be observed, though in other parts of the body they existed in abundance, the eruption having been very confluent.—*Bull. de Thérap.*, vol. xxxix, p. 369.

## FORENSIC MEDICINE AND TOXICOLOGY.

*Medico-Legal Researches on Dried Cerebral Substance.* By M. ORFILA and M. LASSAIGNE.

It having become of importance, on a recent trial, to decide whether a spot of dried matter found on the garment of a culprit consisted of cerebral substance, M. Orfila resolved to institute a series of experiments that might facilitate future decisions in similar cases; and the results, contained in the paper before us, constitute the first contribution he has made to the Transactions of the Académie de Médecine since his unjust dismissal from office by the ever memorable "Provisional Government." Its length precludes us from stating many of the details, but we may notice the chief conclusions the author arrives at. Dried cerebral matter is shown to be distinguishable by the peculiar reactions produced by sulphuric and muriatic acids, and by microscopical examination. All animal substances liable to be confounded with it were submitted to the action of these acids, and the difference of the colours induced are stated; but we will only quote the results which followed their application to the cerebral matter—observing, by the way, that some of the combinations of colour are anything but distinctively expressed by the terms employed.

*Concentrated sulphuric acid* quickly dissolves cerebral matter, producing a *violet colour*. A white precipitate results from the addition to the mixture of distilled water, liquid chlorine, alcohol, the nitrate of the protoxide of mercury, or the bichloride of mercury. The chloride of chrome furnishes a soft slate-coloured mass. The permanganate of potass destroys the colour immediately, and produces a white precipitate. The protochloride of tin gives a rose-coloured precipitate, the chloride of gold a greenish-gray, that of nickel a grass-green, of cobalt a wine-les, and that of platinum a yellow. The acetate of copper produces a blueish-white, and the sesquichloride of iron a yellow one. If the solution is neutralised with pure potassa, a notable quantity of white matter is deposited. Alcohol at 40° added to this deposit, boiled, and then evaporated, yields *an abundant yellow residue*; a similar residue being also obtainable by first evaporating the fluid decanted after the saturation with the potassa, and boiling the product in alcohol.

*Pure concentrated hydrochloric acid* does not dissolve ordinary brain, or dried brain which has again been rendered moist, nor *does it at once change colour*. In four or five days, if the mixture have *been open to the air*, it acquires a *dirty gray colour, slightly approaching violet*. About the twelfth day, a large portion of the matter is yet undissolved, while the fluid is turbid, resembling light-coloured Malaga wine, the colour being of a deeper Malaga red between the fourth and tenth days. If the mixture has been kept in a well-corked bottle, a greenish-gray mass is produced, the brain remaining undissolved a month or more. If it is heated in a small matrass, the liquid becomes turbid in a few minutes, and the fragments of brain acquire a dark violet colour. When it has cooled, the liquid assumes, in ten or twelve minutes, a lighter violet colour. If, in place of moist brain, we act with muriatic acid, in contact with the air, upon brain dried in the sun, the liquor becomes, in some hours, of a Malaga red, passing, in time, into a dirty violaceous-gray. If the brain has been gently dried at the fire, a turbid liquid of a whitish-gray, barely violaceous, is produced.

The *microscopical observations* have been instituted in company with M. Robin, the object being to ascertain whether *dried* cerebral matter was as easily distinguishable from other substances (being thoroughly moistened again, however, prior to examination) as it is in the recent state. The characteristic *cylinder-axes* were always discernible in it.

The following are M. Orfila's chief conclusions:—1. That, among all the human organs, there is no one that comports itself with sulphuric and hydrochloric acids as does the cerebral substance. 2. Among all soft organic substances capable of ad-



hering to clothes or instruments of destruction, so as to present a more or less projecting (as distinguished from the marks caused by fruits, acids, &c.) dried product, no one of them, if we have recourse to both acids, can be confounded with it. White of egg, and some of the soft cheeses, although furnishing results having some similarity, yet offer marked differences in their respective reactions. 3. For the detection of mere *traces* of cerebral matter, means having in view the demonstration of the presence of *phosphorus* are unavailable, inasmuch as the substance exists in so small a proportion in cerebral matter, and is also found in the form of phosphate, in white of egg, and caseum. 4. *Acetic acid* is of no utility in this investigation. 5. *It is possible*, then, to recognise dried cerebral matter by the aid of these acids. 6. The microscope, with a real magnifying power of 470 diameters, and especially with one of from 580 to 600, furnishes *a certain means of distinguishing cerebral substance from all known organic matters*, even when it scarcely amounts to a milligramme in weight. 7. Although it would be justifiable to *affirm* the existence of cerebral substance by the aid of its chemical characteristics, or the microscope alone, it is preferable to resort to both means of investigation. 8. By treating a mixture of cerebral matter and blood (such as might be found on an instrument with which the cranium had been fractured) with a concentrated solution of sulphate of soda, the globules of the blood are preserved, and both they and the cerebral matter can be detected by the microscope.—*Annales d'Hygiène*, tom. xlv., pp. 142-190.

Since the publication of M. Orfila's essay, M. Lassaigne has published some papers upon the same subject. He observes that Vauquelin discovered that cerebral and nervous substance furnished phosphoric acid by calcination, proceeding from the combustion of the small quantity of phosphorus found in the fatty matters of these tissues; and numerous experiments have convinced him that this procedure may be advantageously had recourse to, for the detection of the true nature of spots of dried cerebral matter.

If a small portion of this be calcined in a platinum capsule or plate, while exposed to the air, an acid "coal" is produced, which distinctly reddens litmus; and water in which it has been washed, precipitates lime water in white flakes. A portion no larger than a millet-seed suffices to show this. It suffices to slowly carbonize the matter, by gradually heating it at a spirit lamp (avoiding as much as possible the combustion of the inflammable gases that are disengaged), and to expose the carbon when formed, to a dull red heat. The same procedure observed with other organic tissues of mammifera furnishes a neutral or alkaline "coal."

In a second paper M. Lassaigne observes, that differences in the reactions thus produced are observable in the white and gray matter, when these are obtained in as separate a condition as possible; that of the *white* being constantly acid, and that of the *gray* sensibly alkaline. Examining the nerves in the principal mammifera, he has found the coal produced alkaline, while that resulting from the spinal marrow has been acid. In two birds, the goose and the turkey, he has found both the gray and white matter alkaline; which he accounts for by the difficulty of effectually separating the two in such small brains.—*Journal de Chimie Méd.*, 1850, Nos. ix and xi.

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*On the Elimination of Arsenic in Arsenical Cachexia.* By M. HANNON.

By arsenical cachexia is meant that special deterioration of the economy, which those who have absorbed into their system a certain quantity of metal undergo, and which is of more frequent occurrence than is supposed. The health of many workmen in the trades in which arsenic is employed, is thus injured, as is that of children by sweetmeats coloured by arsenite of copper. So, too, after the injurious employment of arsenic in medicine, and the treatment of cases of poisoning by emetics and antidotes, the patient is never cured until the arsenic has been expelled from the economy. It will remain in the system until it has been rendered soluble by combination with some body, and thus eliminated. M. Hannon believes that *sal ammoniac* effects its elimination, as completely as the iodide of potassium has been

shown by Melsens to eliminate lead and mercury. He believes, too, that arsenic may be prevented from accumulating in the economy by combining this substance with it.—*Revue Médicale*, 1850, vol. i, p. 724.

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*On Poisoning by Oxide of Zinc.* By MM. FLANDIN and BOUVIER.

M. Flandin recently read a paper at the Académie des Sciences (May 6), in which he relates some experiments he had performed with regard to the safety with which sulphate of lead and oxide of zinc might be employed in manufactures, as compared with the carbonate of lead. The *sulphate of lead*, as being insoluble, was supposed to be less easily decomposable in the economy than the carbonate. He shaved a dog, and rubbed into its body four or five grammes of ointment, formed of equal parts of the sulphate and lard, every morning. Before the tenth day the animal was in a state of suffering, became constipated, refused to eat, and grew thin; and it died on the twenty-second day, two ounces of the sulphate having been employed. Chemical analysis detected lead in all the organs, and especially the liver. Very soon after the experiment, the foreman of the establishment became attacked with colic, and died.

A similar experiment was performed with *oxide of zinc*, and the animal continued perfectly well after thirty days' employment of the ointment, increasing in bulk in consequence of the good diet with which it was also supplied, more than two ounces of the oxide having been rubbed in. After ten days' rest, frictions were now commenced with the carbonate of lead; and in ten days symptoms of poisoning were evident, the animal dying on the twenty-third day, when sixty grammes had been employed.

The conclusion M. Flandin arrived at was, that the zinc compounds do not act perniciously on the animal economy as those of lead do; but at the next sitting of the Academy, M. Bouvier communicated the following case of zinc colic he had met with at the Beaujon Hospital. A cooper, æt. 42, was admitted, 19th April, with all the symptoms of metallic colic. He stated, that he had been employed, with other workmen, since the 4th of the month, in barrelling up white zinc at a manufactory, and that, after a week, they had had old barrels to repair, which kept them constantly in a pulverulent atmosphere. They soon began to feel colicky pains, and a repugnance for food, while the wine and brandy they took to stimulate the appetite did not remove a disagreeable pasty taste from the mouth. Thus one of them was seized with the most violent colic pains and constipation, and after enduring horrid suffering for five days, applied to the hospital. When he came in, he suffered from intense pain, bilious vomiting, constipation of five days' duration, and deprivation of sleep; the tongue was whitish, and the abdomen natural, and no fever was present. Being treated as for lead colic, copious stools, cessation of vomiting, and diminution of the pain resulted; and by the aid of the continuance of purgatives and opiates, he was sufficiently recovered to leave the hospital on the 4th of May. Two days before he left, the surface of his body was carefully washed, and the fluid chemically examined. It was found to contain neither lead, arsenic, nor copper, but an appreciable quantity of zinc.—*Gazette Médicale*, Nos. 19 and 20.

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*On the Health of Workmen employed on Copper.* By MM. CHEVALIER and BOYS DE LOURY.

THIS is an elaborate and interesting memoir, detailing the results of the investigations which its authors have been for some time carrying on, in order to ascertain whether colic and other affections are incident to those employed in the various branches of copper-manufacture. They found the greatest discrepancy prevailing in the testimony both of writers, and of persons themselves engaged in the occupations; and therefore entered themselves into personal examination of several establishments. They quote the writings of several authors in favour of the existence of a copper-colic, but many of these had evidently mistaken lead-colic for this disease, and others had stated their opinions upon mere hearsay.

The most recent advocate of the ill-effects of copper is Dr. Blandet, who, in 1846, declared to the *Académie* that thousands of the copper-workers in Paris suffered; but, on examining the hospital registers since 1839, only eighteen cases of colic referred to copper were found. M. Requin, physician to the Hotel-Dieu-Annexe, where great numbers of this class of persons apply, has never met with any symptom especially produced by this metal, but he has often observed disease assigned to copper, which really arose from lead or other metals. M. Sandras, physician to the same hospital, and who has long paid especial attention to the subject, says that he has never met with but two cases of colic which he could attribute to copper. M. Vasseur, physician for ten years to the benefit society of workers in copper, has never met with a case. M. Norvet, physician to the bronze workers for seven years, has only met with two. M. Derickx, director of the Mint, has never known any ill-effects result to the workmen there. M. Piedoye, however, physician to the copper works at Villedieu-les Poëles, in Normandy, declares that colic is sometimes met with there, though far less frequently than Desbois declared it to be in the last century.

In their personal investigations, the authors visited some of the largest manufactories, and, amongst other places, the arsenal, where the copper percussion-caps are made. These visits, and numerous communications they received from places they did not visit, have led them to the conclusion, that copper, when uncombined with lead, arsenic, &c., is quite innocuous to the workmen engaged upon it, and this, whether worked in its solid state, or filling the places with its emanations; the absorption of the substance being amply proved, however, by its discharge with the urine, and its colouring both the bones and hair of the workmen. They frequently had accounts of very various symptoms produced by it; but, upon investigation, they were disposed to refer these to the bad hygienic conditions of many of the places of work, the laborious character of some branches of the occupation, and the dissipated conduct of many of the persons employed. In the manufactories in which suitable workshops were provided, and a wholesome discipline exercised over those employed, no complaints of disease from copper prevailed. The subject is an important one, for there are many thousand persons engaged in the copper-manufactures in Paris alone, and it is a branch of business rapidly on the increase. The authors believe that hygienic precautions are alone called for. Of course, these observations apply solely to metallic copper, and do not refer to the effects produced by the oxides and salts of the metal, which they intend to consider in another essay.

The conclusions arrived at by MM. Chevallier and Boys de Loury are, however, far from being universally received; and M. Robiquet, physician to the copper-mines at Gibet-Ardenne, agrees with MM. Tanquerel and Blandet, in believing that copper does produce characteristic symptoms distinct from those of lead, and this when it is separated from all other metals. There are usually present dyspnoea, dryness of the air passages, a small filiform pulse, absence of transpiration, scarcity of urine, cephalalgia, and torpor; but the characteristic symptoms are cardialgia, with epigastric tenderness, and a neuralgic pain in the rectum; pruritus ani and constipation usually also being present. The best treatment consists in first purging by sulphate of soda, administering emollient and diaphoretic drinks, and on the third day giving opiates.—*Annales d'Hygiène*, vol. xliii, pp. 337-73; and vol. xlv, pp. 27-48; and *Revue Médico-Chirurgicale*, vol. viii, p. 38.

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Contributions to the History, Diagnosis, and Treatment of Croup. By John Ware, M.D. Boston (N.E.), 1850. 8vo, pp. 29.

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Researches upon the Necropolis of New Orleans, with brief allusions to its Vital Arithmetic. By Bennet Dowler, M.D. New Orleans, 1850. 8vo, pp. 30.

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PART FIRST.  
*Analytical and Critical Reviews.*

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ART. I.

1. *Operative Surgery.* By FREDERICK C. SKEY, F.R.S.—*London*, 1850. 8vo, pp. 709. With numerous Illustrations on Wood.
  2. *Beiträge zur praktischen Chirurgie. Nebst einem Bericht über die chirurgisch-ärztliche Klinik der Königlichen Universität zu Halle.* Herausgegeben von dem Direktor der genannten Anstalt, Dr. ERNST BLASIUS, Professor der Chirurgie.—*Berlin*, 1848.
- Contributions to Practical Surgery; with an Account of the Clinique of Ophthalmic Surgery at the Royal University of Halle.* By Dr. E. BLASIUS, Director of the Clinique, and Professor of Surgery.—*Berlin*, 1848. 8vo, pp. 314.

It is not a little singular, that Mr. Skey's should be the first systematic work on Operative Surgery offered to the profession by a London surgeon. Yet it is no less true than singular. Mr. Liston and Mr. Fergusson cannot be considered as surgeons of the London school. Their works represent the traditions of the Edinburgh class-rooms, the practice of the great Northern Infirmary. The lectures of Sir Astley Cooper and Abernethy are sketchy outlines of principles and practice, without the pretension of being anything like complete treatises on Operative Surgery. Samuel Cooper's books, again, are intended to be what they really are, compiled text-books for the student, embracing nearly the whole domain of surgical science and art, operative surgery comprising a comparatively small part of their total contents. The 'Cyclopædia of Practical Surgery' has never been completed, and the parts published are not largely contributed to by London surgeons. We have, in truth, no complete work on Operative Surgery from any one brought up in the school of Hunter, Cheselden, Home, Hawkins, and Brodie; of Astley Cooper, Tyrrell, Key, and Travers, of Blizard and his predecessors at the London Hospital; nor of Pott, Abernethy, Earle, Lawrence, and Stanley, until Mr. Skey produced the book before us. We presume from the title, that he intends it to be



considered as a complete systematic treatise on Operative Surgery; but if the German and French models of Dieffenbach and Velpeau are to be regarded as types of what such a treatise should be,—and we think, that to a great extent they are so, in arrangement at least,—then Mr. Skey can only be said to have produced a series of practical dissertations upon wounds, dislocations, and fractures; aneurism and injuries to arteries; amputations and excisions; hernia; lithotomy; and operations on the eye; with scattered remarks upon tumours, ovariectomy, and a few special operations. No work on Operative Surgery can at present be considered complete, without full directions for the performance of all operations for the restoration of lost parts and the cure of deformity; yet this is one of the most scanty sections of Mr. Skey's book. He says very little of tenotomy and myotomy; and nothing, or what is almost equivalent to nothing, of plastic operations on the lips or urethra, or of those undertaken for the cure of recto-vaginal or vesico-vaginal fistula, salivary fistula, prolapsus uteri, prolapsus ani, or preternatural anus. He says nothing of operations on the teeth, perhaps the most common a country surgeon has to perform. Nothing of catheterism of the Eustachian tube, œsophagotomy, or excision of the neck of the uterus. His chapter on the Extraction of Foreign Bodies is very scanty, and the minor operations of cupping, torsion, and transfusion, have been entirely overlooked. No very evident arrangement is followed. For instance, we have a chapter on Autoplastic Operations, but the rhinoplastic operation and staphyloraphy are in another place. We have another on the operations performed on the Head, Face, and Neck, from which operations on the Eye are excluded.

We have not said all this by way of finding fault, but simply to show that the book is not what one would expect from the title. The fault may be our own; and we may have supposed that the author implied more than he really intends to do, by the somewhat vague and comprehensive designation which he has adopted for his treatise.

In an analysis of this work, we do not purpose to comment very largely upon the successive chapters; but shall lay before our readers such extracts, as will indicate some of the opinions and practices which appear more or less peculiar to the author. Upon one or two points, however, it will be necessary to depart, in some degree, from this rule.

The chapter on Dislocations is one of the best in the book; and Mr. Skey's experience of accidents has been so very large, that his opinions are entitled to great attention. We quite agree with him in the following general principles to be observed in attempting to reduce dislocations:

“The affected bone is dislocated from its socket, generally, in consequence of the violence of a blow it has experienced. It remains dislocated in consequence of the tension of the muscles inserted into it, which become the subject of a passive contraction. This state continues, so long as the limb remains undisturbed by the efforts of the surgeon to reduce it. But no sooner is the attempt made to elongate the limb, than the same muscles, one and all, start into sudden and positive contraction, as though for the express purpose of preventing the descent of the bone. In this unequal struggle, they contend against the yet more powerful agents of traction by pulleys, yielding their ground inch by inch, till the bone is restored to its normal position, when they retire from the contest, and instantly resume their former state of repose. In the case of the dislocation of a large bone, the power exerted by the muscles, in order to retain it in its false position, is enormous, as may be readily inferred from the force employed to counteract them; and this force

is also continuous throughout the whole period of the attempt at reduction; their contractile power becoming probably greater as the bone descends, and approaches the range of the ordinary contractility of its muscles.

"It does not appear very reasonable to suppose, that a force like that employed to reduce a bone, fixed by some of the largest muscles in the body, and often reaching that of several hundred weight, could be diverted from its straight line to any useful purpose, by the mechanical pressure of the hand, or even rotated, by the unassisted effort of one, or even of two, persons. Yet surgeons often take credit to themselves for reducing the bone by such manœuvres; such as rotating the femur inwards, after extension of the limb, for the purpose of raising the head of the bone into its socket, or raising the head of the humerus in like manner, in dislocation of that bone downwards, into the axilla. An experience of twenty-five years of every variety of dislocation, in St. Bartholomew's Hospital and elsewhere, long since taught me the futility of such attempts, and I am strongly of opinion, that the apparent success which occasionally, *though rarely, attends them*, is due to accident alone. If the head of the femur, for example, be drawn down insufficiently, so that it rests on the outer margin of the brim of the acetabulum, it cannot be pushed over this margin into the socket by any act of manipulation, for additional extension is still required; whereas, if it has reached the requisite point, it will be instantly reduced by its own muscles. ....

"In the endeavour to reduce a dislocation, the line of traction should hold reference, less to that of the socket, or surface from which the bone has been displaced, than to the more important purpose of easing it from the surface, on which it has lodged. For example, the rim of the glenoid cavity in dislocation of the humerus, presents an obstacle to the extension of the bone in the immediate line of that cavity; but if the bone be drawn off it, by extension made in any oblique direction, the instant this ridge is passed, the head will rush back into its natural cavity. So, also, in dislocations of the femur on the dorsum ilii, we do not attempt to draw the bone in a direct line with the acetabulum, but we carry it below, round its back and elevated margin, and no sooner does it reach the lower part of the rim, which is much less prominent than the upper and back part, than the muscles immediately restore it to its socket. The same rule holds in dislocation of the ulna and radius backwards at the elbow-joint. I believe the exact line of extension to be much more readily determined, and in truth a less important subject of consideration, than it is generally deemed. I believe, that if we bring the bone sufficiently downwards and place it in the *neighbourhood* of the articulation, the muscles will replace it with as much ease, as that which originally dislocated it.

"The bone appears, as it were, sucked violently into the socket, even at the instant of its sustaining the greatest force of extension. Then is it, that the muscles acting with one accord, set at nought the extending power, and complete the work of reduction, in defiance of all the agents employed at the moment to prevent it. I consider that the muscles are the immediate agents of reduction, and not the surgeon, whose entire duty consists, in placing the bone in a position, to give them the opportunity of displaying this harmony of action, and of exercising a power, even beyond that of the mechanical agents of extension. It is this power that succeeds in forcibly drawing backwards the head of the femur into its cavity, when it has fairly reached the rim of the acetabulum, notwithstanding the force employed at that instant, in extending it. In the examples of the larger dislocations, I place no reliance on any of the above-mentioned efforts of manipulation, but depend, almost entirely, on the act of simple extension, in the fullest confidence of the disposition of the joint to right itself if the obstacles be removed." (pp. 60-63.)

The following remarks upon the reduction of dislocation of the humerus into the axilla, are also well worthy of attention :

"With respect to the plan of placing the heel in the axilla, it should be constantly kept in mind, that the shoulder-joint has this peculiarity in its construction,

viz., that it is composed of two bones, each of which is moveable on the trunk, and that, in the act of extension, we not only affect the position of the bone we are extending, but, through the agency of the muscles connecting them, we also affect the relations of the bone we are extending *from*; and in the case of the humerus and scapula, that the agents of extension do affect both bones, cannot be doubted. By fixing the scapula, we retain the glenoid cavity in a direction looking straight outwards from the trunk; but if we leave the scapula untouched, then, by the act of drawing down the humerus, we depress at the same time the acromion, by means of the deltoid which is attached to both. The consequence is, that in depressing the acromion we *direct the socket of the bone towards the dislocated head*, and readily get rid of the obstacle, which is often immediately caused by the rim of the glenoid cavity alone, and we bring the two surfaces of bone within the range of what I may term their natural affinity, and by the agency of which they immediately coalesce. With the heel in the axilla, we make a simple extension of the arm from the wrist; we leave the scapula unfettered, to assume its own direction; and by the extending force, slightly as it is applied by one person only, *the scapula is made to revolve on its centre, through the leverage exercised on the acromion by the deltoid*; and the pectoralis major and latissimus dorsi, being thus relaxed, exercised no prejudicial influence on the movements of the humerus. I have seen the protracted efforts of pullies, drawing outwards, fail again and again, with the utmost effort, when the head of the bone has been restored to the glenoid cavity, by the single agency of one person subsequently placing his heel in the axilla.

“There is no reason why, in very muscular subjects, or in old dislocations, the same principle may not be applied conjointly with the use of pullies. For the purpose of retaining this admirable, because most efficient, principle, I employ a well-padded iron knob, which may represent the heel, from which there extend laterally two strong straight branches of the same metal, each ending in a bulb or ring of about four inches in length, the office of which is designed to keep the margins of the axilla as free from pressure as possible; for what operation can be more absurd than the endeavour to fix the scapula by means of girds carried round the shoulder, in which we include the margins of this cavity, viz., the pectoralis major and latissimus dorsi, as in a vice, and by which we endeavour to drag up the arm, *at the same instant that we make the most strenuous efforts to drag it down by a yet superior force?*” (pp. 86-8.)

Our own observation, again, is quite in accordance with that of the author, when he asserts, that no written description gives an account of all the various positions assumed by the dislocated femur, and that it is very rarely that any case bears out fairly the written descriptions. Speaking of the ordinary classification of these dislocations, he says:

“By the above division we exclude altogether by name a variety of dislocation of the femur, which is really of no unfrequent occurrence, viz., dislocation on the dorsum acetabuli, and which is nowhere alluded to by authors; also, the fact is not stated, that the head of the bone may occupy the neutral ground *between* the surfaces of the acetabulum and ilium. In truth, observation has taught me that external force may displace the head of the femur in all directions, and in all degrees of distance from its socket; and that no man, who is not familiar with the osteology of this region, can tell its exact position. Practically, I do not assert this knowledge to be so important as it may appear, for we do not necessarily qualify our remedial agents by the existence of these modifications, and yet we are hardly justified in recording the position of a bone, which is in fact untrue.

“I have seen repeated examples of the following varieties—upwards, on the dorsum ilii, varying in distance from the socket from one to three inches; backwards, in the ischiatic notch, on a level with the socket, and an inch above its centre; on the dorsum acetabuli, both backwards, and upwards and backwards; inwards and slightly downwards over the obturator foramen; yet further inwards,

on the ramus of the os pubis; inwards, and slightly upwards on the body of the os pubis, below the spine and transverse part of the bone; and straight upwards, nearly in front of the articulation." (pp. 106-7.)

The whole of this chapter will prove both interesting and instructive to practical men.—The following remarks on the diagnosis of Aneurism are likely to be useful:

"It is the duty of the surgeon to examine the sac carefully, and to ascertain the nature as well as the quantity of its contents. If firm and incompressible, and if painful on pressure, the contents will consist of coagulum. If soft and dilated in size, the proportion of coagulum will be small. If the surgeon now apply one hand on the artery, above the disease, and press with sufficient force to arrest the circulation, and place the other on the tumour, on which he should exercise gentle pressure, he will empty the sac of its fluid contents. Thus he will be able to judge, by the quantity of solid coagulum left behind, about what is the proportion of each. The excess of one, over the other state of the blood, depends greatly on the form and size of the opening in the artery leading into the sac. If the opening be large, the contents will be mostly fluid, and this is readily tested by observing the length of time required to fill the sac. If the pressure on the artery above be suddenly remitted, after having been continued sufficiently long to empty the sac of its fluid contents, while the other hand is placed on the tumour, we shall be enabled to ascertain with some precision how many pulsations of the artery are required to fill the sac, and the rapidity with which the cavity is redistended would lead to the opinion that nearly all the blood takes this direction. I have known the short period of six or seven seconds sufficient to distend a large aneurismal tumour in the ham. The nature of the contents of the sac may also be inferred, with some approach to truth, by the character of the pulsations, which are more distinct, both to the sense of sight and of touch, when the contents are solid. One singular feature in the case I have just alluded to, is worth further mention. In the year 1842, a man was brought into St. Bartholomew's Hospital, with a large tumour in the right ham. Opinions were very much divided as to its nature. Some deemed it aneurismal; the majority, not. The tumour entirely obliterated the popliteal cavity, and extended upwards between the flexor muscles of the thigh, half way between the knee and hip-joints. The pulsations were scarcely perceptible. I examined the case very carefully, and fully convinced my own mind, that it was a case of popliteal aneurism, of a nature which I can only express by saying, it was exceedingly diffused for a circumscribed aneurism. That the blood had escaped from even the dilated coats of the vessel I felt confident, from the fact, that the tumour extended many inches higher than the point of entrance of the artery into the popliteal space. On the following day I examined this case, in company with my late friend Mr. Langstaff. After a full inquiry, he said he considered it a malignant disease, and was leaving the ward, when I requested him to renew his examination. I then introduced my hand under the bedclothes, and placed my thumb on the iliac artery, begging him once more to place his hand on the tumour. I then compressed the vessel unnoticed by him, when he exclaimed, with infinite surprise, 'Why, the tumour is going away! It's gone away!' Remitting the pressure, the tumour again filled with rapidity, when his expressions of astonishment were still greater. I then explained the mystery, and he left the ward with the fullest conviction that the malignant disease was a case of aneurism. And so it proved. The man was operated on and recovered. On another occasion I applied the same test to a case of doubted aneurism of the brachial artery, in the presence of several medical men, most of whom were previously sceptical as to its nature." (pp. 181-3.)

Mr. Skey's method of making the incision for the exposure of arteries before ligature is somewhat peculiar, and he certainly obtains some advantage by it; but we must allow him to speak for himself:

"I almost invariably adopt an *oblique incision*, and generally at an angle with the artery of  $45^{\circ}$ , on the following grounds: that in fat subjects it is difficult to ascertain the exact line of the vessel, and that, however true the first incision may be, it does not follow, in the course of a slow and bloody operation, that the same line will be preserved; that if, from accidental circumstances, the precise position of the artery be lost, the operator is equally uncertain whether he is dissecting on the inner or on the outer side of the vessel, or upon it; that, by dividing across the direction of the vessel, he acquires a confidence, from the conviction that the artery is really under his knife; and, lastly, that he makes an external wound, within which the ligature needle is more readily carried round the artery in a fat subject, in which the vessel lies deep, than in a wound parallel to it. The objection to a directly transverse wound is, that the artery is exposed only in a transverse line, and there is nothing gained, but much may be lost. In the femoral, brachial, radial, and ulnar, and posterior tibial, I consider the oblique incision to be an important element of success in finding the artery with facility. Take the example of the posterior tibial artery, how preferable is it, to make an external incision over this deeply-seated vessel across the line formed between the muscles that surround it, to the endeavour to hit the exact line, where the deviation of one eighth of an inch would carry the surgeon into the substance of either of two muscles, neither of which he would be able to distinguish from the other! I remember, many years since, to have seen a practised operator totally fail in his attempt to tie the ulnar artery immediately above the wrist, from this difficulty." (pp. 188-9.)

**Exposure of bleeding surfaces to air, as a means of arresting Hæmorrhage, is strongly recommended, and with reason:**

"In cases of hæmorrhage from small vessels, there is, in my experience, no agent at all comparable in its power to arrest bleeding, like rest and exposure to air. Whether it be the act of exposure to the atmosphere, or that the vessels are freed from the contact of parts around, I do not know, but the fact is undoubted. The surgeon who has least fear of hæmorrhage loses the least blood. A small wound may be tortured by styptics, and by compression, and by other unprofitable agents, till it becomes the fruitful source of protracted hæmorrhage. Masses of lint are piled up in heaps upon the wound, pressure is maintained till all the parties concerned are exhausted, but still the hæmorrhage returns or continues, by reason of the irritation caused by the very agents employed, and nothing more. Under these circumstances, which I have repeatedly borne witness to, all dressings should be removed; the wound should be opened and exposed to the air, by its edges being drawn widely asunder, and the bleeding apparently encouraged; its surface freely sponged with cold water, the coagula wiped away, and in this condition it may be fearlessly left to bleed. The cessation of the hæmorrhage by such means is often immediate. I was called up at an early hour, many years since, by a medical man, to consult with him on a case of hæmorrhage from a tonsil which he had removed in the middle of the previous day from the fauces of a young lady. I directed him to take her to an open window, on the side of the house from which the wind blew, and to require her to keep her mouth wide open. On the following day, I learnt that the bleeding, by this simple expedient, had almost immediately ceased. A gentleman, in a state of intoxication, fell down and cut his head. The wound bled freely from the secondary branches of the temporal artery. This hæmorrhage continued from seven till twelve at night. When I saw him, his bed was surrounded by some six persons, medical and domestic. The man had a pile of lint of about three inches in thickness on the wound, and another layer was about to be added to the mass. Contrary almost to the entreaty of the parties engaged, I removed the clotted and saturated lint, and roughly sponged the wound, which I exposed to the air. The bleeding ceased within three minutes. On another occasion, I was sent for in the night to the hospital, to assist in arresting the bleeding from a large wound below the axilla, that had been made on the previous day, by my late colleague Mr. Earle. Three house-surgeons were present and other persons. This man had lost



a large quantity of blood by the same ingenious devices as those practised in the above cases. I proposed to remove the lint, and I was answered that its removal would be fatal, such was the tendency to bleed. I ordered the man's bed to be carried to an open window, and removed every appendage from the wound. A large flow of blood followed, and then entirely ceased. I have records of many other similar cases." (pp. 204-5.)

We give the following extract upon the cure of Varicose Veins by the formation of small eschars, not to recommend it, but simply to inform our readers that Mr. Skey has tested it for many years, and adopted it largely both in hospital and private practice, considering it both "efficient and safe." Our own observation has induced us to trust more to pressure, as a palliative, than to place confidence in any supposed method of radical cure. If caustics be used, however, their application, probably, could not be more safe and effective than in the manner described by Mr. Skey :

"Any number of eschars may be made on the diseased veins; I have made as many as nine on one leg at a time, selecting for their application the most prominent parts of the disease upon which the eschar is to be made. The size of these eschars, regulated by that of the aperture made in three or four layers of good adhesive plaster, is of great moment. They cannot well be made too small. I formerly cut an opening of the size of a shilling; but this involved the surrounding skin beyond the margin of the vein, and an ulcer of unnecessary magnitude followed, that was difficult to heal; for in all these cases, consequent on the peculiarity of constitution I have alluded to, the healing action is exceedingly slow. Those that I now employ are less than a fourth part of that size, sometimes but little larger than the diameter of a split-pea, the number of which may be regulated by the extent and complication of the disease. The smallest size is sufficient at once to obliterate the vein. Whatever number be made, the entire operation should be completed at once. The region of the ankle may require two, three, or more, placed over the early branches of the vein. One or two may be applied on the trunk of the vein up the calf, and others below the knee. I have made these eschars also above the knee, but not frequently, not because there is any danger attending it, but because the diseased condition of the vein does not appear to require it. When the places for the application of the caustic have been selected, and the plasters firmly fixed, the ingredients of the paste should be mixed and pasted on the aperture within them. A piece of plaster should be laid over each quantity, and the whole may be removed in from twenty minutes to half an hour. It will then be found that the veins are obliterated, in fact, they have disappeared. Rest in the horizontal posture is desirable, but not essential. In the course of a week or ten days, in a moderately vigorous circulation, the eschars will separate, and the process of healing should then be vigorously pushed, by the aid of bark, wine, good diet, and the local application of adhesive plaster, and stimulants if necessary. Occasionally, the ulcers heal very torpidly, and may occupy many weeks." (pp. 301-2.)

In cases where a joint has been destroyed, suppuration established within its cavity, the ligaments separated from the bone, the cartilage partially or wholly absorbed, and the ends of the bone grating against each other, Mr. Skey still defers amputation until the effect of a free incision into the cavity of the joint has been observed. He has followed this practice since 1838, and on "sundry occasions" has saved a limb that would otherwise have been removed.

The author appears to prefer circular to flap operations. We are disposed to agree with him as a general rule, and believe that most surgeons of much experience are returning to the circular method; but we entirely differ from him in the opinion, that the flap operation is applicable in the

thigh and upper arm, but inapplicable when we have a double bone. (p. 319.) Indeed, the best and firmest stumps we have ever seen, many years after amputation, have been the result of Hey's operation below the knee.

The following remarks upon tourniquets are of importance. It is really absurd that the barbarous contrivance used before the discovery of the circulation of the blood, should still be in common use :

"The tourniquet which I have substituted is composed of two semicircles, one of which fits into the other by running in a groove. Each half is fixed by a spring-catch to the other, and may be enlarged or reduced at will, to any size required for the thigh or upper arm. When required for application to the thigh, the circle, which is made to open, to admit of its application around the limb, is drawn out to its fullest size. In the centre of each semicircle is the pad for pressure and counter-pressure, the former being provided with the ordinary screw. The pads are made small, in order to include as little surface in the pressure as is compatible with the safe application of the instrument. When employed for a lesser limb,—the arm, for example, or the thigh of a child,—the circle is lessened to the required size by raising the lateral springs, and pressing the outer half or semicircle downwards upon the inner one, by which the large circle is converted into one of smaller size, the alteration being obtained by the introduction of two hinges in each half of the instrument." (pp. 327-8.)

Mr. Skey opposes Mr. Liston's rule, of not sawing the thigh bone below its middle. We quote his opinions, but do not answer either for his physiology or practice :

"The evidence of recovery from amputation above and below knee, is so conclusive in favour of the latter expedient, that it should always be resorted to, if practicable ; while, with regard to the thigh, we have the additional motive which applies to the future utility of the limb. To understand this principle, we have but to observe the different gait of a person whose thigh has been amputated immediately above the knee, and who has retained at least three fourths of the bone, from that of another, in whom amputation has been performed at the middle or junction of the upper with the middle third. We cannot but be struck with the easy natural movements of the one, when contrasted with the lateral writhings and contorted efforts at progression employed by the other. It has been deemed sufficient for the purpose of carrying the thigh forwards in the act of stepping, that the psoas and iliacus muscles, as the agents of extending the limb forwards, should be alone retained ; but this general opinion is an erroneous one, *these* muscles not being even concerned in that movement. The act of flexion of the thigh is effected solely by the rectus femoris, of which as large a portion as possible should be preserved, to continue its former function. The division of the muscle does not preclude the retention of its office. It acquires a sufficient adhesion to the material of the stump to answer every useful purpose, as an agent of flexion of the thigh on the pelvis, although that of extension of the leg be destroyed. In the act of walking, the thigh-bone forms the radius of a circle, and the longer the radius, the longer the step taken in progression, the more equal the movements of the two limbs ; and, finally, the longer the portion of the thigh retained, the greater the facilities of applying the material for artificial support. It is very true that it is of no moment whether the radius consists of a living thigh and leg, or of dead wood or other material ; but it is of every import, that the rectus muscle, which is the sole agent by which the leg is put forwards in stepping, in what is called flexion of the thigh, should be retained of as great length as possible, that its action may be free, and sufficient to the required purpose. It is surprising that a surgeon of the eminence of the late Mr. Liston should have counselled the necessity of not sawing the bone below its middle, 'because a long stump is inconvenient to a person in any walk of life.' " (pp. 360-1.)

The following method of treating Bursal Tumours is worthy of further trial, although, in our own practice, we prefer laying the cavity open by free incision :

“ I may assert with truth, that I have treated at least a hundred cases with success, and generally without being attended with the loss of more than a few days’ work to the subject of the operation.

“ The principle of this treatment consists in promoting suppurative action in the sac, and of converting the torpid growth into an abscess. This is effected by passing a single thread through the body of the tumour, and through the cavity in its centre, if the tumour belong to the hard and most chronic forms. My directions, when given to hospital patients, are to require the thread to be withdrawn at any time, should pain or inflammation arise. The general average of time requisite is about four or five days, when a blush of inflammation appears about the orifices of the thread. If the thread be allowed to remain more than a day or two after this period, if the instructions be neglected, or if the patient indulge in violent exercise or active motion, the inflammation will often extend and assume the character of erysipelas, and an attack of illness is inevitable.

“ Unless the patient be seen each day, the order to remove the threads on the occurrence of pain, &c., should be imperative. This application converts the fluid containing cyst early, and the hard, fibrous tumour somewhat later, into an abscess. The abscess is opened, or bursts if neglected, and the cavity and substance altogether are obliterated.” (p. 417.)

Among the operations on the head, face, and neck, Mr. Skey describes, at some length, the means of uniting palatine fissures—Staphyloraphy or velo-synthesis. His observations on the anatomy of the palate, however, refer to the healthy condition only, without any account of Mr. Fergusson’s investigations into the anatomy of cleft palate. Accordingly we find the approximation of the back parts of the fissure, observed during the act of swallowing, characterized as a movement which “ cannot be satisfactorily explained on any ordinary principle of muscular action,” (p. 437); whereas Mr. Fergusson has distinctly shown, that the cause of the movement is contraction of the superior constrictor of the pharynx and of the upper portion of the middle constrictor.

Mr. Skey also thinks lightly of what we are disposed to regard as one of the most convincing proofs of the benefit of minute anatomical research to the practical surgeon, and by far the most important improvement in plastic surgery which has been made by any British surgeon,—we mean Mr. Fergusson’s demonstration of the action of the levator palati in cleft palate, and his modification of previous methods of performing staphyloraphy. Mr. Skey says :

“ In order to obviate the tension consequent on drawing into contact the two opposite sides of the cleft palate, it is necessary to resort to a valuable expedient, first generally adopted by Dieffenbach, of making a lateral incision through the substance of the palate, on each side of the central line. These incisions should be made without reference to anatomy, and should involve the entire structure, whether muscles, cellular tissue, or mucous membrane, for all are equally in a state of tension. But if one muscle rather than another be involved in this tension, it is obviously the tensor palati, the fibres of which run transversely inwards towards the centre. Mr. Fergusson, in a paper published in the ‘ *Medico-Chirurgical Transactions*,’ has referred this necessity to the action of the levator palati, and no doubt this muscle is involved, in common with the tensor, in the lateral tension; but it cannot be, anatomically speaking, so closely involved as that muscle, nor can the same relief be afforded by its division as is obtained by a free incision through the body of the palate, as far forwards as the bony arch itself; and this

incision would include most especially, or my anatomy is greatly at fault, the tensor palati muscle. I have never divided otherwise than in accordance with the above rules." (p. 439.)

Now Mr. Skey does not inform us in how many cases he has performed this operation, nor what has been his proportion of successful results. He publishes no account of his cases, and leads one, therefore, to suspect that the proceedings he has followed have not been generally very satisfactory. In the practice of other very able surgeons, these extensive lateral incisions "made without reference to anatomy" have not encouraged imitation; whereas Mr. Fergusson, in 1849, had published twenty-four cases, in which his division of the levator palati had been performed; and of these success followed in twenty-one, although many of them were unfavorable for operation, and in three the ordinary methods had already failed. Had Mr. Skey seen the preparation by Mr. Fergusson, which most surgeons of this metropolis have had the opportunity of examining, he certainly never could have asserted that the tensor palati has more influence than the levator in separating the edges of the cleft, and producing tension and separation of the newly united parts.

Mr. Skey's remarks upon the after-management of these cases may prove useful:

"I feel it requisite, in the absence of any published knowledge on the subject in possession of the public, to say a few words on the after-management of these cases. The treatment consists in regulating the actions of one series of muscles, and of exciting to action a second.

"The first remark relates to the tongue. The tongue exercises two distinct functions, one relates to deglutition, the second to speech. These functions, when exercised in health, call on the organ for movements totally distinct from each other. In deglutition, as has been already explained, the tongue is engaged in pressing the food against the roof of the mouth and the soft palate, while speech requires its co-operation with the teeth and lips in the enunciation of sounds. There is an exception to this division in pronouncing the letters *d*, *h*, and *l*, and sometimes in sounding which, the tongue touches the roof for an instant, but rather incidentally than as essential to the required sound. In all persons having a defective palate, the movements of the tongue are eccentric, consequent on an instinctive effort to close the opening in the palate by its means, while endeavouring to fill up the interval of the fissure. This action becomes habitual, and is only subverted and corrected by the agency of time. The other series of muscles relate to those of the soft palate, which having been deprived of antagonism, and partially cut asunder during the operation, are weak and unsteady. The soft palate is paralysed, it does not respond to the actions either of the tongue or of the agents of respiration or of volition. Its condition corresponds, though in a minor degree, with that of the same organ in stertor, it vibrates under the influence of a current of air. The soft palate as well as the tongue must be educated.

"With respect to the tongue, I have seen great advantage obtained from speaking with a pebble in the mouth, or a glass bead, which has been fixed to the lower teeth, or some similar means of fixing it in the base of the mouth. In pronouncing the letters *c*, *s*, and *z*, the tongue is required to regulate the current of air passing through the closed teeth; but the organ is instinctively raised to its old position, and hence the difficulty of these letters. This difficulty is only to be overcome by long and continued exercise. In fact, the advantages derivable from the operation do not hold reference so much to the soft palate directly, as to the tongue, which by the union of the fissures is released from the abnormal duty of contributing to its closure.

"The muscles of the soft palate may be exercised without much difficulty in

health. That we possess a voluntary command over them, may be inferred from the power exercised in regulating, by their means, the size of the column of air which passes at any given moment through the nose, as in the act of blowing it. In this action the soft palate is drawn backwards to touch the pharynx. Air is forced against it from below, and on the sudden remission of the contact is the force of the current obtained. Children who are the subjects of this deformity, should be taught to practise this and similar movements before a glass, with the mouth open. They should be required to read aloud frequently during each day, audibly and very slowly, and as distinctly as possible. I have generally recommended that the child should be placed at the distance of the further end of the room, and be required to speak in a loud tone, that all the defects of speech and difficulty of enunciation should be palpable to itself as well as to others. These difficulties will be chiefly comprised in the three letters I have mentioned, *c*, *s*, and *z*, and are really due to the tongue while holding a false relation to the front teeth, rather than to any want of action of the soft palate itself. This may be understood by observing the different characters of the sound in pronouncing the combination of the letters *sch*, as in the word *she*, and the sound of the *s* employed singly. In the former, which requires the approximation of the tongue to the roof of the mouth, children, after the operation, will pronounce almost perfectly; but directly the tongue is required in the front of the mouth to regulate the column of air passing through the teeth, then the defect becomes palpable, and the articulation imperfect. Perseverance in the endeavour to regulate these actions will succeed in enabling the patient to improve his speech in a very important degree, and to pass current in society without observation." (pp. 441-3.)

Our next extract must be from the chapter on Hæmorrhoids. The author combines the operations by the knife and ligature, thus avoiding, he believes, the liability to bleeding after the use of the knife, with the pain and inconvenience of the ligature:

"The mode I allude to consists in passing a thin copper or iron wire in the form of a loop, made through a double canula around the root of the growth, and strangulating it by drawing the wire tight. This being accomplished, the canula may be retained in this situation for any length of time, from half an hour to an hour or more, and the pile may then be removed with the knife or a pair of scissors; and, as it is far easier to arrest bleeding at the commencement than when established, so, should bleeding occur, immediate pressure should be applied by the finger or by pads of lint. Serious hæmorrhage, however, is a rare occurrence." (p. 519.)

There is a great deal of sound reasoning in the chapter on Lithotomy and Lithotritry; but there are also some opinions upon which we feel it necessary to comment. First, says Mr. Skey, "Well developed manhood is indispensable to the use of the lithotrite." . . . . . "It is impossible to make a lithotritry screw of a size admissible into the bladder of a child, that would bear the force employed on it, without danger of breaking in the bladder." (p. 523.) Now, in the first number of this Journal (p. 120), we proved that the difficulties and dangers of lithotritry in children had been greatly overrated; and our own subsequent experience, as well as that of M. Civiale, has tended to confirm us in the belief there expressed. Still, as a general rule, more sittings are required to crush a stone in a child than in an adult, and the liability to arrest of fragments in the urethra is greater. It is only by a comparison of a large number of cases treated by the two operations, that their proportionate success can be determined. As to the instruments employed, if Mr. Skey uses the *screw* lithotrite, perhaps what he says may be true; but we hold that this is the worst kind of instrument employed at the present day, and that the *rack and pinion*



lithotrite is very much to be preferred. This may be had as small as can ever be required for the urethra of a child (provided the meatus be previously dilated), and quite strong enough to ensure safety. We are happy, here, to be able to correct an error into which we had been led in the article on lithotrity just alluded to (p. 103), and to refer this important improvement in lithotrity instruments to its real author, Mr. Fergusson.

Mr. Skey has arrived at the following conclusions :

“ It has been stated that, unless contra-indicated by circumstances, the general treatment for stone by operation is that by lithotrity. But from the above remarks it is obvious that the exceptions are very numerous to its application. It is an impossible operation in childhood, and, except under the qualifications above alluded to, unsafe; even in early boyhood it cannot be resorted to; in stricture of the urethra, or in any morbid contraction of that canal, so long as the contraction continues, the difficulty of the operation is greatly increased; under circumstances of irritable bladder, with frequent micturition; it is objectionable in a stone of very large dimensions, and impracticable in a stone the diameter of which exceeds that of the instrument employed to crush it.” (p. 528.)

Upon some of these opinions we have already remarked, and may add that stricture or irritable bladder may be generally overcome by proper preparatory treatment. The last sentence is quite beyond our comprehension. How the diameter of a lithotrite is to equal that of a calculus, we confess ourselves at a loss to understand.

Mr. Skey has added nothing to our previous knowledge of the relative success of lithotomy and lithotrity, as tested by numbers. Indeed, throughout his book he exhibits a great reluctance to descend to statistical details. He continually speaks of one kind of accident being “more common” or “much more frequent” than another, and of one method of operating as being “less successful” in its results than others; but in no one instance has he furnished us with the numbers which have led him to form such conclusions. With respect to lithotrity, this want of explicitness is much to be regretted; but we are bound to add that Mr. Skey is by no means singular in the reluctance with which he submits the numerical results of his operations to the judgment of the profession. It is not very long since Mr. Hawkins stated, at one of our medical societies, that he had seen the operation of lithotrity performed in two hundred cases. Those who know that Mr. Hawkins is the assistant of Sir Benjamin Brodie, therefore learn that the opinion of the first surgeon of his day is so favorable to lithotrity, that he has performed it upon two hundred of his patients. But neither Sir Benjamin nor Mr. Hawkins have thought it their duty to benefit their professional brethren, by acquainting them with the results of such extensive experience. Silence upon a topic of this nature is especially to be lamented; for it only requires a number of facts, carefully observed and honestly recorded, to settle the fate of one of the greatest innovations in modern surgery, and to decide the debated question of the proportionate success of lithotomy and lithotrity. We venture, therefore, to express a hope, that before very long Sir Benjamin Brodie will add another to the many favours he has conferred upon his brethren, and we may say upon mankind, by laying the results of his experience before them.

The only points peculiar to Mr. Skey in performing *lithotomy* are, that he uses a staff made more convex than usual, where it is intended to project in the perinæum, and straighter than usual from the curve to the point.

He also argues, that the danger of dividing the artery of the bulb has been greatly exaggerated, and believes that the bulb is *always* more or less divided, and the artery of the bulb *frequently* in this operation. He does not say what per centage he means by *frequently*, nor does he give the success of his own lithotomy operations.

We have so lately entered at great length upon the subject of Ophthalmic Surgery, that we need not now refer to the chapter on Operations on the Eye. With one more extract, then, we conclude our notice of this book. The case is one of very considerable interest, and adds very much to the value of Mr. Skey's chapter on the Extraction of Foreign Bodies :

"A man presented himself to me at St. Bartholomew's Hospital, in the year 1845, and requested that I would remove his leg. He stated that he had received a musket-ball in the left knee, at the storming of Ghuznee, in India ; that he had suffered pain in the joint ever since—a period of some four years ; that he had no hope of recovery, as all means of relief had been exhausted. On examining his knee, I found a depression, about three inches above the outer condyle, formed by the ball, which had not passed through the limb. When this fact was fully ascertained, I felt much inclined to trephine the bone : but the question at once raised itself, at what point to trephine ? Circumstances curiously combined to enlighten me in this inquiry. I had at that time under my care Captain H., the officer who led the storming-party at that seige, on the blowing up of the outer gates with gunpowder, and who was well informed of all the details. From him I learnt the height of the walls from which the soldier was shot, and the distance from which he must have been placed from their base ; and by these means I calculated the angle at which he must have been struck, and that the ball, if not diverted from its course, must be deeply sunk in the substance of the inner condyle. This probability was increased by the fact of pain being referred to that region. Under these circumstances, I determined to trephine the bone. For this purpose I had a deep trephine made of about eight lines in diameter, and having drawn a line, forming an oblique circle around the joint from the point struck, I cut down on the condyle at the point crossed by this line. I then applied the trephine, and cut into the centre of the condyle. The bone was harder than it usually is in health, and of this sign I augured favorably. Having removed the centre, at the depth of somewhat less than an inch from the surface, I struck on a metallic body, which proved to be the bullet, and which I extracted with some little difficulty, by means of a strong instrument, on the principle of a corkscrew, which I had had made for that purpose. The man recovered, and retained a useful limb. Doubtless, the success of this operation was greatly due to good fortune, and not exclusively to skill." (pp. 598-99.)

We should be inclined, however, to say, that careful consideration and skill did more than good fortune—notwithstanding the disclaimer of the concluding sentence.

In our introductory remarks we expressed our opinion so freely as to what this book is *not*, that we may now, with a clear conscience, say what we think it *is*, namely, a work very creditable to its author, and one which we can cordially recommend to practical men. The style is at times careless, and even obscure, as we think that our readers will have found out for themselves, from the perusal of the foregoing extracts ; but it is generally plain and forcible. The author is evidently one who can think and act for himself ; and we rise from the perusal of his book with the conviction that it is the production of a surgeon, at once bold and careful, whose opinions are formed upon the foundation of a vast experience.

PROF. BLASIUS's work opens with a numerical report of the Surgical and Ophthalmic Clinical Hospital, at Halle. The patients were received from the town, and from the surrounding country to a circuit of fifteen to twenty miles. The total number of patients, whose cases are included, was 32,949. Of these, 4631 were afflicted with diseases of the eye. The total number of in-patients, however, was but 2354; that of out-patients being 30,595.

On looking over the account of the numbers of different operations performed, we find 125 for the removal of tumours of various kinds, 9 ligatures of great arteries, 60 divisions of tendons, 53 extirpations of carcinoma of the lips, and 15 of carcinoma of other parts of the face, 6 resections of the lower jaw, 8 plastic operations on the palate and urethra, 167 cataract operations, 38 choremorphoses (or operations for the formation of artificial pupil), 19 amputations of the mamma, 28 herniotomy, 2 operations on recto-vesical and 4 on vesico-vaginal fistulæ, only 4 cases of lithotomy, 55 of hydrocele treated for the radical cure, 56 reduction of dislocations, and 67 great amputations. These were among the most important of 1641 recorded operations. Extraction of teeth, application of suture to wounds, opening abscesses, removal of foreign bodies, the formation of moxæ, setons, acupuncture, and such minor operations, although very numerous, are not included in this list of 1641. The number of students varied from 26 to 78; they, as well as the patients, gradually increasing under the superintendence of Dr. Blasius.

The greater part of the work consists of contributions to practical surgery, in six parts: 1, resection of bones; 2, plastic operations; 3, operations on cicatrices; 4, urethrotomy; 5, uterine polypi; and 6, uterine prolapse. Each of these subjects is introduced by some general preliminary observations, the cases are then related, practical remarks and deductions following.

Of seven cases of resection of the lower jaw, five were undertaken on account of carcinoma, which had extended from the surrounding soft parts. In only one of these five was the result satisfactory. One patient died forty hours after the operation. The internal jugular had been wounded and tied. Apoplectic symptoms came on, and extravasated blood was found within the cranium after death. In three cases the cancer returned, and caused death. Blasius has collected accounts of all published cases of exarticulation and resection of the lower jaw, and the results are as follows: cases, 220;—cures, 140; deaths, 53; 8 return of diseases; 19, result unknown. These numbers include all cases, whatever was the nature of the disease or ground for operating. Among them were 50 cases in which cancer of the soft parts was present. Of these, the cures were 17, deaths 24, in 3 the disease returned, and in 6 the result was doubtful. Thus, if we regard the mortality of these operations in general, it is 1 in 4.150, while in cancerous cases it is 1 in 2.083, and if we remove the latter cases from the general category it is 29 to 170, or 1 in 5.862. Of the 53 deaths, 27 may be attributed to the operation, and 13 to a return of the original disease, the cause of death in the remaining 13 not being ascertained. Of the 24 deaths in cancerous cases, 12 were the result of the operation, 7 of a return of disease, and 5 of unknown causes. The causes of death in the 12 cancerous cases, which proved fatal soon after the operation, were in 3 meningitis or apoplexy, 2 suffocation,

2 erysipelas, 3 profuse suppuration, 1 purulent infiltration of the blood, and 1 unrecorded. The conclusion at which Blasius arrives, after careful consideration of these cases, is, that resection of the lower jaw in cancerous cases is only to be undertaken, with a hope of perfect cure, when the disease is purely cutaneous. In other cases it can be but of temporary utility, and therefore can only be recommended under certain peculiarly favorable circumstances.

A case of resection of the head of the humerus is related, in which most of the motions of the joint were preserved; and cases of excision of the elbow-joint, on account both of necrosis and caries. The author has examined all the published cases of this operation he could collect, in order to determine under what circumstances ankylosis might be expected to follow it. He has drawn 36 cases from English and American authors, 27 from France, 26 from Germany, and 1 from Italy; in all 90 cases. Death was the result in 10 cases, in 2 caries returned, in 8 the event is not recorded. Of the remaining 69 cases, a new joint was formed in 41, in 7 ankylosis took place, in 21 no information could be collected on this point. In 24 successful cases of 28 partial resections of this joint, 6 were cured with, and 10 without ankylosis. Of 53 total resections, the cures were 40, 1 being cured with, 29 without ankylosis, the remaining cases uncertain. Thus the probability of ankylosis is greater after partial than after total excision of the elbow-joint.

Interesting cases are detailed of resection of the body of the ulna, and of a portion of the tibia, both in cases of recent compound fracture; and in another case of old united fracture, on account of unseemly projection. Four cases of excision of the os calcis are related, and the section is concluded by some remarks on enchondroma, with a description of the bone-forceps used by the author. They resemble those in common use in our hospitals, with the addition of a spring connection between the blades, and have a moveable instead of a fixed joint, like those used by various hardware artizans. They are said to cut bone much more smoothly than other forceps, and without any splintering.

*Plastic operations.* This section is an important one. It opens with introductory observations upon some of the general principles of plastic surgery. The author contends that many circumstances thought to contraindicate these operations should not do so. For instance, it is said that when the cause of the defect we seek to remedy is still in operation, plastic measures should not be adopted. Yet the author proves by reference to cases, that this precept is unsound. In one of his fortunate rhinoplastic operations upon a girl, lupus on the nose was healed, but still existed on the cheek, and on the neck there were large scrofulous ulcerations. Yet transplantation was perfectly successful, and nine years afterwards the nose remained perfectly sound. In another case of a man, aged 29, whose nose had been destroyed by lupus, the part was not healed, yet union of the flap from the forehead to the nasal stump, after removal of the ulcerating edges, was perfectly accomplished. The result was equally successful in a similar case, the patient being a female, 53 years of age. We know perfectly well that lupus may be cured by excision of the diseased portion of skin, and sometimes by cauterization, which is merely another mode of removing the diseased part; proving that the surrounding parts are healthy, and fit for productive processes. Cancer, again, has

been often improperly supposed to contraindicate these operations, for after removal, the wounds quickly unite or cicatrise, and transplantation of another part to the seat of the excised carcinoma also succeeds. The author has transplanted a portion of skin with the best result immediately after removal of a carcinomatous eyelid. Scrofula has been considered as the special enemy to plastic operations; even Dieffenbach considered that pre-existing scrofulosis in a patient apparently in flourishing health, was more to be feared than herpes exedens or syphilis actually present. We do not believe, however, that in a cachectic individual every wound and every suppurating surface immediately takes on a specific character. We see blisters, setons, and issues, in such patients heal without difficulty every day, and many plastic operations in scrofulous persons have been successful. The question of syphilis is somewhat different. One would heal primary sores, of course, before attempting to operate; but we have then no sign by which we can know whether secondary symptoms may not occur. Blasius performed the palatine suture in a girl who had a hole in the soft palate, the consequence of syphilitic ulceration. The syphilitic disease appeared to have been perfectly cured, yet, instead of union, new syphilitic ulceration commenced. We have yet to learn under what circumstances these operations can be successfully performed in patients subject to a return of syphilis.

Again, it is said that the skin transplanted must be free from cicatrices. This is true to a certain extent, but a cicatrix may often be avoided by good management. Blasius restored the nose of a female, who not only had a very low forehead, but one crossed from above downwards by the scar of a former fall. He could not form his flap from one side, without going into the eyebrow or scalp, so he formed the nose in two halves, one from each side of the forehead, and united them together by sutures along the ridge of the nose.

The next point commented on by our author is the extreme importance of avoiding suppuration as much as possible, not only at the point of union or transplantation, but also at the spot from which the flap has been procured. Ugly cicatrices, alterations in the form of parts, and the effects of contraction of neighbouring parts upon those restored, are thus avoided. The mode of proceeding must be referred to the description of particular operations.

Thirdly, it is not advisable, when replacing or restoring a part, to use old shrivelled remains of that part when we are not able, after it has been raised and smoothed, to prevent it again shrivelling up by means of "an invincible and permanent organic dike." (Damm.) Plugs, tubes, and mechanical supports are quite useless for this purpose.

Fourthly, a part naturally free from hair should never be restored by skin on which hair grows. Here Blasius differs from Dieffenbach and Blandin. It may be possible sometimes to prevent growth of hair, but we can never make certain of doing so. Blasius refers to one case in which he formed a nose partly from two inches of the shaved hairy scalp. The lower part of the nose was thus restored, and the hair persisted in growing so vigorously that it was necessary to shave the nose every day. It is not true that, as a general rule, hairy parts become smooth after transplantation. Jobert closed a vesico-vaginal fistula by transplanting a flap from the labia; four months after the cure was complete,



the part transplanted was found covered with hair of the same colour as that on the pubes. It is not true, therefore, that skin made to assume the place of mucous membrane takes on the characters of the latter tissue.

Professor Blasius describes four principal varieties of plastic operations: 1st. The Indian, or that in which the repairing flap is brought from its natural position at more than a right angle. 2d. That in which it describes a right angle or less. 3d. The French method "*par soulèvement du lambeau*." The flap being brought towards the defective part by a simple movement on its transverse diameter. 4th. Simple traction of a portion of skin, after separation from subjacent tissues, towards the defect, where it is united. These four methods of transplantation from neighbouring parts enable us to complete all plastic operations, and form, at the same time, the distinction between the plans of different surgeons.

The special plastic operations reviewed are restoration of the nose, lips, cheeks, and eyelids.

Cases are related of restoration of the nose by frontal flap; of the septum, of one ala, and of the anterior cartilaginous portion, from the upper lip. All particulars of the operations are described, but the want of woodcut illustrations renders the description imperfect. The Germans are far behind us in this respect, and do not appear to consider what a lengthy paragraph may be saved by the insertion of a small cut. It is true, that in the section on restoration of the eyelids, there are two woodcuts, sufficient for this purpose, but badly executed. In performing this operation, Blasius follows a method of his own, which certainly appears to possess advantages over that of Dieffenbach and his imitators. He raises his flap either from the side of the nose, or the anterior part of the temple; in either case forming it in the shape of a myrtle leaf, by one perpendicular and one curved incision. After uniting the edges of the flap in its new position, the edges of the wound in the face or temple are carefully brought together by insect needles and twisted sutures. It will be impossible to institute a comparison between this and other methods without a series of diagrams; we therefore pass on to the chapter on the Operations on Cicatrices, in which we have only to remark, that several cases are related, in each of which the general principle of the operation performed was excision of the cicatrix, and subsequent union of the edges of the wound so caused to each other, by means of insect needles and twisted sutures. The result was in general most satisfactory.

A chapter on Strictures of the Urethra contains some successful cases of perinæal urethrotomy. In another on Uterine Polypi, cases are related chiefly remarkable from the large size attained by the morbid growth. In the treatment of Prolapsus Uteri the author has followed a plan in two cases which is worthy of further trial. It appears to have been successful in both, although the case was not watched long enough to afford confidence in the statement that relapse did not take place. Several ligatures were passed circularly through the vagina, and gradually tightened so as to produce suppurating strips running all round the canal. When these strips cicatrize, the contraction of the cicatrices produces stricture of the vagina, and a close union of its mucous membrane with the subjacent tissues.

We have but little more to say of this book. The author deserves

credit for industry and good intentions; but his style is singularly verbose and involved, and with good materials he has certainly produced one of the dullest books it was ever our misfortune to wade through. It is, however, a book of a *class* we should be very glad to see multiplied on this side the channel. In our enormous metropolitan hospitals, in those of the northern and western capitals, in the infirmaries of our large provincial and county towns, cases are daily occurring, which, if carefully observed, faithfully recorded, and then made known, would add immensely to our knowledge of disease, its effects and treatment. As it is, the occasional publication of a volume of reports from some three or four hospitals is all the official information the profession can derive from the mass of experience collected by their hospital brethren. Now and then some diligent student communicates a case or two to some weekly journal; but we have nothing like systematic reports from any hospital in the kingdom, except, perhaps, the Consumption Hospital. Surely this great want might be easily repaired. A quarterly and annual report might be drawn up on a uniform plan, from every hospital, infirmary, and dispensary in Great Britain, with short narratives appended of all cases of unusual interest or importance. It would be no misapplication of hospital funds to print such reports; and, in case of impracticable governors, we suspect that our weekly contemporaries would be heartily willing to find a place in their columns for such matter. Perhaps a plan might be arranged for the collection of such reports by some central committee, and for their arrangement and annual publication, somewhat in the form of the Returns of the Registrar-General. Some such plan is worthy of consideration.

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#### ART. II.

*A Systematic Treatise, Historical, Etiological, and Practical, on the Principal Diseases of the Interior Valley of North America, as they appear in the Caucasian, African, Indian, and Esquimaux Varieties of its Population.* By DANIEL DRAKE, M.D.—*Cincinnati*, 1850. Vol. I, pp. 878.

THE above elaborate title belongs to as elaborate a work, the first volume of which only as yet appeared. The title expresses also clearly enough the design and purport of the treatise, which has no less an object than a complete medical history of the interior valley of North America. This history comprises, therefore, full details of the topography of the whole of this immense district, the condition of its inhabitants, and the mode in which they are affected by the climatic and social influences which surround them. The labour which is necessary for making a complete medical history of a single place is very great. The original geological conformation of the soil, the geographical relations, and the connected atmospheric conditions of the country,—the changes which have been produced in it by cultivation,—the races which dwell on it, their mode of life, their political and social state, which may be consequent either on the influences of their own territory, or on the inherent impulses of race, or on the impressions derived from the surrounding states,—in fact, physical and moral influences, of whatever kind, are included in this term of complete medical history.

The germ of the present work was a pamphlet entitled 'Notices concerning Cincinnati,' which was published forty years ago. At that time the greater part of the interior valley of North America was a primitive wilderness. It has since been cleared and peopled; whole states have come into life in spots which, in the memory of man, were unknown and unexplored. This spectacle of the birth and growth of infant nations seems to have produced in the mind of Dr. Drake a strong desire to describe the changes which he saw going on before his eyes, and to leave to posterity as full an account as possible of the present condition of the country and its inhabitants. He has accordingly devoted part of every year to journeys throughout almost the whole of North America, and has sought for and obtained information on all points from the resident physicians of each locality.

To attempt an epitome of the numerous facts mentioned in the work would be impossible. We must confine ourselves to a general review of the author's plan, and of its mode of execution.

The "interior valley" of North America is the term given to that immense area which extends from the Polar Sea to the Gulf of Mexico, and is bounded on the east and west by the Appalachian Hills, and by the gigantic ranges of the Rocky mountains. These mountain limits, approaching each other towards the south, diverge as they pass northwards, and give to the valley which lies between them a rude triangular shape. If eight millions of square miles be assumed as the area of North America, this valley includes six millions, or three fourths of the whole continental surface. One half of the valley penetrates into the black and almost uninhabited regions north of Canada to the pole; the other reaches to the sunny and golden plains which the Mississippi waters, as it approaches the termination of its protracted course.

The Rocky mountains which form the western boundary of the Great Valley, are a continuation of the Mexican Cordilleras; they pass north and north-west to the seventieth degree of north latitude, and run to a height of from ten to fourteen thousand feet. They lie at a distance of about ten degrees of longitude from the shores of the Pacific Ocean; and between them and the coast, and helping with them to form the valley of the Oregon, run the slender but lofty ranges of the Sierra Nevada, the Californian Alps.

From the Rocky Mountains, spurs of elevated land come off at right angles, and stretch into the plain; to the north at the 42d parallel are the Sweet Water Mountains passing to the east; at 34 degrees of latitude an extensive tract of table land, called the *Llano Estacado*, or the *Staked Plain*, the surface of which is about 2000 feet above the streams which flow around its steep acclivities, also passes to the east. From the eastern extremity of this plain, the Ozark Mountains come off and run to the north-east, and finally the great interior valley is also diversified by another table-land, called the *Coteau des Prairies*, which has an altitude of from 1800 to 2000 feet, and rises between the Mississippi and the Missouri rivers.

On the western side of the Interior valley, the Appalachian Mountains form a less effective barrier between the valley and the Atlantic, than the Rocky Mountains form between the valley and the Pacific. They run nearly parallel to the Atlantic, and are composed of interrupted but nearly

parallel ridges. The spurs of land passing towards the trough of the Great Valley are less conspicuous on this than on the other side, and the chain is itself cut through between the forty-second and forty-sixth degrees of latitude, by the Great Lakes, and the outlet of the St. Lawrence.

Penetrating deeply into the sides of the valley are two Mediterranean seas; the Gulf of Mexico at the south, and Hudson's Bay in the north. Into these great inlets, various rivers rising in the centre of the valley empty themselves; so that between the two seas a sort of water-axis is formed, which occupies the centre of the valley. The largest of these streams are the Mississippi, running to the south into the Mexican Gulf, and the St. Lawrence and the Red River flowing north. These rivers begin in the centre of the trough of the valley close to each other. The Mississippi, soon augmented by its great tributary the Minisotah, passes south through eighteen degrees of latitude, and after a course of three thousand miles, pours itself into the sea. The other great river is known at first by the name of the *St. Louis*, and rapidly descends to the west end of Lake Superior; issuing from Lake Superior under the name of *St. Mary's*, it passes into Lake Huron; and then emerging from this under the title of *St. Clair* river, it runs into the lake of the same name. Leaving the lake as the *Detroit*, it is absorbed by Lake Erie; and when it issues from this, it is known as the *Niagara* river, and hastens to pour itself over its celebrated falls into Lake Ontario. From lake Ontario the mighty stream, now known as the *St. Lawrence*, flows eastwardly into the gulf which bears its name. A third river, the Red river of the North, inferior to these two, rises close by them, and flows directly north into Lake Winnipeg, and thence into Hudson's Bay.

In addition to these central rivers, lying in the trough of the Great Valley, various minor streams flowing down the troughs of the lateral valleys are tributary to them. It is unnecessary to enumerate these, since we desire only to put forward the grand features of the interior valley. It may be merely mentioned, that there are seventeen minor hydrographical axes, formed of course by mountains and valleys, to which almost all the rivers may be referred.

The central trough, or grand hydrographical axis of the valley,—that in which the Mississippi, the St. Lawrence (in part of its course), and the Red River lie,—reaches from the Gulf of Mexico to Hudson's Bay. Passing from the Gulf, the trough, with many sinuosities, tends to the north; its altitude rises very gradually, viz. about twelve inches for every minute of latitude, or, following the course of the Mississippi, of  $4\frac{1}{2}$  inches for every mile. The summit level, or culminating line, is between Big Stone and Traverse lakes,\* in N. lat.,  $45^{\circ} 35'$ , about 2600 miles from the Gulf along the sinuosities of the trough. From this point, the level of the trough begins to descend; it falls gradually to Lake Winnipeg, and then precipitously to Hudson's Bay. This trough must have been formed by a deep current from the north, and Lake Traverse and Bigstone lakes are the hollows or chasms left filled when that river ceased to flow; in the same way as the crescent lakes of the lower Mississippi, which were once portions of the ancient channel of the father of waters, have been formed.

\* These lakes, which are on either side of the summit level of the trough of the valley, are only three miles apart, and have been known to be united by floods, so that the whole trough was one water-course.

The St. Lawrence river, during the greater part of its course, lies in a minor or second great trough, which is almost at right angles with the central trough. It may be said to commence at Lake Superior, and to extend thence to the north and east, as far as the Gulf of St. Lawrence. It communicates with the central trough by the Illinois river.

The course of these great hollowings out of the interior valley, may be better represented by an illustration given by Dr. Drake. A voyager may start from the Gulf of Mexico, and ascending the central trough along the course of the Mississippi, pass over the summit on which Lake Traverse is, and so descend into Hudson's Bay and the Polar seas; or before reaching the Traverse summit, he may turn to the east along the course of the Illinois river, pass into the lakes, descend the St. Lawrence, and so reach the Atlantic upon the fishing banks of Newfoundland.

This general outline will suffice to give our readers an idea of the grander features of this great valley, and of the boundaries and water-courses it presents. A very general view of the geology of the valley will lead us at once to the consideration of the proper subject of the book.

A great part of the valley consists of alluvial or bottom lands. The débris of various soils, washed down by the rains, or deposited from the waters of the rivers, are profusely spread over the surface. It is not possible to estimate the area of these alluvial grounds; but they are so continuous, that the traveller hardly leaves them except to cross the streams from which they have been deposited. The valley, however, is not composed solely of such recently deposited soils; there are found in it also traces of older and of grander forces. Along many of the rivers and even mill-streams, "there are," writes Dr. Drake, "in the rear of the alluvial bottoms, older and higher deposits of transported materials, which it may be seen, at a single glance, were made by rivers incomparably deeper and broader than those which now flow in the same valleys. These are commonly called second bottoms, and might sometimes receive a still higher numerical designation, for now and then we see a third terrace."

Nearly related to these old terraces, are deposits which are spread alike over the hills and valleys, from the plains to a height of 1500 feet. The materials which compose them are waterworn, and their arrangement shows that they have been transported from the north. And nearly coextensive with these post-tertiary deposits, are enormous boulders of granite or other primitive rocks, which have probably been floated down on ice, from the Polar Seas.

The strata which lie beneath these upper coverings, and which, as the voyager passes from the Gulf of Mexico northwards, successively crop out at the surface, are the following: cretaceous formations in West Tennessee follow the post tertiary and diluvial deposits of Alabama and the Mississippi, and are succeeded by the coal formations of Illinois and Missouri. In northern Illinois and Wisconsin, the Devonian shales and sandstones crop out from beneath the coal, and then appear the Silurian limestones and slates, which are succeeded by the granite and other primitive rocks which stretch up from Lake Superior to the Polar seas. Volcanic action has but little disturbed the simplicity of this arrangement; the Ozark hills of primitive rock, being the only hills which have been forced up. The action of water is, however, more evident. The valley appears to have been formed by vast submarine currents, which have scooped out and levelled the various



strata, and at the same time, have deposited on these strata the drift and boulders which form the more ancient of the looser coverings.

Such is the vast region which Dr. Drake has undertaken to survey ; and the plan which he has adopted is the following :—The grand mountain features of the valley are of the simplest kind ; as the mountains are grouped chiefly on the borders of the valley, they offer no characters by which the vast plain between them may be divided into regions. In order to divide the great space of the interior valley into districts small enough to be easily described, Dr. Drake has selected the great rivers as his marks of division ; not, of course, using these as they are commonly employed, to designate the boundaries of political states, but grouping together the various provinces, which are comprehended in each of the great hydrographical bodies. When the valley is attentively considered, with a view to its subdivision into hydrographical areas, four great districts can be at once mapped out. If the map of North America be consulted, and a pencil be placed in the state of New York at  $42^{\circ} 15'$  north latitude, and  $78^{\circ} 30'$  west longitude, it impinges on the northern sources of the Alleghany river. If the pencil be then passed to the south and west to  $41^{\circ}$  north latitude, below Lake Erie, and then west and by north round the southern extremity of Lake Michigan, and then in a more northerly direction in the regions west of Lake Superior, a great line of division has been drawn. To the south of this line, all the rivers flow to the south, and pour themselves into the Mississippi or other streams passing to the Mexican Gulf. To the north of the line, the water runs north, forming the lakes, the St. Lawrence, and the Red River ; this line or water-shed, in fact, determines the fall of the water in the three great directions formerly noted, viz., towards the Gulf of Mexico by the Mississippi, towards the Gulf of St. Lawrence by the river of that name, and towards Hudson's Bay by the Red River. The altitude of this water-shed varies from 580 to 1700 feet. It is cut through by the great central trough in the regions to the west of Lake Superior ; and to the west of this section it stretches to the north and west to join the Rocky mountains. The portion of the valley to the south of this line forms the southern or Mexican hydrographical basin, and in this lie not only the Mississippi but various other minor streams. The portions to the north is divided into areas, according as the waters flow into Hudson's Bay or the St. Lawrence ; and north of these another region is entered, the Polar, which is comparatively little known.

The interior valley is then divided into,—

1. The Southern, or Mexican Hydrographical Basin.
2. The Eastern, or St. Lawrence.
3. The Hudson.
4. The Arctic, or Polar.

The political divisions are as follows :—The Southern Basin chiefly belongs to the United States, its south-western and extreme south to Mexico, a small northerly portion to Great Britain. The St. Lawrence Basin is almost equally divided between England and the United States. The Hudson belongs chiefly to England, but a portion projects into the United States. The whole of the Polar Region belongs to Great Britain.

Each of these basins is then treated of in detail by Dr. Drake. We shall enumerate the different heads under which he considers the Southern Hydrographical Basin, for the purpose of exhibiting his method; and we shall then proceed to extract from the work such portions as bear more strictly on the proper medical history of these important countries.

In considering the Southern Basin, our author treats,—1st. Of the Gulf of Mexico. 2d. Of the Mississippi, and its banks. 3d. Of the land west of the gulf and river. 4th. Of the land east of the gulf and river. The gulf is discussed under the following sections:—its position, form, and area; its depth; its currents; its temperature; its tides and inundations; and its coasts. Under this latter section is brought in the description of all the towns, such as Vera Cruz, Tampico, Galveston, Mobile, &c.

A single extract, which we take almost at random, will suffice to give a specimen of this part of the work :

“1. The beautiful Bay of Pensacola, in the State of Florida, is connected with the Gulf of Mexico by a strait one mile in width, the greatest depth of which is 24 feet. The banks of this entrance consist of sand drifts, which rise but a few feet above the surface of the water. That on the west side, separated from the main land by a shallow lagoon, is called Foster’s Island; that of the opposite side, likewise separated by a long navigable sound, is called Santa Rosa Island. This island stretches off to the east for the distance of 40 miles, being from one to two miles in width, and rising in some places to such a height, that its white sands are visible to a considerable distance, and serve as beacons to the navigator. Its surface presents many little pools and marshes abounding in shrubs and rattlesnakes, and is generally studded with tufts of a heath-like undershrub (*ceratiola ericoides*), among which there are a few scattering and stunted pines and live oaks. Its whole outer beach is lashed by the waves and swells of the Gulf. Its opposite shore is separated from the continent by Santa Rosa Sound, just mentioned, which opens into the Bay of Pensacola, about three miles from its mouth. From below the junction of the sound the bay widens, yet one shore is everywhere distinctly visible from the other. On both sides a tertiary or post-tertiary plain, from two to eighty feet high, composed of yellowish sand above and white sand beneath, approaches more or less closely to the margin of the bay, and constitutes its banks.” (pp. 49-50.)

The description of the bay is continued in this minute and careful fashion through two more pages; but as our object is simply to show the method, we omit these, and proceed to extract the passage referring to the town of Pensacola, which lies in N. lat.  $30^{\circ} 28'$ , and W. long.  $87^{\circ} 12'$  :

“Its site is a level plain of blown sand, rising but a few feet above the surface of the water, and surrounded by the post-tertiary pine-covered terrace, which everywhere environs the Bay. Between the lower plat and this terrace, there is a narrow semi-circular belt of swamp, originally covered with cypress trees (*cupressus disticha*), which have been replaced by a dense jungle of Titi bushes (*mylocarium ligustrinum*). Numerous springs of soft water, the product of rain upon the adjacent plateau, discharge themselves into this swampy belt, the extremities of which are salt marshes of limited extent. In the month of March I found the heat of these springs as low as  $62^{\circ}$  Fahr., a temperature which shows their origin to be superficial, and that they had been affected by the previous winter. By these springs the water of this paludal tract is kept pure, and by the compact Titi grove, the rays of the sun are prevented from acting on its surface.

“Pensacola is an old town, and settlements near the outlet of the bay were made before that of the town. Having belonged successively to Spain, France, England, and the United States, its population, improvements, and mode of living offer a

mixture of the whole; the houses mostly of wood, are chiefly built in the Spanish and French styles, and are scatteringly distributed over the plain. Apart from the people of the town, the seamen of our national ships, the persons attached to the navy yard, and the small garrisons which man the different forts, the population of Pensacola is but limited, for the surrounding country is, in general, too sterile for profitable agriculture.

"The yellow fever has been several times prevalent in the town among the shipping, and at the navy yard, but the number and malignity of its invasions bear no comparison to its severe visitations of Mobile and New Orleans. Of autumnal intermittent and remittent fever it will be proper to speak more extensively. From the forts to the town of Pensacola inclusive (all on the west side of the Bay), although there are some swales and small swamps or ponds among the sand drives, and some narrow tracts of salt marsh, there are, as we have seen, no deposits of silt, and the organic matters accumulated in the wet or paludal spots are chiefly those which belong to the pine forest. Now the inhabitants of this range of coast have for a long time enjoyed an exemption from autumnal fever, remarkable for a southern locality. The town of Pensacola has even been resorted to as a summer residence by citizens of Mobile and New Orleans. When, however, we ascend the same coast, about ten miles above the town to the estuary of the Escambia River, we find a state of things entirely different. The silt brought down by that stream has filled, as we have seen, a large portion of the western head of the bay, and thus generated a marsh several miles in width, near which the settlers have been fatally scourged by autumnal fever, although they escaped yellow fever when it prevailed in the town and navy yard below. The medical history of this devoted locality dates back more than eighty years, as may be seen from the following narrative by Lind." (p. 52.)

Here follows a quotation from Lind, giving an account of a dreadful outbreak of autumnal fever, which occurred in 1766, among a party of sixty French settlers, who had been sent by the English government to this spot. Dr. Drake then continues :

"No other settlement in the locality seems to have been attempted for a long time afterwards. At length, in the year 1832 or 1833, a new attempt was made, by laying off a town to be called Florida, on the eastern side of the estuary, in the edge of the pine wood. All the pine terraces of the south are proverbially free from autumnal fever, but here the pine lands lie to the leeward, whilst extensive silt marshes spread out to the windward. Between twenty and thirty wooden houses were built and tenanted by as many families. Their history, as given me by Dr. Hulse, the intelligent and reliable surgeon of the Naval Hospital, and by Mr. Innerarity and Mr. Kelly, old and respectable citizens of Pensacola, may be told in a few words. Year after year, while the inhabitants of the coast below remained healthy, they were assailed by autumnal fevers of the most malignant character; the spot was at last called a 'Grave Yard,' and being abandoned by those who survived, I found, in passing through it in 1843, but two families remaining.

"These well-ascertained facts have so important a bearing on the origin of autumnal fever, that I have considered them worthy of circumstantial detail. The heat and moisture of the lower and upper portions of this little bay are the same; but while the former has only a few limited tracts of pine marsh, the latter includes extensive deposits of silt and organic matter, and to them, I think, we are bound to attribute the fatal insalubrity which has been described.

"Perdido Bay is found a few miles west of that which has been described. Its coasts are composed of white sand with copses of live oak. Its seclusion is very great, yet several naval officers have placed their families on its retired banks near the gulf, where they are said to spend the summer and autumn in perfect exemption from every form of fever. Such is the connection between a sandy surface and a salubrious summer atmosphere." (pp. 51-3.)

These extracts exhibit not only the extreme care and minute detail with which the topography and medical history of each place are given, but also the very important information which is profusely scattered throughout the whole work.

After considering the gulf and its coasts, Dr. Drake passes to the Delta of the Mississippi. The description of this immense outlet is most carefully written. The purity of the father of waters appears almost as questionable as that of our own Thames. In addition to an immense quantity of suspended mineral detritus and vegetable débris, whole trees are continually uprooted and carried down, and large masses of rock are drifted off; and these substances, with the carcasses of animals, and the contents of the sewers containing the excreta of above a million of persons, are all mixed up together by the constant passage of above four hundred steamers, and twice or thrice as many flat boats. The waters abound with microscopic infusoria; in a single specimen of water, Dr. Bailey detected no less than twenty species. A fact connected with the water of the Mississippi is rather curious. One part of Louisiana, including the fine settlements of La Fourche, the Teche, and the Attakapas, is remarkably free from the remittent and autumnal fevers, which desolate so many parts of this fertile region. This exception, which does not appear to be accounted for by any peculiarity of physical condition, is attributed, by Dr. Cartwright, of Natchez, to the influence of a plant, the *Jussieua Grandiflora*. Almost all the lakes and the bayous (the lateral outlets of the Mississippi) are covered over with this large flowering plant. It is exclusively aquatic, grows three or four feet above the surface of the water, and gives the water on which it grows the fallacious appearance of a natural meadow. The roots, radicles, and leaves of the plant form such a dense covering to the water, as to enable snakes to cross over the stream. On the wide bays and lakes, masses of wood and trees get detached and are then entangled among its meshes, forming what are called "floating islands," on which a man can be supported. The *Jussieua* and other aquatic plants, constitute the basis and substratum of that singular kind of savannah, known as the *prairie tremblant*. By the close intermixture of these plants, on which grow grasses and terrestrial plants, a vast vegetable raft is formed, on which a man can crawl. When the foot is placed on it, the whole mass trembles, hence the name of trembling or shaking prairie. This plant is presumed to be the cause of the healthiness of these districts, partly because the limits of the healthy region coincide with its diffusion, and partly because it purifies stagnant water in a singular manner. Dr. Drake, however, advances some strong arguments in favour of the view, that the salubrity of the climate and the presence of the *Jussieua* are simple coincidences, or rather common effects of anterior causes.

The description of the towns and countries in the Delta of the Mississippi is given at great length, but it is almost impossible to analyse these sections. Before quitting the Delta, however, we may extract some curious passages from an account of a voyage up the Mississippi made by the Jesuit Father Du Poisson, one hundred and twenty years ago, nine years after the first settlement of New Orleans. Three priests started on a proselytising expedition, which does not seem, however, to have been very successful; and they appear to have suffered a real martyrdom in the passage. The slow progress, the burning sun, the want of wind, the dangers of the "rafts,"

or floating trees, were disagreeable enough; but these were trifling compared with the want of provisions, which was very soon experienced; luckily, their rice lasted, and they seasoned it with "salt, bear's oil, and a good appetite." The good fathers, however, thought these but small troubles:

"But the greatest torment, in comparison with which all the rest would be but sport, which passes all belief, and would never be imagined in France, still less actually experienced, is that of the mosquitoes—the cruel persecution of the mosquitoes. The plague of Egypt, I think, was not more cruel—'I will send swarms of flies upon thee and upon thy servants, and upon thy people, and into thy houses, and the houses of the Egyptians shall be full of swarms of flies, and also the ground whereon they are.' They have here the *frappe d'abord*, and also the *brulots*. The latter is a species of very small gnat, whose sting is so sharp, or rather so burning, that it seems as if a spark of fire had fallen on the spot. There are also the *moustiques*, which are like the *brulots*, with the exception that they are much smaller, so that one can with difficulty see them; their attacks are particularly directed against the eyes. There are also the *guepes* and the *thon*; in one word, there are the '*omne genus muscarum*.' But none of these others are worthy to be mentioned with the mosquitoes. This little insect has caused more swearing since the French have been in Mississippi, than had previously taken place in all the rest of the world. Whatever else may happen, a swarm of these mosquitoes embark in the morning with the voyager; when they pass among the willows or near the canes, as very often takes place, a new swarm fastens with fury on the boat, and never quits it. It is necessary to keep the handkerchief in continual exercise, and this scarcely frightens them—they make a short circuit and return immediately to the attack. The arms become weary much sooner than they do. When we land to take dinner, which is between ten and two or three, there is an entire army to be combated. We make a *boucane*, that is, a great fire, which we stifle afterwards with green branches; but it is necessary for us to place ourselves in the very thickest of the smoke, if we wish to escape the persecution; and I do not know which is worse, the remedy or the evil. After dinner we wish to take a short nap at the foot of a tree, but that is absolutely impossible; the time allotted to repose is passed in contending with the mosquitoes. We embark again in their company; and at sunset, on landing, it is necessary immediately to run to cut canes, wood, and green branches, to make the *baire*, the fire for cooking, and the *boucane*. There, it is each one for himself; but it is not one army, but many armies, which we have to combat—for that time of day belongs to the mosquitoes. One is perfectly eaten and devoured; they get into the mouth, the nostrils, and the ears; the face, the hands, the body, are all covered; their sting penetrates the dress, and leaves a red mark on the flesh, which swells on those who are not yet inured to their bite. Chicagou, to enable some of his nation to comprehend what a multitude of French he had seen, told them that he had beheld 'as many in the great village (at Paris), as there were boughs on the trees, and *mosquitoes in the woods*.' After having supped in haste, we are impatient to bury ourselves under the *baire*, although we know that we go there to be stifled with the heat. With what address, with what skill, does each one glide under his *baire*! But they always find that some have entered with them, and one or two are sufficient to ensure a miserable night.

"Such are the inconveniences of a voyage up the Mississippi. And yet how many voyagers endure them all for the prospect of a gain even the most moderate. There was in a boat which ascended at the same time with our own, one of those heroines of whom I have already spoken, who was going to rejoin her hero. She did nothing but chatter, laugh, and sing. And if for a little temporal benefit, if even for crime itself, one can endure a voyage like this, should men fear it who are appointed to labour for the salvation of souls?" (p. 117.)

We shall terminate our notice of this portion of Dr. Drake's work, by



abridging an interesting parallel which he draws between the two majestic rivers, that so especially characterise this region of the globe. 1. "The Mississippi flows nearly from north to south; the St. Lawrence, originating in the same region, flows to the south-east, and then to the north-east, being turned by the flanks of the Appalachian mountains. The sources and *embouchure* of the former are in the same meridians—of the latter, in the same parallels. One is a river of latitudes, the other of longitudes." From these dissimilitudes it results that the banks of the Mississippi present far greater diversities of organic life and of disease, than do those of the St. Lawrence. 2. Although the sources of the two rivers are found at nearly the same elevation, those of the Mississippi reach it by much the longest routes, and in regularly inclined planes, presenting few of the lakes, rapids, and cataracts, which render the St. Lawrence so remarkable beyond all other rivers. 3. The quantity of water discharged annually by the Mississippi is much less, in proportion to the area of its basin, than that discharged by the St. Lawrence. This arises from the small quantity of rain falling on the western portions of the Mississippi basin, the sinking of water into the loose, diluvial materials that cover its rocky strata, the escape of large portions into the low and broad alluvial plains on its borders, and the evaporation consequent upon the high temperature of the regions through which the lower Mississippi flows. 4. The amount of drift-wood and softer vegetable matter borne down to the sea, or deposited along its banks, by the Mississippi, is incomparably greater than by the St. Lawrence. 5. A still greater difference exists in the amount of earthy matters which these rivers transport to the sea; for while the larger tributaries of the Mississippi, and the river itself during the lower 1400 miles, are always turbid, the St. Lawrence is always clear, owing to the firmer materials of which the bed of this last is composed, and the decanting process which the turbid waters of its tributaries undergo in the lakes they form. The consequence is, that while the Mississippi has filled up the bay or arm of the gulf, which once projected far into the continent, and is constructing a cape in the Gulf of Mexico, the St. Lawrence is still met by the tides nearly 500 miles from its gulf. 6. With respect to the transportation of ice, the condition of the two rivers becomes reversed; for while the Mississippi transports none, and is never frozen through the lower 800 miles of its course, the St. Lawrence is frozen every winter, and, below Lake Ontario, is obstructed with ice during a third of the year. The equability of the climate along this river is thus much interfered with; the opening of vegetation at and beyond Quebec being rendered more tardy, compared with that of the island of Montreal, than would be caused by the difference of latitude, owing to the quantity of ice carried down into the estuary. In summer and autumn, the temperature of the waters of the Mississippi, now greatly reduced in volume, is much raised; but the waters of the St. Lawrence, nearly all derived from deep lakes, suffer little loss of bulk in summer, and undergo comparatively little elevation of temperature. 7. The lower portions of the two rivers differ greatly in their scenery: while the traveller finds himself for two or three hundred miles of the latter course of the Mississippi in the midst of a swampy plain, the St. Lawrence from Montreal to Quebec, though having also low banks, is more and more embraced by the higher lands in their rear. "The birches, maples, and larches, here represent the cotton tree, liquid amber, live oak, and

cypress ; orchards of plums and apples are substituted for the peach, fig, and orange ; and fields of wheat, oats, peas, timothy, and potatoes, take the place of cotton, sugar, and rice plantations." 8. In one respect both rivers agree, the depressed banks of the one between Montreal and Quebec, and of the Delta of the other above and below New Orleans, being covered with villages by the descendants of the original French emigrants.

" 9. If these two rivers, with their respective geological accompaniments, had been placed respectively in each other's geographical position, their medical histories would have been widely different from what they now are. The alluvial deposits of the Mississippi would, it is true, have carried autumnal fever somewhat farther north than we now find it ; but the greatest difference from the present state of health would have been found in the south ; where a mountain range, and the almost total absence of deposits of silt and organic matter, would have nearly precluded those fevers, which the burning heat of summer and autumn now quicken into annual prevalence." (p. 432.)

Having concluded his laborious topographical survey of the valley, Dr. Drake proceeds, in the second part of his work, to the consideration of what he terms *Climatic Etiology*, under which head the circumstances of temperature, atmospheric pressure, the direction of winds, and the amount of rain, are embraced. The results of this portion of his labours are confessedly very imperfect, as the data he has as yet been able to obtain are neither sufficiently numerous nor trustworthy to afford, for the most part, any other than partial and provisional conclusions. When we consider how recently the importance of accurate and continuous meteorological observations has been acknowledged, and acted upon on a large scale, even in densely peopled countries, and in how few of these they are indeed even yet effected, we can easily imagine their imperfections and inaccuracies in regions brought within the pale of civilized usages even within the memory of the living. Dr. Drake has most industriously collected such materials as were accessible, and tabulated them ; so that their future augmentation and correction will be a matter of comparative ease, and one likely of accomplishment, now that this zealous apostle of science has awakened attention to the subject during his frequent missions.

We may extract a few of his observations upon thunderstorms. These diminish in frequency and violence from the tropical to the polar regions, occurring in every month of the year on the shores of the Gulf of Mexico, but more often in hot weather. In the middle latitudes at St. Louis, Dr. Engelmann has recorded their frequency for sixteen consecutive years,—the mean of this period being 49. A regular increase in their number takes place there from December to June, and a decrease from June to December. The extremes do not fall in the months of the least and greatest *mean* temperatures, which are January and July, but in the solstitial months, when the direct action of the sun is at its minimum and maximum. These phenomena, which thus occur as frequently as 49 per annum in N. lat.  $38^{\circ} 37'$ , decrease as we advance northward, and lat.  $70^{\circ}$  may be assumed as their zero in the Interior Valley. Discharges from the thunder-cloud to the earth are much more common in towns than in the country. No summer probably passes without houses being struck in Cincinnati, although there are many lightning-rods ; but in travelling through the woods in various parts of the valley, the author has often passed many miles without being able to find a single tree scathed. "The

buds and leaves of trees, no doubt, act as attracting points, while their sap renders them good conductors; and thus the electricity is conveyed quietly into the earth, which, moreover, is generally moist under their shade. It is a popular opinion that the beech (*Fagus ferruginea*) is never struck by lightning. I have not, as yet, seen one which has suffered." Steamboats are seldom struck, experienced seamen stating to the author that they have repeatedly known these escape, while in company with vessels which have suffered:

"The physiological effects of a thunder-storm are always considerable. They may, perhaps, be referred to several heads. 1. A sudden reduction of temperature. 2. Lowering the dew point, and an increase in its complement. 3. An augmentation in the pressure of the atmosphere. 4. A restoration of the equilibrium or neutral condition of the electric fluid. 5. The terror excited in most persons, by the near approach of a thunder gust, must not be overlooked in recognising its physiological effect.

"An upward discharge of electricity, during a thunder-storm, is not an uncommon event. When animals, in open places, remote from all elevated objects, are killed by lightning, the discharge is, no doubt, generally from the earth. But they are sometimes destroyed when near to such objects, while the objects themselves escape. Not long since, a horse and a small carriage, with two men, were struck with lightning in one of the streets of Cincinnati, while the high houses on either side remained untouched. Many years since, in that part of the city which has generally suffered most, I saw a bean-pole which had been split by lightning as far down as the surface of the ground. The fluid had probably been discharged from the earth through this pointed rod, and while passing along the moist extremity in the ground, did no mischief, but on reaching the dry shaft above, destroyed it. The following fact shows that electricity may be discharged from the earth in flashes, without producing thunder. My informant, Judge Collier, was at Montgomery, and about 8, p.m., April, 1833, saw, with many other persons, repeated flashes, or electrical explosions, at the surface of the earth, which illuminated the surrounding objects, but produced no sound. Overhead there hung a black cloud, which sent forth a sound like that of low, distant thunder; a violent tempest immediately followed." (p. 616.)

This portion of the work is terminated by an interesting chapter upon the "*Climatic Distribution of Plants and Animals.*" Passing over the climatic geography of the native plants, we may briefly notice that of some of the cultivated ones. The banana (*Musa paradisiaca*) constitutes the leading article of cultivation on the *tierras calientes* of tropical Mexico, but the author is not aware of its northern limit. Coffee has not been introduced farther north than Cuba. The sugar-cane, which attains its highest development in the torrid zone, is cultivated on the Mississippi as high as 31°. Rice is chiefly planted below 30°. Cotton is a profitable crop to 36° 30', and sometimes ripens a degree and a half farther north. Maize produces three crops between the tropics, and accommodates itself to climate to a greater extent than the plants already mentioned. It is sometimes bitten by the early frosts in lat. 39°, but can be cultivated 10° farther north. At Pembina on Red River (49°), Major Long found that it ripened, though with small yield; but at Fort Garry, one degree farther north, Captain Lefroy states that it does not reach maturity. The mean annual temperature of Pembina is about 38° F., that of its summer about 64° F. The sweet potato (*convolvulus battatas*) belongs to the south, but in sandy soils may be cultivated to lat. 40° or 41°. The Irish potato, (*solanum tuberosum*) towards the shores of the Gulf of Mexico, is deficient in size and in amylaceous development; but improves in both as we go

north, and attains its highest perfection at lat.  $43^{\circ}$  or  $44^{\circ}$ . Wheat, according to Humboldt, cannot be profitably cultivated below an elevation of 4000 feet, in the latitude of  $19^{\circ}$  N. In the states around the Gulf of Mexico,  $10^{\circ}$  or  $12^{\circ}$  farther north, it is still inferior, and improves in quality as we advance into higher latitudes, reaching its maximum of development in Michigan and Western New York, where at  $43^{\circ}$  N. lat. the mean temperature is about  $46^{\circ}$  F. Captain Lefroy informed the author that it has been successfully cultivated at Fort Liard, on Mackenzie's River, in the  $60^{\circ}$  N. lat., thus having a wider range than maize. The orange tree is sometimes destroyed below lat.  $30^{\circ}$ , but frequently produces a sour fruit to lat.  $31^{\circ}$ . The fig produces well to lat.  $33^{\circ}$ , beyond which it requires winter protection. The Pride of China (*Melia azederach*) flourishes to lat.  $33^{\circ}$ . The peach, which but partially sheds its leaves in winter around the Gulf of Mexico, produces an uncertain crop as far north as  $43^{\circ}$ . The apple, on the other hand, attains little perfection below lat.  $33^{\circ}$ , but improves as we advance northerly, to the limits of existing cultivation.

The 33d degree of latitude constitutes more than any other a climatic limitation in plants, both indigenous and cultivated—a limitation, indeed, so abrupt, as to indicate a sudden change of climate. This arises from hilly ramparts from 1000 to 1500 feet high, which stretch nearly across the northern curve of the Gulf, formed by the Cumberland Mountain on the eastern, and the Ozark mountains on the western side of the Mississippi:

“The country south of this range, from the western edge of Georgia to the southern boundary of Texas, embracing the states of Louisiana, Mississippi, Alabama, and part of Florida, consists of an amphitheatric series of alluvial, diluvial, tertiary, and cretaceous plains, having a gentle southern declivity, which constitutes the *tierras calientes* of the northern arc of the Gulf. It is not until the traveller from higher latitudes has passed the northern boundary of this amphitheatre, until he has descended below Memphis and the mouth of the Arkansas river, that he realizes, by a change in the aspect of vegetation, that he has entered a new climatic region. Thermometrical observations have not as yet been made with sufficient exactness to determine the extent of this change of climate; which, however, is made manifest as much by disease as by vegetation. In every fertile and well-inhabited portion of the country, south of its hilly ramparts, the diseases have more of a southern character than to the north. Passive congestion largely takes the place of inflammations; and malignant intermittents and remittents occur with greater frequency. Yellow fever, which has repeatedly prevailed in almost every town up the Mississippi to Vicksburgh, north latitude  $32^{\circ} 24'$ , has never but once reached Memphis in  $35^{\circ}$ , and has not prevailed at any intervening town. Thus its limits, on the whole, have been those of the live oak, cypress, and long moss; and it will not, any more than they, be found among apple orchards, wheat fields, and groves of blue ash, sugar maple, and the arborescent buckeyes.” (p. 630.)

Dr. Drake finds few materials for the indication of the distribution of animals, with the exception of Dr. Richardson's 'North American Zoology,' contained in the printed 'Report' of the British Association, and which only refers to the northern portions of the valley. The climatic range is a wide one, as would be anticipated from the geographical and hydrographical features of the valley.

“1. The general course of our mountain chains is from south to north; from which it results, that the quadrupeds of the valley, by obliquely ascending their slopes, may continue their migrations to a much lower latitude than would otherwise be practicable; while those which inhabit the mountains are also invited into

southern latitudes by the same reason. This is quite analogous to what we observe of various plants, as the apple, wheat, and other cereals; which, passing by the plains that surround the Gulf, are found on the terraces and table-lands of Mexico, within the tropics. 2. The course of our great rivers the Mississippi, the Nelson, and Mackenzie, the first of which flows to the south, the two others to the north. These rivers not only invite many fishes beyond the latitudes to which they otherwise would have been confined, but also promote the migration of certain birds into climates which they would not have entered. 3. The vast grassy plains, which stretch from near the mouth of the Rio del Norte to the mouth of Mackenzie's river, through more than forty degrees of latitude, without being interrupted by a single deep and impassable gorge, a herbless desert, or a transverse range of mountains, greatly favour the wide range of our quadrupeds. 4. The vast number of lakes which lie above the 42° parallel, together with the inland sea, called Hudson's Bay, favour the northern extension of birds of passage; and no doubt increase the number of those which would traverse the continent from south to north, if the whole surface were as dry as the plains to the west of the Mississippi.

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"It appears from these citations, that a large portion of our quadrupeds have a wide climatic range across the continent, the result, no doubt, of the unbroken continuity of the Rocky Mountains, and the great plains which lie to their east. Animals, which might have begun in the temperate latitudes, have generally advanced and become acclimated in more southern or more northern climates. The latter especially have invited them, and given to the cold regions a fauna richer in large quadrupeds than the warm, thus reversing the order of our arborescent flora. This truth is made yet more obvious when we refer to the marine mammalia of the south and north. In the Gulf of Mexico there is, I believe, no species larger than the common porpoise, which may occasionally be seen vaulting up to *passes* or ultimate mouths of the Mississippi; but the Polar Sea, which subtends the northern extremity of the valley, abounds in seals, dolphins, walruses, and whales of the largest size. The first supplies the chief articles of food, clothing, and fuel to the tribes of the Esquimaux, who inhabit the desolate regions. Thus, so great is the range of our climates, that at one extremity of the valley man draws his subsistence and comfort from the vegetable kingdom,—at the other from the animal." (pp. 631-33.)

With regard to the domestic animals, the flesh and milk of the cow degenerate below lat. 33°, *i. e.*, where the mean annual heat is 65° and above. The mule, in the same region, retains an unimpaired constitution. For the first year the horse here is weak, and possessed of little endurance, but if he survive that period, his health and strength then become good. The horses bred round the Gulf are, in general, small, but hardy. The wool of sheep, taken into the south part of the valley, degenerates. The hog flourishes in the south no less than in the temperate and northern latitudes.

The dissemination of the human race through these diverse climates is considered in the succeeding part of the work, the author here only remarking upon this point, "that man has accommodated himself more effectually to all our climates than any other member of the animal kingdom; being a *permanent* inhabitant of the whole, from the southern shores of the Gulf, where the mean annual temperature is 80° F., to the latitude of 70° north, where it is but 5° F. It appears, then, that he may become a permanent denizen of any region that will afford him sustenance."

In the Third Part of his work, Dr. Drake treats of what he terms the "*Physiological and Social Etiology*," furnishing under this head an account



of the modes of living, dwelling-places, occupations, and recreations of the inhabitants of the valley,—confining his attention, however, in the present volume, to the *Caucasian* variety of the population, with the intention of treating upon the habits and diseases of the African, Indian, and Exquimaux varieties, in separate and subsequent dissertations. This Caucasian population has been derived directly or indirectly from Western Europe, by successive migrations from France (1534—1769), Spain (1528—1763), Britain (1763), and the Atlantic States (1761). During the last quarter of a century, there has been a direct and increasing immigration of northern Europeans, the majority of whom have settled north of lat.  $39^{\circ}$ , nearly all having come from kingdoms north-east of France, *i. e.* about lat.  $50^{\circ}$ . The author thus enumerates these more recent immigrants in the order of their numbers: 1. The *Germans*, from various parts of Germany, outnumber the immigrants from any other part of Europe, few settling in the slave States, and the majority dispersing over the country, and readily amalgamating with the existing population, in place of remaining in the large towns. 2. The *Irish*, next in numbers, more disposed to inhabit cities, and less offended at slavery, are found in large numbers equally in New Orleans and Mobile, and the towns to the northward, where they are more numerous than the Germans. 3. The *English*, scattering over the valley, and bearing a close resemblance to its inhabitants, are soon confounded with them. In Canada, and the lead-mine regions of Illinois and Wisconsin, they are numerous. 4. The *Scotch* are found both in town and country, chiefly from the Lowlands, and more abundantly in Canada than the States. 5. The *Welsh*, more clan-nish, have settled chiefly in Cincinnati and the south-east of Ohio, where many of them are employed in mines and collieries. 6. The *Norwegians* form a new stream of immigration, terminating at about lat.  $43^{\circ}$ . 7. The *Poles* are dispersed in considerable numbers, being chiefly men, inhabiting towns. 8. The *Jews*, for the most part English, German, and Polish, are found in the cities from Quebec to New Orleans, and especially in Cincinnati.

The physiological modifications which may be expected to arise from this commingling of races in a new climate, must be regarded as of prospective rather than as of actual occurrence; and Dr. Drake contents himself with enumerating the various circumstances which must prove influential in effecting these, as intermarriage, change of climate and food, and change of political, moral, and social condition.

1. *Intermarriage.* The entire frontier of the valley, from Quebec to the Gulf of Mexico, presents a mixed race of whites and Indians, which is gradually lost in the population residing immediately within the boundary. The readiest amalgamation with the Indians takes place among the northern French and the southern Spanish creoles; but the Anglo-American immigrants from the States have at all times, in the absence of war, shown a disposition to such union. Wherever a Negro population, whether slave or free, exists, the same coalescence is observable; so that the streets of all the towns are thronged with mulattos, quadroons, and other mixed breeds, and the Caucasian blood is slowly but constantly imbibing an African element. In willingness for this junction, first stand the Spanish Creoles; then the French Creoles; then some classes of British and German emigrants; and lastly, native Anglo-Americans. In Canada, intermarriage between the French and British population, though limited

by the prejudices of race and history, is not uncommon, especially in the towns. So frequent are marriages between the French and Anglo-Americans along the middle portions of the Mississippi, that the former element is fast disappearing. A similar rapid absorption of the French Creole race has been going on on the lower Mississippi and northern coasts of the Gulf of Mexico, since the cession of Louisiana; and, in the same region, the Anglo-American race has been long receiving, either directly or through the French, a tincture of Spanish blood. Intermarriages of the English and Irish immigrants with the indigenous population, are common events; and the amalgamation of the German and Norwegian immigrants is only delayed by the obstacles offered by a difference of language. The immigrants from the Atlantic States and their descendants freely intermarry, many of their sectional prejudices soon becoming moderated in the valley. A large proportion of immigrants of both sexes are single, and they usually marry the children of the resident population.

"From all this it follows, that the world has not before witnessed such a commingling of races. Those of England and the Atlantic States, the most complete of modern times, bear no comparison with ours; and, if we ascend to the earliest historic periods, no case of equal complexity is met with. The Roman empire, it is true, was greatly compounded; it was, however, an assemblage of distinct nations, between which there was little, in many cases no, social, nor even commercial, intercourse. It was an aggregate: ours is a living compound, as yet in the forming stage. Three out of five varieties of the human species, with all the important races that belong to one,—the Caucasian, or overruling element,—cannot fail, in the end, to give a new physiological and psychological development.

"In their western migrations from the sources of the Tigris and Euphrates to the banks of the Mississippi and the shores of the Gulf and Lakes, tribes and nations have been governed by a law of increasing social amalgamation. The head of the Mediterranean Sea presented greater diversity than the plains of Chaldæa—Greece still greater—Rome went beyond her—the population of Western Europe is still more compounded—that of our Atlantic States, diversified in a degree yet higher—that of our Valley beyond all. Thus, the union and living coalescence of nations have been in the direct ratio of time and distance from the birth-place of the species. The course has been westward, bending to the north in Europe, but again, as we have seen, inclining to the south, to reach America. Dr. R. Paterson, of Philadelphia, has investigated its direction in the United States for fifty years. In 1790, the centre of population was near Baltimore, Maryland; and in 1840 it was in Morgan County, Virginia, both in the same latitude. Thus it appears, that the curve of migration for the United States still runs nearly from east to west.

"But the influence of the latter element is not at an end. The *Great Central Valley of North America* is the *last* crucible into which living materials, in great and diversified streams, can be poured for amalgamation. The double range of mountains which separate it from the Pacific leave too little space for an empire on the shores of that sea; and the detached communities which may there grow up will be but deviations from the homogeneous millions, with which time will people the great region between the Appalachian and Rocky Mountains, which is thus destined to present the last and greatest development of society." (p. 647.)

2. *Change of Climate.*—The people of Western Europe have, in emigrating to North America, generally made  $10^{\circ}$  of southern latitude; but yet they live in an average annual temperature very much the same as that to which they had been accustomed. There is, however, a far greater difference between the summer and winter temperatures than in Western

Europe, and sudden and extreme variations are far more common. The climate, too, is dryer, and the atmosphere more electrical.

"To these climatic conditions we are bound to admit a modifying influence, which, if I mistake not, is perceptible in the loss of a ruddy complexion, in a diminution of the capillary and cellular tissues of the face, and a consequent reduction of the convexity of the cheeks, with an increasing tendency to dark hair,—in short, the production or further development of a bilious temperament. Without insisting on the accuracy of these special observations, I am convinced, that whatever tendency exists is not in the direction of the sanguine temperament."

3. *Change of Food.*—The inhabitants of the valley have a fuller diet than those of Europe; their food being both more complex and containing a larger proportion of animal substance. However slow the immigrants may be to adopt other customs, they rapidly fall into that of the valley, of eating animal food freely three times a day; and when we consider that, as in the case of some of the Irish, the tasting it once a week was heretofore an exceptional occurrence, we can believe our author when he says, that such inordinate indulgence, joined with a great variety of ill-cooked, half-masticated vegetables, is often immediately injurious, and may hereafter generate constitutional peculiarities.

4. *Change of Social, Moral, and Political Condition.*—This change, with respect to the great mass of immigrants, is immense, and in most respects highly gratifying, so that Dr. Drake naturally attaches great importance to its future physiological influence. The immigrant escapes from the crowd that hems him in at home and impedes the development of his natural powers, and is transplanted to a land where the grandest natural objects replace the works of art among which he has always lived. He finds ample space for locomotion, and, under the influence of even slight motives, he makes long journeys and frequent removals, and engenders new associations. The monotonous routine to which he had been condemned is broken through, and he finds himself where change and enterprise are facilitated and encouraged, and where failures are not attended with ruinous consequences, but are quickly recovered from. He sees the way to wealth open to his energies, and the love of property exerts its magic influence upon a nature that perhaps heretofore hardly hoped for bread. Every variety of religious culture is open to him; and he at once is endowed with the privileges, the responsibilities, and the elevating influences of a political existence.

"The variety and amount of emotion, the excitement of passion, and the activity of thought developed by this new condition, are great in proportion to his previous torpor,—as vegetation advances more luxuriantly after a cold than after an open winter. This is the true reason why our immigrant population are so eager to plunge into the party strifes which are for ever heaving the bosom of our society.

"Such are some of the new social circumstances under which the transplanted population of Western Europe live in our great Valley; and the physiologist cannot doubt, that the mental states, intellectual and emotional, generated and permanently sustained by them, will in successive generations work out changes in innervation, and co-operate with the physical causes which have been discussed, in creating a type of constitution different from that of Europe, or any that has gone before it in any country." (p. 650.)

*Modes of Living in the Valley.*—Although the author has already discussed this topic in part, while considering the causes inducing changes in the constitutions of immigrants, yet he again adverts to it, in reference

to its influence in the production of disease. Of the three meals usually taken, he believes that breakfast, and not dinner, should be the principal, the stomach being better able, after a long repose, and while both body and mind are tranquil, to dispose of a large meal, than in the midst of the heat and turmoil of the day. A hearty breakfast seldom induces oppression and drowsiness like a hearty dinner, the succeeding hour being in general one of pleasing and efficient excitement, contrasting with the sleepiness, dullness, and feverishness following the full dinner. The inhabitants of the valley habitually eat too much and too fast, and consume far too large a proportion of animal food, and that in summer and autumn, as well as in the colder and healthier seasons. Even children are usually allowed meat three times a day. Large quantities of vegetables are also consumed, so that the total amount of food taken is very great. The culinary art seems quite in its infancy, so that food is rendered often highly indigestible by the faulty modes of preparing it.

*Drinks.* *Tea* is used in every part of the valley, but especially the north, green being preferred to black. *Coffee* is, however, gradually banishing it at breakfast, and confining it to the evening meal. The chief drawback to the use of coffee is the bad manner in which it is prepared, the French, in America as in Europe, alone seeming to be conversant with its due and proper making. The Creole French, from New Orleans to Quebec, use it to the exclusion of tea, and even the poorest of them understand its preparation.

“There is much reason for believing, that an early cup of coffee, in summer and autumn, is protective against the fevers of the southern parts of the Valley. Certain it is, that the French population (among whom it is common to drink it on rising) are less liable to them than the Anglo-American; but, as they occupy the oldest-settled portions of the country, which, *cæteris paribus*, are least affected, allowance must be made for that. In various parts of the Valley coffee is beginning to supersede ardent spirits, as a means of protection and support under fatigue and great exposure to the elements; for which purpose all who have made the experiment regard it as preferable.” (p. 661.)

*Alcoholic Beverages.*—It is gratifying to find that the consumption of these is undergoing great diminution in the valley. Fifteen or twenty years ago, “morning bitters” and “mint juleps,” the basis of which was commonly whiskey, were habitually consumed by persons of both sexes and all ages, under the pretext of preventing autumnal fever. So, too, at one time, the practice of taking whisky or brandy and water before or immediately after dinner was universal. Both these customs are rapidly becoming obsolete, and yet it is not found that the autumnal fever commits greater ravages, or that digestion is less efficiently performed. The consumption of wine, too, is greatly on the decline, not being now, as heretofore, regarded as indispensable at morning calls and evening parties, and being taken far more sparingly at dinner. Sherry and Madeira, adulterated with brandy, are the wines in general use when taken, except in Louisiana and other parts of the south, where, especially among the Creoles,—the most temperate portion of the population,—much claret is consumed. Notwithstanding the disuse of drinking usages in society, much guzzling still goes on at the dram-shops of the cities; but the fact of the victims of this habit being compelled to resort privately to these places for the indulgences they formerly could command in society, proves

the progress that public opinion is making in the matter. In families, also, the consumption of beer and cider has diminished ; but large quantities of beer are still consumed by the English and German immigrants.

Dr. Drake is an opponent not only to the habitual, but even the occasional use of alcoholic drinks ; maintaining that they possess no preventive power, as, indeed, is now generally acknowledged, against the diseases incidental to the valley ; and that, as regards exposure to the elements, experience has determined the superior efficacy of food and coffee under cold or wet, and of tea or lemonade under great heat. His testimony upon this point is of importance, inasmuch as his sphere of observation has been so extensive, and his opportunities of hearing the opinions of practitioners in very different regions both numerous and prolonged. Of the pernicious effects of over-indulgence in them in these latitudes, he offers abundant evidence. He does not affirm that individuals who abandon themselves to this are more liable to contract prevalent diseases, though, probably, he might very safely have done so ; but when they are attacked, such persons are far more liable to die. He believes such beverages to be much more pernicious to the constitutions of the young, than to those in middle and advanced life. "Very few young men with habits of excessive drinking reach the meridian of life, being cut off by convulsions, apoplexy, or some disease rendered incurable by their alcoholic cachexy." There are very few female drunkards in the valley, except among the lower classes of Irish immigrants ; and the only woman the author has known die of delirium tremens was from England. "Among the hygienic means of averting the habit of excessive drinking, I know of none equal to the use of tea. To this we may, in fact, attribute the escape of our female population from intoxication ; and I have rarely seen a young tea-drinker of the male sex become, in later years, the victim of alcoholic stimulation."

In reference to this subject, we may notice one of the incidental facts that are so thickly interspersed in his topographical description. He is describing a terrible epidemic of autumnal fever, which ravaged the Maumee basin, at the south-west angle of Lake Erie, in 1838, and which ensued upon a *drought* of unexampled duration, during which the marshes, pools, and shallow rivers were all dried up.

"Under this great drying process it was, that the ordinary autumnal fever was raised into such an epidemic as had not been known before. All the accounts concur in representing, that the localities which were ordinarily the wettest, suffered most, and *vice versa*. The labourers (employed in the excavation of a canal at that period), 400 or 500 in number, were chiefly Irish, who lived in temporary shanties, while some occupied bowers formed out of the green limbs of trees. It does not appear that a greater number of these operatives suffered than of the resident population ; but a far greater proportion of those attacked died. Professor Ackly gave me a fact, which deserves to be recorded. One contractor kept a liquor store, and sold whisky to all whom he employed, which was drunk freely by themselves and their families. The mortality among them was very great. Another lodged his operatives on straw beds, in the upper rooms of a large frame house ; made them retire early ; kept them from the use of whisky ; and nearly all escaped the disease." (p. 364.)

*Tobacco*.—The abundant use of this substance in smoking is a predominant evil in the valley ; irritating the nervous system, impairing the digestion, and arresting nutrition, so that those addicted to it are often



emaciated and feeble. Much as the author disapproves of the practice, he cannot agree with those who consider it as equally deserving of reprobation with the use of alcoholic drinks, inasmuch as its effects in engendering a diseased state of the system are far less powerful. The habit of indulging in it is generally acquired early (sometimes at seven years), and is so by a very great majority of smokers before they are twenty-one, differing in this from alcohol, the habit of resorting to which is often formed after middle life. Fewer, too, leave off the use of tobacco than of alcohol, but it may be doubtful how far this is dependent or not upon an awakened sense of the evils of the latter. The attempt to abandon it often gives rise to a nervous irritability, only to be allayed by its resumption.

Snuff-taking is not prevalent, except among the French of Louisiana and Canada; but a horrid practice prevails in many parts of the south, where women and girls rub their teeth and gums with the powder. "They chew the end of a green twig, until they mash it into a kind of brush, which they dip into the snuff-box, and then rub their teeth with it. This operation is known through that region under the name of 'dipping.' The practice, like that of eating slate-pencil, chalk, or clay, often spreads through families and female schools, so as to constitute a serious and disgusting evil."

We do not find anything, among the author's observations on the clothing, habitations, occupations, and amusements, of the inhabitants of the valley, calling for special notice; and we will only quote the observations with which he concludes the first book of his work.

"Our etiology is now brought to a close. If the reader has found its perusal a work of labour, he will be prepared to estimate the amount which has been required to collect, arrange, condense, and give unity to so many diversified facts, connected with a country of such vast extent, and races of people so various. In doing this, I have introduced nothing which I did not consider necessary to a full understanding of the diseases which are to come under our consideration; for all peculiarities of constitution, both corporeal and mental, exert a modifying influence on disease. In this country these peculiarities are not yet largely developed, but we may study their causes, and, as far as possible, infer their effects; which our distant successors will see in their full development. A synthesis of varieties and races is going on; and the result, I may here repeat, must be a national constitution—physical and mental—of which the Anglo-Saxon, itself a compound, will be the basis and governing element. The physicians of a future day will see, what we cannot now, a prevailing temperament—a stature, form, complexion, and physiognomy—characteristic of an indigenous, but greatly compounded race; with its own physical, intellectual, and moral constitution; its special liabilities to and exemptions from disease; its national idiosyncracies, and the required peculiarities of hygienic regimen, and therapeutic treatment. In the course of his development, what hereditary diatheses may disappear, and what new ones take their places; what new maladies may arise, or old ones cease or become greatly modified, under the joint influence of mingled blood, of climate, water, occupations, modes of living, customs, and moral, social and political influences, cannot be specified; but a few predictions may be hazarded:—1. Autumnal fever will decrease, and typhus and typhoid become more prevalent. 2. Gout will occur oftener. 3. The diseases produced by the intemperate use of ardent spirits will diminish. 4. Consumption and scrofula will increase. 5. Apoplexy, palsy, and epilepsy will become more frequent. 6. Diseases of the liver will become less, and those of the mucous membrane of the bowels more prevalent. 7. Mental alienation will be more frequent." (p. 701.)

The portion of Dr. Drake's work which we have hitherto examined, is only introductory to his main subject, the description of the *Diseases of the Interior Valley of North America*—although, in the topographical portion, many observations respecting these are incidentally met with. As this occupies 700 of the 860 pages of which his first volume consists, he is enabled to treat only of the autumnal fevers of the valley, intending to take up, in succession, in the future volumes, the yellow fever, typhus, the eruptive fevers, and the phlegmasiæ.

**AUTUMNAL FEVER.**—The author considers this the preferable term for the fevers known in various parts of the valley as autumnal, bilious, remittent, intermittent, congestive, miasmatic, malarial, marsh, malignant, chill-fever, ague, dumb ague, and *the fever*, seeing that it involves no etiological or pathological hypothesis, and includes the various modifications. Although the different forms often differ widely in their occasional manifestations, yet in their prevalence at the same times and places, their frequent conversion into each other, the *sequelæ* they give rise to, and the treatment they require, evidence is found that they are the mere varieties, the common offspring of the same specific remote cause, whatever that may be.

**Geographical Limits.**—Being an endemic of hot climates, its southern limit is not found within the sphere of the author's observations. Its eastern boundary is the Appalachian Mountains, as far as lat. 33°, below which it has no other eastern limit than the Atlantic. To the south-west the Cordilleras of Mexico and the southern rocky mountains constitute its boundaries; while in higher latitudes it ceases on the great plains of the western desert, long before these mountains are reached. To the north it does not prevail epidemically above the forty-fourth parallel, and ceases to occur even sporadically about the forty-seventh.

In reviewing the circumstances which are necessary for the development of the fever within these limits, it is found, that all things being equal, it prevails most where the amount of *dead organic matter* is greatest, and least where it is least. A variety of observations show that it frequently occurs on first breaking up the soil, the long-continued cultivation of which is usually attended with its diminution. The agency of forests has been differently described, and is not as yet accurately ascertained. Wherever the malady prevails, the *surface water* is proportionally extensive; but the agency of this alone does not suffice, as water abounds beyond the geographical limits of the fever. So too, while a *high temperature* (above 60°) is essential to the production of the fever, this alone will not suffice to induce it in localities wanting the two other conditions.

Passing over the author's account of the speculations which have been framed in explanation of the nature of the poison inducing the fever, and of the channels by which it obtains access to the economy, as possessing no novelty, and derived from sources familiar to our readers, we proceed to his more original observations upon the history and treatment of the varieties of the disease. In the incipient stage these are not always distinguishable, and it is only as the malady advances that its type is then declared—the primary division being into intermittent and remittent. *Intermittents* may be mild, simple, and accompanied by a perfect intermission, and not dangerous even when left to themselves. But again, they

may exhibit a deep or protracted cold stage, with imperfect reaction, constituting the malignant or congestive form; in the first, second, or subsequent paroxysm of which, the patient may expire. Thirdly, the disease may assume an inflammatory character, with a diminished cold and a prolonged hot stage, running at length into the remittent type. There are the same varieties of *remittents*. These may be simple, and may terminate without much active treatment, oftener in the middle than the southern latitudes, in health or in intermittents. Others from the commencement, or in their progress, exhibit signs of inflammation of some organ, and have a tendency to the continued type; while in the south, a form of the disease has received the name of malignant, of much rarer occurrence than malignant intermittent, from which it differs by the absence of a regular *apyrexia*. The first two varieties often terminate in intermittent. Intermittents, left to themselves, rarely cease, till they have continued a long and indefinite time, but they can be arrested at any period of their course. Remittents, when mild, may cease of themselves, but are not so susceptible of being cut short in their early stages. We can assign no reason why the same cause in some persons induces a remittent, and in others an intermittent; in some the simple, and in others the malignant form, while yet all may inhabit the same locality.

“It might be presumed that the statistics of these varieties of fever would throw light upon the subject. The table presents the relative proportion of intermittents and remittents at twenty-six military posts. If these be divided into the southern, middle, and northern groups, we find that for the southern the remittents make twenty-one per cent., the middle fourteen, and the northern twenty per cent. Thus it does not appear that temperature exerts an influence in the relative numbers. But may not humidity? Six posts, around the Gulf of Mexico, give for remittents twenty per cent.; seven on the Lake shore give thirteen; and eight along, or west of, the Mississippi, where the atmosphere is driest, give only ten per cent. From these numbers it would appear, that humidity increases the proportion of remittents. But can we adopt this conclusion? I think not; for ordinary observation has shown, that remittents are more common than intermittents on dry ridges, while in deep vallies and other humid localities, intermittents prevail. It must be, then, that while the army reports may be correct as to the aggregate, they are not to be relied upon for the relative number of intermittents and remittents. The mean of the twenty-six posts is about eighteen per cent. of remittents; but, from several yearly reports, kindly communicated to me by Dr. Silas Ames, of Montgomery, N. L. 32°, I find the proportion in his practice is about forty-three per cent. I have met with a transcript of the records of the Charity Hospital, New Orleans, for seven years, by Dr. Farmer, which presents the proportion at but ten per cent. ! Such discrepancies show how little reliance can be placed on the attempted classification of autumnal fever into remittent and intermittent.” (p. 736.)

1. *Simple Intermittents*.—These require no description, being identical in characters with those of other countries. They never prove fatal, except secondarily by the lesions to which the repetition of the paroxysms, when too long neglected, give rise. Many practitioners delay treatment until several paroxysms have occurred, believing that cases so managed are less liable to relapse; but the author does not approve of this practice. He sanctions that of others, which was once universal, though now rapidly falling into disuse in the south, who employ a *preparative* treatment prior to the use of quinine, consisting of venesection, (when the patient is robust and the case recent,) or emetics and cathartics. Many practitioners adopt Mackintosh's practice of bleeding in the cold stage. In the administration

of the quinine, the most diverse opinions are entertained as to the best period; the author, with many others, preferring a short time prior to the access of the paroxysm. He believes, too, that in the early stages, where our object should be to cut the disease short, large doses should be given. Thus many of the most eminent physicians have found it the best practice to administer from five to ten grains on the decline of the fever, as much in eight hours after, and the same before a paroxysm. Still the majority of practitioners administer but one or two grains at short intervals, a practice which is indeed usually successful, and in protracted cases is probably the best. As *adjuvants*, calomel is given when there is biliary derangement, and nitre or ipecacuanha when the excitement is considerable, or when the drug is given during the hot stage; and of opium, administered in sufficient dose an hour or two before the paroxysm, to induce a state of narcotism prior to the chill, Dr. Drake and others entertain a very high opinion. Drs. Henry and Merriman, of Springfield, give from three to six grains of solid opium, with as much quinine, and observe a more certain arrest of the paroxysm than when they omit the opium and double or treble the quinine. The author believes that the best means of preventing relapse is, after having broken in upon the paroxysm by the quinine, to resort to the use of the bark itself, and continue it for long after. The two are by no means quite identical in their properties, the bark being destitute of any diaphoretic property, and acting as an astringent and tonic. A greater reduction of the powers of the system is requisite for its employment, than for that of quinine; but in cases in which the vital energies are seriously impaired, it is the appropriate medicine. Many trials have been made in America with indigenous bitters as substitutes for cinchona, but with only partially favorable results. The increasing price of that drug, and the rapid process of extinction which the trees supplying it seem to be undergoing, render the investigation suggested by the author into the virtues of these substitutes a matter of the greatest importance. In this point of view we should have been glad of some more definite information from him, as to the extent to which *arsenious acid* has been employed in the valley. What little he says of it is in its favour; but we are rather surprised to find him giving a preference to the solid form. He combines one grain with four of opium, and divides the mass into sixteen pills, giving three or four in the twenty-four hours. In obstinate cases, sixteen grains of quinine added to the formula renders it a very efficacious one. We believe that arsenious acid cautiously administered, in the fluid form, is a valuable substitute for quinine, both in ague and neuralgia, and that it by no means commands the attention it deserves as such. M. Boudin's investigations into its employment among the military in France and Algeria have led him to favorable conclusions of great importance, both in a sanitary and economical point of view.

2. *Inflammatory intermittents*.—All parts of the valley, though the southern least, every autumn present examples of these, the proportion to simple cases varying much in different years. In such cases, a tense pulse, prolonged hot stage, imperfect intermission, and local symptoms referable to the organ affected, are met with. This organ is, in the great majority of cases, the spleen, and occasionally the liver, or the mucous membrane of the stomach. So, too, the brain or lungs may become accidentally affected. The effect of a supervening inflammation is to increase the

acuteness of the fever and transform it into a remittent; this change, however, occurring more readily in the case of pulmonary or cerebral inflammation, than in that of the spleen, liver, or stomach, which appear to depend upon the same remote cause as the fever itself. In treating this form of intermittent, antiphlogistics, proportioned to the activity of the inflammatory action, must precede the use of the quinine; and, indeed, if this drug be prescribed indiscriminately, when there is a prevalent atmospheric constitution of an inflammatory character, and without preliminary venesection, though signs of inflammation may not actually be obvious, many cases will be aggravated by it, and others will resist its influence.

3. *Malignant intermittents*.—To this head the author refers all the intermittents met with which are not reducible to the two former sections, and it embraces forms of the disease characterised by greater differences than prevailed among them. They have, however, all a common character of anomaly or irregularity in their course. In all, the cold stage especially predominates; and a downward tendency of the vital actions prevails. They occur at the period when epidemics especially rage, and in localities most subject to the fever; and when not arrested by art, they terminate fatally and rapidly. Of course, this form never exclusively constitutes an epidemic, but is mixed up with the other varieties. In the localities where it especially prevails, it has, since the date of the invasion of the epidemic cholera, 1832-4, been on the increase. No age or sex seems to possess especial liability or immunity. The author believes it to be more difficult to present a graphic sketch of the symptoms of this disease than of any other.

“This results from several causes. 1. Their number: all the functions being morbid. 2. Their simultaneous occurrence, as if the whole of the organism had been affected by the direct action of the remote cause at the same time. 3. The suddenness of their occurrence on the access of the paroxysm; presenting in a single hour, a transition from a state apparently bordering on health to one of impending dissolution. 4. The deep involvement of one great organ in one case, of a different one in another, and a consequent modification of the symptoms. Compared in the diversity of their phenomena with the most malignant cases of scarlatina, typhus gravior, or epidemic cholera, they are decidedly more difficult to portray in a methodical and faithful manner than either. Moreover, their malignity sometimes shows itself by the slightest possible anomaly. Thus, a partial numbness, or a coldness of the great toes, instead of a regular chill, or a disposition to sleep at the access of the paroxysm, may be all that suggests anything more than the most harmless intermittent. Hence, they stand connected, on the one hand, with a simple ague; while, on the other, they graduate into the malignant remittent type in such a manner that a separate description would scarcely be worth the trouble, were it not that a series of morbid states, however intimately catenated, must be studied in its links, before it can be comprehended in its entirety.

“By far the greater number of cases begin as regular intermittents, with a cold fit, too slight to excite more than a moderate shivering; such, for example, as ushers in a simple remittent. The cold stage is not followed, however, by the well-developed and prolonged hot stage of that variety of fever, but by one so inconsiderable, that the patient in many cases is soon upon his feet, and often resumes his business until the next day, or the day after that. If nothing have been done, the second paroxysm will be more severe; his coldness will be greater, and more prolonged, yet not productive of a shake: he may have a considerable degree of drowsiness, or dyspnoea, with a sense of thoracic oppression; his stomach may become irritable; with a sense of epigastric sinking; or some topical sweating may



show itself. To these symptoms, but more slowly than the day before, will succeed a reaction of moderate force; and when it ceases, the patient, if not alarmed, will be again out of bed, and perhaps occupied. The third and even the fourth paroxysm may thus pass away, each, however, presenting an increase of intensity in the symptoms, and a full development of them be reserved for the fifth. But this is rare: and, in the majority of cases, the third fit not only discloses the danger of the patient, but often proves fatal; or he struggles through it, to die in the next. The disease does not, however, always advance in this gradual manner. Almost every physician, where it prevails extensively, has met with examples of fatal termination in the second, and sometimes in the first paroxysm. Still further, cases of the most simple kind, which through several occurrences have shown no sign of malignancy, have, under the unadvised operation of an antimonial emetic, or an active saline cathartic, proved fatal in the next paroxysm. When the dangerous or fatal paroxysm comes on, the functions of the whole body seem blighted." (p. 759.)

Pathologically, a malignant paroxysm is little else than the undue prolongation of an intense degree of the cold stage of an ordinary intermittent. The blood repelled from the surface produces pernicious engorgements and obstructions of the great internal organs. Its aeration is defective; and the power of generating caloric is diminished, at the same time that its escape from the body is favoured by the copious exhalation which takes place. The patient dies under the combined influence of depression of the vital powers, and the consequential or accidental engorgement of some great organ, which has procured for this form of the fever the epithet of *congestive*; while, in the event of a partial reaction occurring, and life being prolonged for a few paroxysms, in some of the engorged organs a low degree of inflammation may be set up.

In treating the *paroxysm*, some practitioners in the north, encouraged by the success of *venesection* in the cold stage of ordinary intermittents, have employed it in this form for the purpose of inducing reaction. The advantage of the practice is, however, very questionable in cases in which reaction is really obtained with difficulty. Emetics employed for the same purpose, are now very generally discountenanced, especially tartar emetic, as are also, of late years, drastic purgatives. A resort to stimuli, both external and internal, is almost instinctive; but true reaction, even by the agency of the most powerful of these, is by no means easily procured. The cold affusion is spoken of in high terms, as a powerful agent to this end, by some of the southern practitioners; but it has not found favour with the majority. Every variety of internal stimulus has been also administered; opium being the medicine which, perhaps, has been more constantly employed than any other, and proving, if not obviously beneficial, at least harmless, when the state of the brain did not contraindicate its use, and being of great value, given in very large doses, when watery diarrhoea is present. Quinine has been abundantly given during the paroxysm, with, however, very unsatisfactory results; many practitioners observing the powers to become even lowered during its employment. A great portion of the danger of the paroxysm consisting in the engorged state of the internal organs, measures must be adopted to relieve those of them which seem especially oppressed. In the case of congestion of the heart and lungs, but especially of the brain, *venesection* may be required, and at all events, local bleeding and counter-irritation are indicated.

The concurrent opinions of practitioners in all parts of the valley, declare *quinine* to be an effectual remedy when properly administered dur-

ing the *intermission*. When it fails to prove so, this usually arises from the fact, that the interval is too short and imperfect in quotidians and double tertians to allow the drug to make its proper impression on the system. In other cases, the persistent hyperæmia of some important organ impedes its operation, or the stomach may be too irritable to retain a sufficient quantity. The drug, again, may be administered in too small doses, experience has shown, that the doses which are efficacious in the ordinary intermittents are of no avail in the malignant, and quantities which at one time would have been regarded as destructive, are now almost universally administered. The doses given by some practitioners are now indeed enormous, as from ten to sixty or eighty grains at once. Others, however, believe such quantities uncalled for; and the author considers a scruple taken during the intermission is in ordinary cases sufficient; but in threatening cases, he, too, recommends from forty to sixty grains in the same space of time, a larger portion of the entire quantity being administered about two hours before the expected paroxysm. *Opium* proves a powerful adjuvant to quinine, when given in large doses; but when diarrhoea is not present, it may be reserved for the last dose of quinine prior to the paroxysm. During the early period of the intermission Drs. Henry and Merriman do little or nothing; but three or four hours before the chill give four grains of opium and eight of quinine, and with almost invariable success. Others give only two grains.

4. *Simple and Inflammatory Remittent Autumnal Fevers*.—A just conception of the symptomatological relations between intermittent and remittent fever, may be formed, by supposing the cold stage of the former reduced to a mere chill and the hot one to become aggravated and prolonged. Mild attacks in good constitutions, will often, when little or nothing is done, terminate in recovery or pass into a regular intermittent. In more violent attacks, inflammation of some viscus may soon be produced; that of the spleen, liver, stomach, duodenum, brain, and lungs, occurring in the order of frequency in which they are enumerated. Patients who die within the second week, generally do so from inflammation. When this period is passed, the periodicity of the disease may be lost, and typhous symptoms become developed and increase until a fatal termination occurs in from three to four weeks; sub-acute inflammation, especially of the brain, being generally present in such cases. In other cases again, especially in the south, after a few regular paroxysms, the hands and feet are observed to continue cold through the hot stage, indicating, with other signs, persistent congestion of the great organs, and the existence of a lurking malignancy. In the north and the well-cultivated middle latitudes, the disease often presents, almost from the beginning, a tendency to a continuous type, having the characteristics of the *synochus* of Cullen. The simple and inflammatory remittent fever, in fact, constitutes especially the fever of the temperate climate of the middle latitudes rather than of the northern, where a tendency to the continued form prevails, or the southern, where the malignant or congestive type is oftenest manifested.

*Treatment*.—The author believes that a free and early venesection, before any inflammation is set up, is the best step to take for the conversion of the remittent into an intermittent form; after which, and the use of a purgative if required, the quinine may be at once and freely given. Ten

grains, with one or two of opium, and, on account of the usually accompanying biliary derangement, ten grains of calomel, constitute the formula to commence with; repeating the quinine, with or without the other ingredients, at not very long intervals. The longer the fever has continued, the less is the quantity of blood that can be drawn without impairing the vital energies. When inflammation arises early, it generally yields to a copious bleeding and quinine; but when it is late in becoming developed, it often sets all treatment at defiance. Dr. Drake brings forward a large body of evidence, showing the general abandonment by physicians, in all parts of the valley, of the old practice of submitting the patient to a long preparatory evacuant treatment before administering the quinine, and the advantage which has accrued from the early employment of this medicine.

5. *Malignant Remittent*.—This is the most dreaded form of the autumnal fever; for although malignant intermittents, left to take their course, terminate fatally, yet by treatment they are curable; while under every known treatment, the malignant remittents are often fatal.

“There is no danger of confounding a case of malignant with one of simple or inflammatory remittent fever; for in the former, certain symptoms which belong to the cold stage continue in the hot, and even run through the intermission. 1. The pulse does not rise in fulness and in force during the exacerbation, but remains undeveloped, being generally small, frequent, weak, and more or less variable. But when the remission begins, it generally improves in every quality; yet it does not become as healthy, as in the remission of a simple or inflammatory case. 2. The feeling of abdominal oppression, and the anxiety, restlessness, and gastric irritability, are deeper in this than any other form of remittent fever; and these symptoms never entirely cease in the remission. 3. A coldness of the hands and feet, or of the ends of the toes and fingers only, continues throughout the hot stage, while the trunk of the body and the head are in high fever heat.

“Malignant remittents may be distinguished from malignant intermittents: 1. By presenting remissions only. 2. By showing less reduction of temperature. 3. By the comparative absence of cold sweats; and 4. By more delirium and less apoplectic drowsiness. With these exceptions the description of malignant intermittents, already given, answers very well for malignant remittents. In fact, the symptomatic diversity between them is chiefly the difference between intermission and remission, between cessation and abatement. Yet this difference is indicative of pathological modifications, which, from their obscurity and danger, demand a rigid investigation. In algid intermittents, the feeling of thoracic oppression, the dyspnoea, the thirst, and the icy coldness of the limbs, are either followed by death in a brief period of time, or they cease, and a comfortable intermission follows. In soporose intermittents, the apoplectic stupor ends in death, or the patient revives at the end of the next paroxysm, and remains free till the recurrence of the next. In both forms there is such a complete suspension of morbid action, such a restoration of healthy function in the internal organs, that the patient seems almost free from disease, although the next paroxysm may prove fatal. He has neither fever, congestion, nor inflammation; but there is, in his system, a disposition to fall again into the pathological state of the preceding day; and the cure consists in changing or destroying this disposition by the known antiperiodics.

“Now in malignant remittents, there is no time when the fever is absent; and whatever irritations or congestions are formed in the cold stage, whatever inflammations are set up in the hot stage, remain, though moderated in degree throughout the remission. Their continuance is, perhaps, at once the reason why intermissions do not take place, and the cause that this form of fever is not as curable as the intermittent. Whenever, in simple remittents a complete intermission is effected, the antiperiodic puts an end to the disease, as certainly as if it had been originally of that type; and we may presume, that if a perfect apyrexia could be

brought about in malignant remittents, they would be as easily cured as malignant intermittents." (p. 796.)

For the excitement of reaction, Dr. Drake believes that moderate general or local bloodletting (especially cupping along the spine), and cutaneous stimulation, when it can be excited, which it often cannot, are the best adapted means; and he attributes great benefit to the emollient effect of streams of tepid water allowed to flow over the head or abdomen, when irritation or inflammation of the internal organs is present. Many practitioners also have found signal benefit from the emetic properties of the *Lobelia inflata*, which excites vomiting without inducing prostration. Cautious purgation only is admissible. It is not until the remission, by the adoption of these means, has been rendered more complete, that quinine, in small doses, can be safely given; increasing these, and combining them with opium or other adjuvants, when an intermission becomes established.

*Pathological anatomy of autumnal fever.*—Upon this point Dr. Drake furnishes very little information, owing in part to the prejudices of the inhabitants of the valley against necroscopic examinations, and in part to a want of practice in making dissections in many places where the fever prevails. The *simple and inflammatory intermittents* seldom prove fatal; but when persons die from other diseases, who have previously suffered from the latter, it is very common to find traces of old inflammatory action on the surface of the *spleen*. From the accounts the author has been able to collect of the morbid anatomy of *malignant intermittents*, it results that in several cases very few lesions were found, the congestions which had existed having disappeared, and the patients dying from nervous depression. In the few cases in which the brain was examined, it was found congested, as were the lungs in other cases. The stomach, bowels, and liver, were in some cases congested, in others healthy, the spleen being the organ that was most often engorged. The traces of actual inflammation in any of the organs were few and uncertain. When *simple remittent* fever advances to the typhus state, the brain then always suffers from irritation, hyperæmia, or inflammation; and if the patients die, they do so in general from cerebritis, so that on examination, hyperæmia, fibrinous or serous exudations, and occasionally softening, are found. If death take place at an earlier stage of the fever, before its conversion into this typhus form, then the thoracic or abdominal viscera, instead of the brain, exhibit the effects of inflammatory action. In a paper communicated to the 'American Journal' in 1842, Dr. Stewardson states, that together with different lesions in different cases, in a large proportion he observed a peculiar change in the condition of the *liver*. The colour of this organ was that of bronze in some, bronze and olive in others, in all a mixture of gray and olive being the predominant colour, the natural reddish-brown of the organ having disappeared, or being only to be faintly traced. The change was uniform, or nearly so, throughout the whole extent and substance of the liver, which was also softer in texture than natural. Both this pathological appearance, and the others of the various organs mentioned by Dr. Drake, have been observed, however, in far too few cases to admit of any conclusions being drawn from them. This portion of his work, indeed, is meagre in the extreme, and calculated to give but a poor opinion of the scientific enthusiasm of the resident physicians of the valley.

*Consequences of autumnal fever.*—For determining whether the cause of autumnal fever may not likewise induce other morbid states of the economy, Dr. Drake does not believe that a sufficiency of facts exist; but that it will play an important part in the production of the peculiar national physiology which he believes will eventually characterise the inhabitants of the valley, he feels certain:

“Over most of the interior valleys a ruddy complexion is rare, and often replaced by a slight turbid hue or a tinge of sallowness. When standing before the medical classes of Lexington, Louisville, and Cincinnati, composed chiefly of young men between twenty and thirty years of age, I have seen very few with plump and rosy cheeks [we doubt whether many London lecturers even are often refreshed by such a sight]. In general the malar bones appear prominent, from defective cellular development of the cheeks. These deficiencies exist in various degrees, and are greatest among the people in what are called malarial districts. When we mingle with them, we see conclusive evidence that their physiology is not sound, although they may regard themselves in health. Those of the worst aspect have in general experienced one or more attacks of fever, which have left them infirm; but others have never suffered from that disease, and yet they are not vigorous either in appearance or reality. They who have constantly breathed the atmosphere of such localities, and have suffered attacks of fever while young, are often stunted in their growth, and never reach the port or portraiture of perfect manhood. But before we ascribe these effects to an empoisoned atmosphere only, we must recollect that heat and moisture generally prevail in such localities, and grant that an undefinable portion of the injury should be attributed to them.

“If we admit the reality of what has been set forth, and connect with it a periscope of the valley, but recently become the abode of civilized man—as yet, in its oldest settled portions, but in the transition state—many parts abounding in swamps, others intersected with alluvial streams, and almost everywhere overshadowed with forests, we may presume, that a national physiology, with its peculiar infirmities and predispositions, is, or must necessarily be, the consequence. This, if I mistake not, is actually the case at the present time, and constitutes a reason why bleeding and other active evacuations are not borne as well by those who live in low paludal districts, as by those who inhabit drier and higher localities. In the former, many diseases not inherently periodical display more or less of that type, evincing that the constitutions of the inhabitants have been acted upon by the cause of autumnal fever.” (p. 832.)

We are sorry to find that the new “national physiology,” of which Dr. Drake, in a passage formerly quoted, spoke in such glowing terms, is likely to be deteriorated by this depressing malarial element. Fortunately it need not be so permanently; since the vast flood of active settlers which is constantly rushing into the valley, will ere long work such changes in the physical conditions of the localities they may inhabit, that the source of disease, by no means peculiar in its operation and effects to America, will there, as it has elsewhere, when human ingenuity and labour have been brought to bear upon it, be at last restricted within very much narrower limits than at present.

Although the immediate mortality from yellow fever is much greater than that from autumnal fever, the diseased conditions to which this last may give rise, do much, by their number and importance, to reduce the difference:

“Yellow Fever, even when not skilfully treated, leaves but few vestiges behind. Death, or sound, even improved health, is the fate or fortune of the patient. It is far different with the subject of autumnal fever. When combated with skill in its



early stages, his recovery, it is true, may be perfect; and this, in mild remittents, may be the case if no medical aid be administered. But no fact is better established, than that many cases are followed by consequences from which patients slowly recover or finally die. Some of these lesions are found in the innervation, and manifest themselves as neuralgias in various parts of the body; others exist in the blood which remains impoverished; others in the exhalent or absorbent vessels, generating dropsies; others in the stomach and bowels, originating dyspepsia, diarrhoea, or constipation; others in the liver, which may remain torpid or inflamed, with attendant jaundice; lastly, others in the spleen, left inflamed, or enlarged and softened." (p. 834.)

Of the various organs here alluded to, the *spleen* is that which most commonly becomes diseased as a consequence of attacks of autumnal fever. It sometimes suffers also during the yellow and typhoid fevers, becoming enlarged and softened; but after these have been recovered from, there is no remaining enlargement of the viscus to be perceived. In simple intermittent it undergoes simple enlargement, which may disappear after the fever is cured; but in the malignant form of the fever the spleen is seriously affected, and continues enlarged long afterwards. The inflammatory form of intermittents induces actual splenitis. During the paroxysms of malignant remittent, it suffers very severely; for even although but two or three of these have occurred, enlargement is often left after recovery; while in those who die it is found swollen, greatly softened, so as to be almost diffuent, rarely exhibiting, however, in this form of the disease, other marks of inflammation. When *splenitis* occurs in the inflammatory intermittent, it may be either capsular or parenchymatous. The coagulable lymph thrown out in the former case may induce deformity or atrophy of the organ; while in the latter, induration from the effusion of lymph, or suppuration, may occur. Occasionally the case becomes complicated by the adhesion of the spleen to the diaphragm, and the propagation of the inflammation to the pleura and lungs. Very active antiphlogistic treatment is essential in the acute cases, but the paroxysmal character of the accompanying fever has often restrained the practitioner from putting it into force. Dr. Drake represents suppuration of the spleen as of frequent occurrence in this disease, more frequent than is suppuration of the liver; and of eleven cases of which he furnishes brief notes, in six the pus was discharged by the bowels, in three externally, in one by both modes, and in one was not discharged at all. In no one of them did the abscess make its way into the stomach, peritoneum, or lungs, and hectic fever did not accompany its discharge. All the cases occurred north of lat. 35°, most of them above lat. 38°, and the author never heard of a case south of the former parallel.

The most frequent result, however, is *enlargement* of the organ, which sometimes attains a very large size; yielding with difficulty, if at all, to treatment while the patient remains in the infected locality. When so enlarged, blows or external violence sometimes rupture it; and a case is given in which a spleen, containing about a gallon of blood, ruptured spontaneously. Sometimes patients perish from the profuse intestinal hæmorrhages which are common in this affection. In the more recent cases venesection may be required, and quinine is very useful, especially given during the decline of splenitis, with nitre. In the more chronic form, iodine, blistering, chalybeates, and good diet, are chiefly to be relied upon.

Dr. Drake terminates his interesting account of the autumnal fever, together with this first volume of his work, with the following remarks :

“The article on Autumnal Fever is now brought to a close. It has extended through many pages ; but a smaller number would not have sufficed to present even an outline of its etiological and therapeutical history, through so wide a geographical range as that of the southern half of our interior valley, in almost every part of which it is an annual endemio-epidemic. Of all our diseases it is the one which has the most intimate relations with soil and climate—that in which peculiarities, resulting from topographical and atmospheric influences, are most likely to appear. Hence it was chosen to stand next to the book of General Etiology, as illustrating, better than any other disease, the importance of the facts that make up that book. It is, moreover, the *great* cause of mortality and infirmity of constitution, especially in the southern portions of the valley, and therefore entitled to severe and patient attention. What I have collected and presented has required more labour than many of our brethren might suppose ; and yet perhaps they will not realise so fully as I do myself how much must be added—how many errors corrected—before the pages through which they have travelled can be entitled to universal acceptance. Meanwhile, if what has been written should stir up a single young physician to a more diligent observation of the fever, or save the life of one individual, who might otherwise have become its victim, my labour will not have been in vain.” (p. 866.)

The same modest appreciation of the results achieved by so many years of arduous investigation, pervade the work throughout ; no attempt being made to force conclusions from insufficient data, which are only offered provisionally, and as stepping-stones to a firmer landing-place. This, however, does not prevent the author from feeling assured that he has achieved a work of great utility, at least for the future, if not appreciated by the present time ; and in his preface he thus speaks of it with justifiable gratulation :

“If the work prove a failure, as it respects public favour, the author will not be without his reward, for he has found enjoyment in the labour of producing it ; and having confidence in its general accuracy, knows that it must stand as a great collection of facts, a picture of the etiological condition and the diseases of a newly settled country in the middle of the nineteenth century, with which future and more gifted medical historians will compare the causes, phenomena, and treatment of the maladies which may then prevail.”

We sincerely hope, however, that there is no danger of so useful a work not meeting with sufficient encouragement in America, to induce its veteran author to complete it without delay. We have had occasion to observe of late upon the dearth of medical works in that country, having pretensions to originality and research ; native talent seeming to be wholly expended in dishing up and garnishing the productions of European pens ; and it will indeed be melancholy to find an author, who has had the courage to produce a work of this magnitude, and one which would do honour to any country, left the sole consolation of the hope of a posthumous reputation.

The getting-up of the work, upon the whole, does credit to the Cincinnati press, and the maps which accompany it are well executed ; but we may suggest, for the future volumes, that the headings of the pages should indicate the divisions of the work, and that those who have the correction of the press should prevent a recurrence of innumerable orthographical errors, and the lavish and indiscriminate introduction of commas.

## ART. III.

1. *Compendium de Médecine Pratique, ou Exposé Analytique et Raisonné des travaux contenus dans les principaux Traités de Pathologie Interne.* Par M. LOUIS DE LA BERGE, Docteur en Médecine, Agrégé à la Faculté de Médecine de Paris, Chef de Clinique Médicale à la même Faculté; M. ED. MONNERET, Agrégé à la Faculté de Médecine de Paris, Médecin du Bureau Central des Hôpitaux; et M. LOUIS FLEURY, Agrégé à la Faculté de Médecine de Paris, Membre Correspondant de l'Académie Royale de Médecine de Belgique. Ouvrage Autorisé par le Conseil Royal de l'Instruction Publique et par le Conseil de Santé des Armées de Terre.—*Paris*, 1836-46. 8 vols. 8vo, pp. 698, 638, 642, 636, 639, 634, 615, 496.
2. *Guide du Médecin Praticien, ou Résumé Général de Pathologie Interne et de Therapeutique Appliquées.* Par F. L. I. VALLEIX, Médecin des Hôpitaux de Paris, Membre Titulaire de la Société Médicale d'Observation et de la Société Anatomique, Auteur de la 'Clinique des Maladies des Enfants Nouveau-nés,' du 'Traité des Névralgies,' &c.—*Paris*, 1842-47. 10 vols. 8vo, pp. 576, 600, 627, 559, 632, 608, 586, 599, 847, 1006.
3. *Handbuch der medicinischen Klinik.* Verfasst von Dr. CARL CANSTATT, königlich-bayerischem Gerichtsärzte und Mitgliede mehrerer gelehrten Gesellschaften. Zweite vermehrte Auflage.—*Erlangen*, 1843-47. 4 vols. 8vo, pp. 382, 1102, 919, 1109. Also published with the second title of *Die specielle Pathologie und Therapie vom klinischen Standpunkte aus bearbeitet von Dr. CARL CANSTATT, &c.*
4. *Handbuch der Pathologie und Therapie.* Von Dr. C. A. WUNDERLICH, Professor der Medicin, Vorstand der medicinischen Klinik zu Tübingen. Dritter Band.—*Stuttgart*. 8vo, pp. 1238.

TAKING up the subject of the Diseases of the Intestinal Canal at the point at which we broke off in the October Number of this Journal, we proceed to the consideration of Wunderlich's fourth division of the disorders accompanied by anatomical changes. This division includes the different varieties of Hyperæmia and Hæmorrhage, quantitative disturbances of Nutrition and Secretion, Catarrhs, and Inflammatory Affections of the Intestinal Canal.

Commencing with affections which are limited to a particular part of the intestinal tract, we shall afterwards notice those which are simultaneously developed in several portions.

*Inflammation of the œsophagus* is treated more fully by our French than by our German authors. It may be divided into—

I. Acute œsophagitis; under which title we include—(a) Follicular œsophagitis; and (b) Pseudomembranous œsophagitis: and—

II. Chronic œsophagitis.

Simple acute œsophagitis is allowed by all writers to be a rare disease, which is probably owing to the deep-seated position of the œsophagus, and to the circumstance, that it is only occasionally and for short periods exposed to the action of external agents. It is confined to no special

period of life ; and Billard even records a case in which it seemed to occur during intra-uterine existence. Amongst the occasional causes may be placed the abuse of certain medicines, especially mercury, iodine, and tartar emetic, the contact of acrid and corrosive substances, the temporary impaction of a hard voluminous body, and possibly the efforts made to accomplish its removal. Cases are recorded in which it has arisen from the retrocession of exanthematous affections, and even from the ingestion of very cold water, when the body was in a state of perspiration. The quasi-inflammatory affection of the œsophagus occurring in hydrophobia, is simply a complication of that affection ; and the same remark applies to the state of this organ in variola, yellow fever, &c.

Pain, gradually increasing in intensity, is the first and the most constant of the symptoms of œsophagitis. At first it is often only perceptible as the alimentary bolus passes the inflamed spot ; it is then, however, so violent, that in some cases patients have refused even to swallow liquids. The exact seat of the pain is a point of great diagnostic importance. In the cases observed and collected by M. Mondiere, to whom we are indebted for the best Monograph which we possess on this subject, and who has himself suffered from the disease, the pain was principally referred to the inferior portion of the pharynx, especially at the commencement of the disease ; occasionally the patient refers to the region of the larynx, to the epigastrium, or to the interscapular space, as the seat of pain. In all M. Mondiere's cases there was a more or less difficult and abundant expectoration of glairy matters, and a sensation of heat along the course of the œsophagus. Hiccough is usually mentioned as a symptom, but it certainly is not an essential one. The pain caused by the deglutition of even the mildest substances, as, for instance, the saliva, emollient drinks, &c., is often so severe as to excite true spasm of the œsophagus, and to give rise to vomiting. Of the general symptoms it is unnecessary to speak in detail ; the most striking are the frequency and hardness of the pulse, the heat of the surface, and the general agitation, which is much increased by the thirst and by the apprehensions of the patient.

The follicular œsophagitis described by Mondiere and succeeding writers is seated in the mucous follicles. This is very rarely an idiopathic affection ; it is most commonly observed in the later stages of typhoid and other severe fevers.

Pseudomembranous or diphtheritic œsophagitis is a very rare affection ; it most commonly occurs as an extension of buccal and pharyngeal diphtherite, and presents itself under pathological conditions similar to those under which croup is developed. In diagnosing this disease, the interior of the mouth must be carefully examined ; difficult deglutition, regurgitation of the milk and of drinks through the nose, the refusal to take the breast, &c., being the signs which, in the infant, lead to the suspicion of diphtheritic œsophagitis.

According to M. Mondiere, abscess is not a very rare termination of inflammation of the œsophagus. He believes that it appears more rare than it really is, because the abscess may often open and its contents be evacuated without the knowledge of the patient. The following case, abridged from one of his memoirs, seems to support this view.

A man, aged 40 years, travelling during a hot summer, and overcome with thirst, drank freely of very cold spring-water. In the evening there

was difficulty in swallowing, accompanied with pain. His food was arrested at the level of the second portion of the sternum, and was sometimes rejected. For six days he sought no medical aid; but at length, being continuously tormented by dysphagia, he applied to M. Bourguet, who could detect no swelling in the throat, but observed that the food was always rejected a short time after it had been taken. Finding that no success attended the employment of venesection, leeches, emollients, &c., he explored the œsophagus with a wax bougie. At first the instrument would not pass beyond the spot where the pain was experienced and the food stopped, but by gentle force the difficulty was overcome, and the instrument passed into the stomach. On withdrawing the bougie, it was found to be covered with pus. From this time the patient swallowed easily, and was soon perfectly well.

Acute œsophagitis is a disease which seldom lasts longer than a week, except in those cases in which it terminates by suppuration. The ordinary termination is by resolution; indeed, the only dangerous cases are those in which suppuration or gangrene supervenes. An abscess, seated in the upper part of the œsophagus, may compress the upper portion of the larynx, and give rise to symptoms of œdema of the glottis.

As there is often considerable difficulty in establishing a correct diagnosis, we shall enter somewhat fully into this subject.

Acute œsophagitis may be confounded with diseases not having their seat in the œsophagus. It may happen, that in an affection of the trachea, as, for instance, when there is an ulcer on its posterior surface, the deglutition may be difficult; but the cough, and the pain induced on taking a full inspiration, sufficiently indicate the tracheal nature of the case. In œsophagitis, the cough, if it be present, is merely guttural; and further, the contrast is sufficiently striking between the intense pain felt in œsophagitis during the act of swallowing, and the trifling discomfort that is sometimes perceived in tracheal affections during deglutition. Finally, the spasms and vomiting which we have described as frequently occurring, are not present in diseases of the trachea.

When there are ulcerations on the upper part of the larynx, and especially when they occur on the epiglottis, there is pain and difficult deglutition, and fluids are rejected by the nose; but, at the same time, the respiration is difficult, and cough is induced by a strong inspiration: and further, although deglutition is impeded, there is not that intense pain which, in œsophagitis, is always observed when the food comes in contact with the inflamed spot.

Acute œsophagitis may also be confounded with other diseases seated in the œsophagus. It is by no means easy to diagnose between this disease and rheumatism of the œsophagus. If, however, pain in the œsophagus supervene, in the absence of all febrile disturbance, when all the functions are properly discharged, and especially after the disappearance or during the course of muscular rheumatism, and this pain be acute during deglutition, but absolutely disappears during the intervals, and diminishes rather than augments after the ingestion of the first few mouthfuls of food, we may regard the affection as rheumatic rather than inflammatory.

The true nervous spasm of the œsophagus may be usually distinguished from acute œsophagitis by the following circumstances. It almost always occurs in the course of certain neuroses, and especially in hysteria; it is



characterised by irregular movements of the œsophagus, which give rise to vomiting, but it does not usually present that intense pain on the passage of the food to which we have so frequently alluded; neither is it accompanied with any febrile disturbance. As cancer, stricture, and dilatation of the œsophagus are more liable to be confounded with chronic than with acute œsophagitis, it is unnecessary to do more than mention them. Abscess of the œsophagus may possibly be distinguished from simple inflammation by its narrow limits, the fixed position and persistence of the pain, the protracted duration of the disease, the sudden stoppage of the food at exactly the same spot, and the immediate vomiting; we may thus be led to suspect, if not to diagnose with certainty, the existence of an abscess.

The recorded cases of acute œsophagitis are not sufficient to enable us to draw any certain conclusions regarding the efficacy of different modes of treatment, from the results of experience. In persons of a robust and vigorous habit of body, venesection may be prescribed to allay the general excitement of the system. Leeches and cataplasms to the neck, and the inunction of mercurial ointment, constitute the routine external treatment, while mild, viscid, or oily medicines, when they can be swallowed, and calomel mixed with butter and placed on the back of the tongue, form the ordinary internal treatment. Small pieces of ice or a little lemon-juice may be given to allay the thirst; and perfect rest and silence must be enforced. In œsophagitis arising from the action of caustic acids, we must, in the first place and immediately, use antidotal remedies, as magnesia, carbonate of lime, &c., and afterwards proceed as in ordinary cases.

There is nothing calling for special remark in the other forms of œsophagitis. We pass on, therefore, to Affections of the Stomach.

The first disease we shall notice is *erythematous gastritis*,—intense hyperæmia of the stomach, with severe and acute pain. Medical nomenclature is in so vague and unsatisfactory a state, that it often becomes difficult to recognise the same morbid condition under its various names. We believe, however, that the chapter or section in Wunderlich, on “Intense Hyperæmia of the Stomach, or Erythematous Gastritis;” in Canstatt, on “Gastritis from Noxious Ingesta;” in Valleix, on “Simple Acute Gastritis;” and in the ‘Compendium,’ on “Slight Subacute Gastritis,” all refer more or less to the same affection.

Erythematous gastritis may arise from various causes, of which the most common are hot or very cold drink or food, irritant matters, especially to such as are unaccustomed to their employment, many special kinds of food (fat, crabs, shell-fish), sometimes mere overloading of the stomach, external wounds or blows, corrosive substances, poisons, chills, suppressed menstruation, &c.

Such, according to Wunderlich, are the causes of erythematous gastritis. We shall extract the whole of his remarks on this disease, premising, that we think he would have done better to have made it merely a mild variety of acute gastritis.

“The symptoms ordinarily commence soon after the exciting causes have begun to act, and gradually increase. At first there is merely a sensation of nausea, which is not very distinct, and which the patient often connects with that of hunger. There is sometimes dryness of the mouth, headache, and a feeling of general soreness, with or without chills and flying heats. On a sudden the patient complains

of severe dragging pains in the region of the stomach, which is extremely tender to the touch; painful vomiting then ensues, the rejected matters being at first acid and watery, and often mixed with a little blood, and afterwards bilious; the concluding efforts of vomiting are accompanied with much straining and a sensation of suffocation. Any food—however small the quantity or mild the nature of it—induces a recurrence of the vomiting. The patient then complains of an unquenchable thirst, which increases till it assumes the character of burning and dragging pains in the neck. The face, which at first was red, now becomes pale, collapsed, and expressive of the most severe illness. The pulse is very frequent and small, and the skin is sometimes cold. The patient has a feeling of utter prostration, and his voice is entirely gone; there is a dark mist before his eyes, and he often falls into a state of sopor or of quiet muttering delirium. The respiration is sometimes much affected. These symptoms usually moderate in a short time (a couple of days), and often do not attain this degree of severity. The pains diminish and the vomiting is suspended; there being only an occasional tendency to its recurrence. The tongue begins to become coated; but it is only after a considerable period that the extraordinary sensibility of the epigastric region subsides.

“*Treatment.* Abstraction of blood, local rather than general, pounded ice to the region of the stomach, emollient or derivative clysters, and tepid baths (especially in the later stage). The internal treatment is of secondary importance, and must be confined to the administration of the mildest remedies in the smallest doses,—mucilaginous medicines, aqua laurocerasi, fragments of ice. The administration of calomel in not too small doses (gr. v—x), if the irritability of the stomach is not too great, seems an appropriate and safe means of moderating or suppressing the local hyperæmia.” (Wunderlich, vol. iii, pp. 883-4.)

In the rare cases which terminate fatally, the mucous membrane is described by our author as presenting patches of red of various shades and forms, thickening, ecchymoses of blood at various points, and incipient softening,—in short, the leading anatomical characters of intense hyperæmia.

Intervening between the history of this disease and that of acute gastritis, from which we have already expressed our opinion that it ought not to have been separated, we have two sections devoted to gastric hæmorrhage and acute gastric catarrh.

There are few diseases in which it is more difficult than in *hæmatemesis*, to associate and duly connect the symptoms and the changes of structure; for, while it is a symptom based on various diseased conditions of the stomach and the general system, it also may arise from the accidental entrance of blood into the stomach from the œsophagus, mouth, nose, or lungs. Gastric hæmorrhage, in its limited sense, is a form of stomach-disease, which may occur either as a consequence of pre-existing organic changes (in which case it is merely a single and isolated symptom), as in gastric ulcer, cancer of the stomach, &c., or it may occur alone, as the most essential symptom.

We shall briefly review and analyse the various causes of hæmatemesis, as assigned by our different authors. Wunderlich observes, that it is by no means uncommon during the first fortnight of infant life, but that in later childhood (up to the fourteenth year) it is very rare. In this opinion he is not singular, as Jos. Frank,\* and other eminent authorities, hold a similar view. Valleix, however, states, that during a space of three

\* Frank (*Præc. Med.*, vol. i, p. 537) observes, that young children and infants are predisposed to hæmatemesis. It is true that he has collected a considerable number of cases from the writings of Storck, Vogel, Schmitt, &c.; but these are usually given by their respective authors simply on account of their rarity.

years, in which he paid special attention to the diseases of infancy, he did not meet with a single case. At and soon after the fourteenth year, it becomes commoner in the female sex from its connection with menstrual disturbance—a point to which we shall presently recur. It is in adult life, and chiefly from the fortieth to the fiftieth year, that this affection is most frequent. In women it is not uncommon during pregnancy and soon after the cessation of the menses.

Valleix has collected and analysed twenty-seven cases of hæmatemesis, and in only seventeen could he ascertain the exciting cause; in nearly all these cases it depended on some alteration in the stomach giving rise to the rupture of a vessel, or on suppression of the menses.

Blows or contusions on the epigastric region often give rise to the vomiting of blood. Emetics have been asserted by Frank and others to produce the same effect; Valleix, however, is of opinion,—and we quite agree with him,—that in these cases there must have been pre-existing disease, and that the hæmatemesis has only been *hastened* and not *caused* by the vomiting. Several cases are on record in which foreign bodies, as fragments of bone, pieces of money, &c., in the stomach have caused ulceration of that organ, and thus given rise to fatal hæmorrhage. Latour, and we believe some other writers, have recorded cases in which intestinal worms in the stomach, and leeches accidentally swallowed, have given rise to this symptom. A case is recorded by Defermon\* in which hæmatemesis was caused by the inspiration of carburetted hydrogen.

The suppression of hæmorrhages, and especially of the menstrual discharge, is a frequent cause of hæmatemesis. In five out of the twenty-seven cases collected by Valleix, this symptom was connected with suppressed menstruation.

There are some blood-diseases in which hæmatemesis is of frequent occurrence, as, for instance, scarlatina, scurvy, typhous, severe intermittent and yellow fevers, and plague; but these forms of hæmorrhage merely require mention here. Hæmatemesis is frequently dependent on diseases of the liver and spleen; for the hepatic and splenic arteries being obstructed, an excess of blood is directed to the arteries of the stomach. In the same way when there is derangement of the portal circulation, there is always a tendency to hæmatemesis. Finally, there is hæmatemesis from organic lesions, of which the most frequent is erosion of the stomach (especially cancerous erosion) implicating an artery, (that artery being almost always the coronary artery.) Nine of the twenty-seven cases collected by Valleix were of this nature.

Various attempts have been made by Martin Solon (*Dict. de Chir. et de Méd. Prat.*, article *Hæmatémèse*), Andral (*Anat. Path.*), and others, to establish divisions of hæmatemesis. These are pretty freely criticised by Valleix, whose divisions of them with *active* and *passive* is, we conceive, of little practical value.

We shall now briefly glance at some of the most important symptoms. Before the hæmatemesis occurs, there are usually feelings of *malaise*, headache, disinclination to move, heaviness, and of pressure and pain in the hypochondriac region; the patient also frequently complains of nausea, loss of appetite, and constipation; and abdominal pulsation, according to Wunderlich, may be sometimes perceived.

\* Bulletin des Sciences Médicales, 1827.

After these symptoms have lasted for some time, there is a sudden vomiting of black—as it were carbonized and partially coagulated—blood; this may continue by fits and starts for some hours, and a large quantity of blood be thus ejected. If, however, the blood proceed from the erosion of an artery, it is red and florid. At the same time there is acute pain in the region of the stomach, often great thirst, frequent tremors of the limbs, a rigor, and even a convulsive attack.

When a certain quantity of blood has been ejected, the patient feels relieved, although extremely weak; there is, however, great tenderness in the epigastric region, abdominal pulsation, intense thirst, and a frequent pulse. When the quantity lost is very great, or the patient was previously debilitated, faintness occurs, the extremities become cold, the countenance deadly pale, and the lips cold and blanched.

There is often a second attack after four or five days; the patient during the interval being feverish, thirsty, and sleepless, and having a very white tongue. The blood that is now brought up is less abundant, and is redder and purer than on the former occasion. The bowels are usually costive; if they at length act spontaneously, the discharge is accompanied by colic pains, but whether they act spontaneously or with the aid of medicine, the stools are scanty and have a black appearance as if they contained charred blood. After this action of the bowels, the symptoms gradually abate, the thirst becomes less intense, and the appetite gradually returns.

It is seldom that the first attack is fatal; the patient usually rallies from it more or less perfectly, but before he has acquired his former strength, a second attack usually supervenes, from which he rallies less completely; and thus he gets gradually weaker, till the general characters of anæmia, with murmurs of the cervical vessels and palpitation, exhibit themselves. While these repeated attacks generally prove fatal in a few weeks or months to weak and debilitated persons, strong persons, especially those in whom the hæmatemesis is connected with disturbance of the menstrual function, may resist the disease for years; — “often,” says Wunderlich, “for twenty years or more, even when there are four or five attacks annually.” In weak persons it soon gives rise to dropsy.

When the hæmatemesis depends on gastric ulcer or cancer, its course is sometimes very chronic. For months, or even for a year or more, a small quantity, in the form of coffee-grounds deposit, may be ejected every two or three days, or every week.

We extract from Valleix the following diagnostic table:

*I. Distinctive signs of hæmatemesis and of epistaxis, when the blood is ejected by the mouth.*

HÆMATEMESIS.	EPISTAXIS.
There are frequently anterior gastric symptoms, pain, disturbed digestion, &c.	No symptoms referable to the digestive organs.
The vomiting of blood not preceded by nasal hæmorrhage.	Nasal hæmorrhage preceding the vomiting of blood.
No traces of blood in the nasal fossæ.	Traces of blood, either soft or dried clots, in the nasal fossæ.

With regard to the last point of difference, it is possible that when the hæmatemesis is very abundant, some blood may escape by the nasal fossæ.

## II. *Distinctive signs of hæmatemesis and hæmoptysis.*

HÆMATEMESIS.	HÆMOPTYSIS.
The blood is black, in soft or broken clots.	The blood is most commonly red and frothy.
The blood is rejected by vomiting after nausea, weight in the epigastric region, a sensation of fluctuation, pains in the stomach, &c.	The blood is rejected by the mere efforts of coughing.
Dullness of the epigastric region.	No dullness in the epigastric region.
Alimentary matters mixed with the blood.	The blood pure or mixed with expectoration.
After the hæmorrhage, black fetid stools containing blood.	After the hæmorrhage, natural stools.
No sub-crepitant râle in the chest.	Sub-crepitant râle over a greater or less extent of the chest.

## III. *Distinctive signs of idiopathic and symptomatic hæmatemesis.*

IDIOPATHIC HÆMATEMESIS.	SYMPTOMATIC HÆMATEMESIS.
Digestion not disturbed, or only for a very recent period.	Prolonged disturbance of digestion.
No obvious emaciation.	Extreme emaciation.
No signs of a cancerous tumour in the stomach.	Pain and a tumour in the epigastric region.

The diagnosis between idiopathic and symptomatic hæmatemesis is, however, often far from certain.

## IV. *Signs indicating that the hæmorrhage is continuing, after the vomiting of blood has ceased.*

- A feeling of general distress and discomfort, with weight, fluctuation, and pain in the epigastric region.
- Coldness of the extremities, shiverings, cold and clammy sweats.
- Extreme sinking and faintness.
- The pulse small, miserable, and thready.

Our space will not allow of our attempting to follow Valleix through the sixteen pages which he has devoted to the treatment of hæmatemesis. We shall, however, endeavour to lay before our readers a very brief sketch of the treatment to be pursued.

The premonitory symptoms will often warn a patient who has had previous attacks, that the recurrence of hæmatemesis may shortly be expected, especially when a natural discharge (as the menstrual or a hæmorrhoidal discharge) has been suppressed. The apprehended attack may often be kept off by the enforcement of perfect rest, the suspension of all food, the abstraction of blood from the epigastric region, or in plethoric subjects from the arm, and derivation towards the seat of the previous discharge, which may be accomplished by stimulating clysters, &c.

During the attack, and immediately after it, we must prescribe ice, both internally in rounded fragments, and externally; leeches if the hæmorrhage has not already been very copious; the horizontal position; and ligatures or the exhausting apparatus (Junod's) to the extremities.

After the hæmorrhage has been arrested, perfect rest and quiet must be maintained for some time; and when there seems to be no threatening of its return, the bowels may be moved by mild laxatives and clysters, and thus freed of the blood which they contain. The food must be of the



mildest description and given in extremely small quantity, and all food and drink must be taken cold.

If there is great sensibility of the stomach, or much general excitement, we must have recourse to narcotics; it is, however, not advisable to give them in such doses as to produce a decided masking of all the painful sensations.

When the hæmorrhage continues obstinately to recur, we must carefully exclude everything from the stomach, except the necessary medicines, and even these must be given in the smallest possible form. Wunderlich insists so strongly on this point, that he observes that the ice placed in the mouth must not be swallowed. The most suitable medicines are dilute sulphuric acid, acetate of lead, and the vegetable astringents (as, for instance, rhatany); and a little opium may often be advantageously combined with them. A bladder containing pounded ice must be laid on the epigastric region, and derivatives, as, for instance, sinapisms, or large cupping-glasses, must be applied to the extremities. Wunderlich also recommends that the lower bowel should be stimulated with clysters containing vinegar, croton oil, and aloes, and that, if the patient be of robust constitution, leeches should be applied to the anus.

As soon as we have reason to believe that the hæmorrhage has ceased, we must omit all internal remedies, and persist merely in external applications.

There are a few special symptoms to which, in reference to treatment, we would direct the attention of our readers.

However distressing the thirst of the patient may be, we must never yield to his desire for drink. We must endeavour to relieve the urgency of the symptom, by allowing him small pieces of ice, or permitting him to suck slices of lemon.

When there is severe pain in the gastric region, we must have recourse to opium, taking care, as we have already observed, not to give it in stupifying doses.

The abdominal pulsation is often extremely troublesome; this symptom, according to Wunderlich, is best treated by leeches to the anus, or stimulating clysters.

Fainting is a frequent and very serious symptom; every effort must be made to restore the patient as speedily as possible, by volatile irritants, &c.

When there is extreme anxiety, and much general excitement, opium is the principal remedy. Sometimes dyspnœa or even suffocation may arise from blood finding its way into the trachea, or closing the opening to the air passages. A case is recorded by P. Frank, in which the patient was nearly suffocated by the accumulation of large clots of blood in the isthmus of the fauces. He was alone when he was seized with the hæmatemesis, and in consequence of the above accident, fell to the ground in a state of unconsciousness. On the removal of these clots from the mouth and throat, he gradually recovered.

When very obvious symptoms of anæmia ensue with great rapidity, we are sometimes compelled to give ether, wine, or musk, sooner than would otherwise be judicious. By administering these remedies we may excite fresh hæmorrhage; by withholding them we may see our patient expire before our eyes. It is obviously our duty to choose the lesser evil. Most commonly, however, the anæmia is a secondary condition, arising after all

the most pressing dangers have disappeared. Nourishing food, wine, and even iron may then be safely given.

During the period of convalescence, all disturbing causes, both mental and corporeal, in any way likely to induce a return of the disease, must be most guarded against. For a long time after the attack, the patient has often a loaded tongue, and experiences anorexia and a great loss of digestive power. In these cases the appetite must not be excited by highly-seasoned food and condiments; we must rather have recourse to passive exercise, change of air, and tepid baths; moreover, the bowels must be daily moved by injections.

Wunderlich very truly remarks that there are cases of hæmatemesis in which such energetic means as those we have described are unnecessary. Thus, when the hæmorrhage is connected with disturbed menstruation, rest in the horizontal position, and a proper regulation of the diet, are often all that is necessary. And in those cases where it depends on cancerous destruction, palliatives, rest, and a mild diet, usually afford all the relief that can be rationally expected.

The next disease to which we shall advert is *chronic gastritis*, one of the most frequent of the disorders of the digestive system.

It most commonly arises from acute catarrhs and inflammations which have not been perfectly cured, repeated irritation from improper food or drink, especially from the abuse of alcoholic drinks, undue exercise while the process of digestion is going on, and not unfrequently the prolonged sojourn (sometimes even for months) of indigestible substances in the stomach. Moreover, we sometimes find it associated with various forms of chronic diseases,—with morbid conditions of the blood, with diseases of the brain, heart, and liver, with affections of the intestines, of the generative organs, of the joints, and of the skin. Further, when the functions of an organ are performed in an extraordinary manner, the organ itself not being diseased, we not unfrequently meet with this disorder; as, for instance, after great mental labour, vivid emotions, deep grief, prolonged vigils, excessive muscular activity or perfect inactivity, abuse of the generative powers, pregnancy, too protracted suckling, repeated suppression of the cutaneous transpiration, &c.

We shall not enter into the subject of the morbid anatomy of this affection, further than to remark, that in many cases the mucous membrane is found to be in a perfectly healthy condition, while the subjacent membranes afford signs of diseased action. Wunderlich accounts for this apparent singularity, by assuming that the diseased condition extends from the mucous membrane by contiguity of tissue to the deeper seated textures, and that as the catarrh moderates, the mucous membrane returns to its normal condition, while the sub-mucous tissues remain in an infiltrated condition, or undergo further morbid changes.

One of the most common symptoms of this disease is the pain, which sometimes occurs spontaneously, is often very severe, and is described by different patients as heavy, burning, lancinating, &c.; and in other cases only occurs when pressure is exerted on the epigastric region, or after the ingestion of food. The pains which occur spontaneously are almost always aggravated by pressure. Great differences are observed in reference to the food which is most easily digested; some patients can only take

the mildest articles of food, others can only bear highly seasoned dishes ; some must take everything warm, others everything cold ; some cannot bear salted meats, others strongly desire them ; generally speaking, however, acid and fatty kinds of food are found to excite pain and disturbance in every form of gastritis. Cases occasionally occur, in which, although there is undoubted evidence of the presence of chronic gastritis, there is no indication of pain, or even of increased sensibility, in the region of the stomach. Vomiting is not so frequent a symptom as some writers have maintained ; it seems to be associated with exacerbations of the disease. The nature of the vomited matter is by no means uniform ; while in some cases it consists, in a great measure, of undigested food, in others it is almost entirely composed of a thin acid fluid. Nausea is a much more frequent symptom than actual vomiting, being invariably present to a certain extent. On examining the epigastric region, distension is sometimes, but by no means generally, observed. No important inferences can be drawn from the loss of appetite and the thirst, which are usually more or less present. The tongue is very seldom in the normal condition ; it may present almost every variety of coating in this disease ; sometimes it is clean, but speckled with numerous red swollen papillæ, especially towards the apex ; these red papillæ may sometimes be seen through a white coating. As a general rule the tongue is always moist, unless when the gastritis supervenes on some febrile disorder. The saliva is frequently increased in quantity, and often has an acid reaction. Constipation is far more common either than diarrhoea or a natural state of the bowels.

Although the symptoms are generally sufficiently clear to leave no reasonable doubt regarding the nature of the disease, cases sometimes occur, in which it is impossible to say whether the patient is suffering from chronic gastritis, gastralgia, or cancer, or ulcer of the stomach. Some remarks on the distinctive diagnosis of these diseases will be found in our last article.

In reference to the treatment of chronic gastritis, we may observe that our German writers lay proper stress on the extreme importance of due attention to dietetic and hygienic measures, while our French authors seem to overlook this point altogether. Wunderlich commences his remarks on the treatment of this disease, with the observation, that a proper regulation of the diet is essential to the successful cure of chronic gastritis ; we should prescribe a moderate quantity of mild food, such as can be taken by the patient without producing feelings of discomfort, rest while the process of digestion is going on, daily but not too fatiguing exercise, proper attention to the functions of the skin, especially by frequent baths, warm clothing, particularly over the epigastric region and the feet, and pure air. By attention to these rules, moderate cases of chronic gastritis, especially when not of very long standing, are soon relieved. When the urgent symptoms have disappeared, the constitution may be fortified by a course of acid, or acid-chalybeate mineral waters. In some cases of chronic gastritis, a milk diet is followed by the best results, while in other cases it seems only to aggravate the symptoms. We have met with patients in whom a very low rigid diet produced an aggravation of the symptoms, and who, under a more abundant and nutritious food, soon exhibited signs of improvement. Amongst the more active means employed in these cases, we must mention the application of leeches to the epigastric region ; the bowels must be

kept open by the mildest aperients or by clysters. Wunderlich recommends that at first the only medicine should be gum-water, to which a little carbonic acid, and, if the stomach be very sensitive, a narcotic may be added. If these mild means fail, recourse may be had to small doses of nitrate of silver, or of the mineral acids, to tincture of iron, and to chalybeate waters ; in very obstinate cases, we must try astringents,—lead, bismuth, iron, rhatany, and kino; and good wine mixed with a little water.

The next disease at which we shall glance is *inflammation of the duodenum*. It is an affection that does not present itself with sufficient frequency to enable us to ascertain, with accuracy, the conditions under which it is developed. We know, however, that it occurs both in the acute and chronic forms, either independently or accompanying similar diseases of the stomach, and that it may terminate fatally. It has been observed to occur after great excesses in diet, and in persons addicted to habits of intemperance, after the abuse of purgatives, after and during certain forms of mental derangement, after extensive burns, in diseases of the liver and in the case of impacted gall-stones, and, probably, as Dr. Hodgkin has suggested, after severe and continuous vomiting.

The following are regarded by Canstatt (vol. iv, p. 508) as the characteristic symptoms of this disease :

1. Pain, feelings of uneasiness, of tension, &c., deep in the epigastric region, and extending from thence towards the right hypochondrium, and towards the spine and the right scapula, increased on pressure at these parts.

2. In chronic cases these disagreeable sensations occur from three to six hours after a meal, increasing in severity, and lasting for an hour or more ; they then seem to move towards the region of the right kidney, and either gradually disappear or terminate with a fit of vomiting.

3. Nausea and vomiting are described as ordinary symptoms of inflammation of this portion of the intestine ; in chronic cases frequent vomiting, at an interval of five or six hours after a meal, is regarded by some writers as characteristic of this disease.

4. A jaundiced state of the skin has been regarded as a pathognomonic symptom of this disease. As, however, jaundice very often occurs without a trace of duodenitis, it can only be regarded as a valuable diagnostic symptom in reference to this condition, when there is no swelling, pain, or apparent disease in the hepatic region. (According to Stokes, inflammation of the duodenum is the cause of the icteric tint in yellow-fever.)

5. On exercising pressure on the region of the duodenum, we may sometimes detect a circumscribed hardness and fulness. In chronic cases the tumour has a doughy consistence.

6. The discharge of fatty matters from the bowels has been regarded as diagnostic of this disease. This is doubtless owing to the inflammation closing up the orifice of the pancreatic duct.

7. In ulceration of the duodenum, the emaciation is (according to Albers) very great ; indeed, much greater than in disease of the stomach.

8. Piorry states that to determine whether there be inflammation of the duodenum, we must first determine, by percussion, the limits of the liver, the stomach, and the transverse arch of the colon. The two latter must be examined both when empty and when full. (The stomach may be partially filled by the drinking of a glass or two of water, and the colon by a copious

enema.) If in the space between these boundary lines we find pain and swelling, we may conclude that duodenitis is present.

We shall not enter into the consideration of the general symptoms, because they do not differ, in any marked degree, from those of inflammation of the adjacent portions of the intestinal tube.

In regard to the termination of inflammation of the duodenum, there is no doubt that it often ends favorably. Wunderlich observes that he has seen a good many cases of acute icterus, in which, from the sensibility in the duodenal region and the general symptoms, there could be no doubt regarding the nature of the disease, and where the disease terminated favorably after a period varying from a fortnight to six weeks.

The principal dangers of this disease are dependent, partly on a rapid augmentation of the general symptoms, and the occurrence of prostration and adynamic fever; partly on the formation of perforating ulcers; and partly, on the extension of the inflammation to other parts of the intestinal canal, or to the peritoneum or liver, which latter organ may either become acutely affected by the topical extension of the morbid process along the biliary ducts or the blood-vessels (as Ribes maintains), or chronic inflammation may be set up.

The treatment to be adopted in the acute form of the disease, is much the same as that for inflammation of the stomach or other parts of the intestinal canal.

Copious and repeated local bloodletting is of the greatest importance; perfect quiet must be maintained, a low diet must be prescribed, warm cataplasms must be applied over the painful region, and the bowels must be acted on by injections. Our medicines must be confined to the class of mild demulcents; Wunderlich, however, is of opinion that large doses of calomel may exert a salutary local action on the inflamed surface. If severe vomiting be present, it must be controlled by opium; if there be much cerebral excitement, cold must be applied or blood extracted locally, or even a blister applied to the scalp or back of the neck; and if thirst be a very annoying symptom, it may be relieved by the frequent use of small pieces of ice.

In chronic duodenitis, much attention must be paid to the food and to the due regulation of the bowels. A few leeches may be occasionally applied, and warm baths and stimulating pediluvia will be found useful.

Passing over the subject of enteritis, we shall notice at some length the *inflammatory affections of the cæcum and its appendix* (typhlitis).

The first and most distinguishing point in which inflammation of the cæcum differs from inflammation of other parts of the intestinal canal, is *the position and the nature of the pain*. The pain is seated in the region of the cæcum, and the right iliac region is very tender on pressure. It does not change its position, or extend in different directions, as in other kinds of enteritis; although sometimes colic-like pains extend to the ascending and transverse colon, down into the pelvis and along the right thigh. In these cases there is numbness of this limb, and spasmodic retraction of the right testicle. The pain usually increases when the patient assumes the erect position or lies on his left side, the most easy posture being on the right side, with the thighs bent forward so as to relax the abdominal walls; it is also augmented to a great degree whenever the bowels are



moved, although in this case it soon abates. The intensity of the pain varies (as in other forms of intestinal inflammation) according as the mucous membrane or the outer coats of the cæcum, or the surrounding areolar tissue, be involved. It is in the last-named case, that the pain in the thigh is usually most acutely felt. In inflammation of the appendix the pain is usually very severe.

A second characteristic symptom of typhlitis is a deep-seated feeling of hardness and swelling in, and not extending beyond, the cæcal region. Hard excrements retained in the cæcum frequently give rise to this swelling, which, on percussion, yields a dull sound, strongly contrasting with the tympanitic sound yielded by the upper intestines, which are in this case distended with gas; but after the discharge of these hard fæces, a certain amount of induration and swelling still remains, especially in perityphlitis and in chronic inflammation. If, as is often the case, perityphlitis proceeds to suppuration and the formation of abscesses, in the position of the previous hardness and swelling we perceive a sensation of fluctuation.

No general rule can be laid down in reference to the condition of the bowels; sometimes there is constipation, sometimes diarrhoea, sometimes the two states alternate. In stercoral typhlitis, or that form in which the cæcum is loaded with hardened fæces, constipation generally prevails. If the mucous membrane of the cæcum be the tissue principally affected, we often have fluid, mucous, or muco-sanguineous, fetid evacuations, accompanied with colic pains. In a more advanced stage of this affection of the mucous membrane, the stools are frequently white and of a thick mucous consistence. There is often tenesmus, although not so often as in dysentery. When all the coats are affected, constipation is almost always present.

The other symptoms vary according to the seat, the intensity, and the course of the inflammation. There may be severe fever, slight fever, or even no febrile symptoms. No special indications are presented by the tongue. In inflammation of the appendix, the symptoms are often so severe, as to excite the suspicion that invagination or ileus is present.

With regard to the further progress and final results of typhlitis in its different forms, it may be observed that typhlitis stercoralis generally terminates favorably as soon as the impediment presented by the impacted fæces is removed. The inflammation may, however, extend from the cæcum to the adjacent areolar tissue, giving rise to increased swelling, hardness, and tension—in a word to perityphlitis,—and may terminate finally in perforation of the intestine. In scrofulous persons, perityphlitis runs a very chronic course.

We extract the following remarks on the treatment of perityphlitis from Wunderlich:

“Treatment is most effective when the inflammation is neither metastatic nor has been extremely rapid in its course, and when the best time for the application of remedies has not been allowed to pass. It consists especially in the repeated local abstraction of blood, in the application of cataplasms to the painful part of the abdomen, and in warm baths. The diet must be so arranged that the bowels may be kept open without the aid of strong purgatives. The inunction of mercurial ointment may be had recourse to, and in very chronic cases we may apply iodine ointment to the abdomen, and, at the same time, give iodine internally. When suppuration can no longer be avoided, the formation of the abscess must be promoted by cataplasms, and, when the operation can be effected with safety, it must

be opened. When extensive suppuration has manifested itself, no line of treatment is very successful. The treatment then becomes that of peritonitis. When there is extensive destruction of areolar tissue, or metastatic inflammation, or perforation of the cæcum, we can only adopt such palliative treatment as may postpone the fatal result." (Wunderlich, vol. iii, p. 909.)

We proceed to the *affections of the colon* pertaining to this class. Hæmorrhage from the colon, unaccompanied with other disturbances, is rare, and indeed, when it occurs, we cannot ascertain with certainty that the blood does not proceed from the small intestine, although in the former case it may be presumed to be discharged by the anus more rapidly and in a less decomposed state. It is, however, a not rare concomitant of other diseases of the colon—of dysentery, and of inflammations and ulcerations of various kinds.

We shall first notice colitis, or, as Canstatt terms it, colonitis—an affection, as we shall presently show, quite distinct from dysentery.

Colitis, both in its acute and chronic forms, is a common disease, being, according to Wunderlich, "very often primary, but still more frequently occurring in the course of other affections, especially those of a chronic nature." So common is it in childhood, that Rilliet and Barthez found more or less disease of the colon in half the number of bodies they examined. It is more common in women than in men, being, as Wunderlich considers, often dependent on menstrual disturbance. Amongst the causes that have been enumerated by authors as giving rise to this disease, we may mention a sedentary mode of life, sudden changes of weather, unsuitable food and drink, chills, abuse of purgatives and clysters, of spices and alcoholic fluids, and of acid or fermenting wine or beer. It may remain after an attack of dysentery, and may occur as a secondary affection in hæmorrhoidal attacks, in diseases of the stomach, intestines, and liver, in peritonitis, in uterine and ovarian disease, and in all diseases of the blood, especially towards the end of their course, when they are about to terminate fatally.

We have condensed the following remarks from Wunderlich's observations on the pathological anatomy of this disease.

Although hyperæmia may occur in the colon in every degree of intensity and extent, extreme vascularity is chiefly observed in secondary and chronic forms. We hardly ever find the whole colon equally hyperæmic throughout, there being generally only isolated spots, which are strongly injected, while the remainder is less hyperæmic, or even pale. Sometimes there is a disseminated hyperæmia, with intervening pale spots, throughout the whole colon. The redness is often very pale, but it may present every shade, from the highest scarlet to a livid or even black tint. In several cases the submucous areolar tissue is injected, softened, tumid, and admits of the easy detachment of the mucous membrane.

In the descending colon and in the sigmoid flexure, we find, more frequently than in any other part of the canal, plastic exudations, pseudo-membranes of considerable firmness, and adhering to the red puffy mucous membrane, and commonly occurring in the form of rings, traversing the whole caliber of the gut. The hyperæmia of the mucous membrane not unfrequently gives rise to superficial erosions of a deep-red granular appearance, which become connected in irregular ulcers. Follicular inflammation is of common occurrence in the large intestine, often giving

rise to ulceration. The follicles at first swell and project beyond the surface of the mucous membrane in the form of little nodules. They sometimes contain a clear viscid fluid, and at other times a turbid and occasionally purulent fluid, and are often surrounded by a vascular ring, or by a brown or slate-gray circle. In a later stage they present a split or cleft appearance, and exhibit a tendency to proceed to ulceration. When the little ulcers are actually formed, the intestine presents innumerable red spots, with a jagged edge and a slight loss of tissue; and, on pressure, a minute drop of pus may be often caused to exude. When the follicles have been thus destroyed, the destructive process extends to the sub-mucous areolar tissue. Each ulcer gradually loses its round form, becoming first elongated, and then indented and irregular, and finally unites beneath the mucous membrane with adjacent ulcers, just as we observe in the ulcers in dysentery. The mucous membrane presents great varieties in its appearance, being often pale, soft, and of a pale-gray tint, at other times of a blackish colour, and sometimes tough and thickened; as a general rule, it is atonic and anæmic, it being only very rarely that it is of a bright red tint. These ulcers seldom lead to perforation, there being usually a thickening induced, not only of the subjacent areolar tissue, but of the muscular coat and subserous tissue, and not unfrequently, exudations on the serous coat developed, which extend far beyond the limits of the ulcers. Follicular ulcers, when not very extensive, and when they have only existed for a short time, heal without leaving any trace; at other times they leave a slight depressed cicatrix. Colitis, accompanied with much destruction of tissue, and leading to perforation, most commonly occurs in cases where the colon becomes implicated in affections of adjacent organs, as of the peritoneum, ovaries, or liver.

In very chronic affections of the colon, we often find this portion of the intestine much dilated, its cells strongly developed, and sometimes filled with incrusting fæces, the walls collectively thickened, but the mucous membrane of a rusty or livid tint, and either dry or the seat of a grayish, tough, or even purulent secretion.

The symptoms differ according as the colitis is acute and primary, acute and secondary, or chronic.

In the first variety the pain is severe, colic-like, and although generally continuous, is sometimes paroxysmal, and there is considerable abdominal distension; sometimes there is constipation, but much more frequently diarrhoea, the dejections being very numerous and abundant, and at first fæcal, but subsequently thin, tinged with blood, and passed with much pain. Tenesmus becomes soon established, although in a less degree than in dysentery. This condition of things may persist for some time, and then gradually disappear. The pain may soon reach a high degree of intensity, extending to the lower extremities, and then to the testicles, and causing dysuria, headache, and nausea; and the tongue is generally, but not always, coated. These apparently severe attacks often abate rapidly, the dejections becoming less frequent, the pain diminishing, and all the bad symptoms often disappearing in the course of a week.

In the second variety—in acute secondary cases—the progress and the importance of the colitis are extremely variable, everything depending on the morbid condition giving rise to the colitis. If it arise from intestinal

incarceration, or from destructive processes extending to the colon, its progress is very rapid and fatal; this is also the case when it supervenes towards the close of exhausting diseases, in which case fetid stools often pass involuntarily, and increase the exhaustion. In all cases of secondary colitis, pain may be altogether absent; and we may be only led to suspect the presence of the disease from the extreme frequency of the dejections.

Chronic colitis manifests itself in various ways. There may either be obstinate diarrhoea, continuing with occasional remissions even for years, the appetite remaining almost unaffected, but emaciation and œdema of the limbs finally supervening; or there may be torpor of the colon, with abdominal tension, occasional pains, flatulence, obstinate constipation, with the occasional evacuation of hard nodules covered with blood or pus, alternating with severe attacks of diarrhoea. Sometimes the predominating symptoms are apparently associated with other organs, and we have palpitation, chronic vomiting, dry spasmodic cough, cerebral congestion, or neuralgia of the fifth or other nerves. In this torpid form there is usually loss of appetite, the countenance is blanched, there is dyspnoea, and a general feeling of dullness and depression, which not unfrequently merges into decided hypochondriasis or melancholia.

We have omitted in these remarks all notice of this affection as it occurs in early childhood, as Wunderlich has added little or nothing to the admirable account given by Billiet and Barthez of this disease.

Slight cases of acute primary colitis require little treatment. Rest, a non-stimulating diet, warmth to the abdomen, and perhaps a mucilaginous mixture of some sort, are all that is required. In more severe cases, recourse must also be had to the local abstraction of blood. If the diarrhoea be a very prominent symptom, we must combat it with small doses of ipecacuanha with opium, with starch enemata with or without opium, or with enemata containing nitrate of silver, and by applying counter-irritants to the abdomen and feet. It is only, however, when it is extremely severe that we should resort to stronger means, as to the vegetable astringents (columba, ratanhia, and kino), or to alum or acetate of lead. When there is a tendency to collapse or sopor, it may be necessary to sustain the vital powers by quinine, wine, or musk.

In acute secondary cases, we must be cautious in the employment of local bloodletting. We may first try mucilaginous or nauseating medicines; if these are insufficient, we must have recourse to warm irritating applications to the abdomen, to opium both by the mouth and in enemata, or to injections of nitrate of silver, and, finally, if these fail in arresting the diarrhoea, we must have recourse to the stronger astringents, as extract of rhatany, the salts of iron, acetate of lead, alum, &c.

In chronic colitis with diarrhoea, the greatest attention must be paid to the diet; the food must be nutritious, and should consist in a great measure of milk and of a little white tender meat, while all acids, spirituous drinks, indigestible articles of food, and the use of tobacco, must be strictly interdicted. If the strength of the patient will bear it, leeches may be applied to the abdomen or anus. If the milder medicines fail, recourse must be had to opium in moderate doses, to the extract of nuxvomica, and to the metallic astringents. Clysters of cold water, of a weak solution of nitrate of silver, or of starch with a little opium, may

be prescribed every two or three days. In these cases we have often to try many remedies before we succeed in finding the most successful one.

When chronic colitis is accompanied with intestinal torpor, we must unload the bowels, without having recourse to irritating purgatives or enemata. Injections of cold water, or injections containing soap, oil, or salt, may be used; while for internal administration we may prescribe castor-oil, castile soap, curds and whey, or some mild aperient mineral water mixed with milk. Warm baths, warm clothing, moderate exercise, and friction of the abdomen are serviceable, and much benefit is often derived from the application of a few leeches to the anus. Due attention to the diet is of course imperative.

Wunderlich concludes this subdivision—that of hyperæmia, hæmorrhage, &c., affecting only a single portion of the intestinal canal—with a notice of the *affections of the rectum and anus*. These he considers under the heads of—

- a. Hypertrophy of the sphincter ani muscle.
- b. Hyperæmia of the rectum.
- c. Hæmorrhage of the rectum.
- d. Acute catarrh of the rectum and inflammation of its mucous membrane (Proctitis).
- e. Acute periproctitis.
- f. Chronic catarrh and blennorrhœa of the rectum; chronic proctitis and periproctitis.
- g. Hæmorrhoids.

As chronic catarrh of the mucous membrane of the rectum is a very common disease, occurring both as a primary affection and as a sequence of acute hyperæmia and inflammation, and as it is one which often does not readily yield to treatment, we shall give a sketch of Wunderlich's views of its symptoms and treatment.

In simple chronic catarrh there is considerable, but seldom very intense pain; constipation is always present, hard masses of fæces covered with mucus or pus being occasionally passed with considerable distress; and there are sensations of gnawing and itching about the anus. On examining the rectum with the finger no impediment is detected, and the mucous membrane does not seem specially sensitive.

In blennorrhœa there is a more abundant secretion from the mucous membrane, and mucus is often passed unaccompanied by any fæces. The mucous membrane is relaxed and comparatively devoid of sensibility, or else it is ulcerated.

After noticing the best dietetic and medicinal means of overcoming the constipation, he proceeds to observe that the mucous secretion is best treated by mild, lukewarm, or cold injections, and that if they are insufficient, we must add to them a little nitrate of silver or a mild astringent. If the mucous discharge be very obstinate, we must examine with the speculum whether ulcers are not present, and if this be the case, and they are within reach, caustic must be applied to them, or they must be treated with astringent injections or ointments. When the constipation and the mucous discharge are dependent on a very relaxed condition of the mucous membrane, at a point beyond the reach of enemata, sulphur will often be found useful, and the bitters—as millefolium, gentian, trifolium, (*menyanthes trifoliata*, Ph. L.), and wormwood—may be tried. Injections



of cod-liver oil have also been found serviceable. After a laxative treatment has been pursued for some time, Wunderlich recommends a short course of a chalybeate and saline mineral water.

In Wunderlich's second subdivision—hyperæmia, hæmorrhages, quantitative disturbances of nutrition and secretion, catarrh, and inflammation, affecting not merely a single portion of the intestinal canal, but several parts simultaneously, there are included:—

1. Acute gastro-intestinal catarrh and acute gastro-enteritis.
2. Cholera.
3. Dysentery.
4. Chronic gastro-intestinal catarrh, chronic gastro-enteritis, and chronic entero-colitis.

We shall give a brief sketch of his views regarding *dysentery*.

After entering at some length into the epidemic character of this disease, he observes that its sporadic occurrence as a primary affection is very rare; although attacks resembling dysentery are often observed after the administration of strong drastic purgatives, in cases of metallic poisoning, or in acute exacerbations of hæmorrhoids, or of common diarrhœa.

When it occurs as a secondary disease, it may be either epidemic or sporadic. It not unfrequently follows typhus, although, according to Rokitansky, it is never simultaneously present with typhous affection of the bowels; it is also a frequent sequence of pneumonia, a still more frequent complication of puerperal fever, and sometimes shows itself during the progress of a non-puerperal peritonitis. It also occurs in inflammation of the liver, in which case he observes that the hepatic abscesses may be regarded as metastatic and consequent on the dysenteric affection of the colon, and on the infection of the portal blood. Finally, dysentery is common in countries where malignant, remittent, or intermittent fever predominates, the two diseases being often combined. It is possible, as Wunderlich suggests, that the dysentery may be the primary affection, and the rigors only the indication that the blood is infected.

We extract Wunderlich's observations on the pathological anatomy of this disease, which, however, we may observe, are in a great measure borrowed from Rokitansky:

“The changes in dysentery are limited to the large intestine; it is only rarely (and then in a slight degree) that the lowest portion of the small intestine is implicated. They are most strongly developed in the rectum and sigmoid flexure, and usually, only in a far less degree, in the upper part of the colon and in the cæcum. These changes may be arranged in several degrees or classes partly corresponding to the extent of the development of the disease, and partly to the fact that in severe cases the greater changes occur in the lower, and the lesser changes in the upper part of the canal. The division adopted by Rokitansky into four stages or degrees is a very excellent one.

“In the lowest degree we find in the large intestine a thin, dirty, gray and reddish secretion, underneath which the mucous membrane at the *Plicæ sigmoideæ* is reddened and swollen in narrow striæ encircling the canal. The epithelium at these spots is sometimes raised in small vesicles (Linnæus's *Scabies interna*), and sometimes scales off and presents a bran-like appearance, or peels off in grayish or white layers. The subjacent mucous membrane appears red as if excoriated, is somewhat soft, and if the handle of the scalpel be passed over it, a light red, viscid, and sanguineous secretion exudes.

“Second degree. The above-described individual alterations extend over a large surface instead of being mere striæ; presenting, however, a greater development at some spots than others. At these spots the mucous membrane is converted into a soft, sanguineous, gelatinous substance, which may easily be detached. The intestine presents wart-like protuberances, which were formerly regarded as an hypertrophy of the sub-mucous areolar tissue, but which, according to Rokitansky, are merely a sero-gelatinous infiltration of that tissue. They do not occur at all the spots, but only at those where the disease is most deeply seated in the mucous membrane. At the other spots the sub-mucous tissue is simply swollen. In this stage or degree the intestine is distended with gas and coated with a dirty reddish-brown secretion, or sometimes with plastic exudation.

“Third degree. The wart-like protuberances are here more closely arranged, so as to give to the whole of the inner surface of the gut an embossed, irregular appearance. The sub-mucous infiltration is at least in part purulent; the mucous membrane is partly puffy, and appears of a dark red, blackish-brown, or dirty green (moss-like) tint, and is partly converted into a slough; it often disappears at various spots of greater or lesser extent, leaving only dark red, loose, bleeding fragments, and exposing the infiltrated and irregularly swollen sub-mucous areolar tissue. The intestine, which is considerably dilated, contains a brownish-red, very fetid, flocculent, ichorous matter.

“Fourth degree. In the highest degrees, which, however, in our epidemics only rarely occur, but are frequent in cases of so called putrid dysentery, the mucous membrane becomes converted, in great patches, into a black, friable, and, as it were, carbonized mass, which during life is sometimes voided in larger or smaller pieces. The submucous areolar tissue is either infiltrated with a sero-sanguineous, or a black and, as it were, carbonized mass, or it remains bleached, and the blood contained in its vessels is solidified and carbonized into a pulverulent, crumbling sand. Further, at certain spots we find greater or lesser accumulations of pus or ichor. The muscular layer is bleached, thickened, and lacerable. The intestine is still sometimes found in a state of passive dilatation,—but more commonly it is collapsed, and is filled with an almost black fluid of a putrid odour.

“In its further progress the second degree sometimes proceeds to ulceration; and this is always the case with the third degree (if death does not occur too early), and with the fourth degree, if a tendency towards healing be exhibited. These ulcers are of various kinds, according to the period in which they have been formed, according to the degree of the intestinal affection which has given rise to them, and according as the vessels of the intestine are overloaded or not. Consequently, we have in part follicular ulcers and in part extensive ulcerations; the former belonging especially to the second degree of dysenteric degeneration, and the latter to the third and fourth degrees. The base of the ulcer which lies in the sub-mucous areolar tissue, or if this be destroyed, in the muscular layer, appears sometimes pale, of a bad colour, and sunken, and at other times red, covered with bloody specks, and swollen. The edges formed by the mucous membrane are irregularly sinuous, sometimes rounded off, sometimes presenting a torn appearance, sometimes swollen and again on other occasions flaccid; they are of a pale or dirty gray colour, and generally more or less undermined. The longer a case has gone on without showing a tendency to heal, so much the more extensive are these underminings. They may, however, often be observed in the dead body in a tolerably early stage of the disease. The ulcerated surfaces are thus often perfectly covered by overhanging mucous membrane, and can only be discovered on moving it aside; when however this is done, they are seen to be of great extent in the sub-mucous areolar tissue. The swollen and infiltrated mucous membrane lies loosely on these surfaces, and when the ulcerations are exposed, its fragments form, as it were, bridges crossing and appearing to divide the ulcerated surfaces. These ulcerations may in the course of time contract in length, and thus heal and cicatrize, or they may go on increasing in extent.

“In addition to these changes on the inner surface of the intestine, its peritoneal

coat is seen to have more or less lost its glistening appearance, to have assumed a dirty gray tint, and sometimes to be invested with a plastic exudation, or, in the highest degree of the disease, with bad-coloured ichorous exudation. The glands in the mesocolon are sometimes congested, of a dark blue colour, and tumefied.

“The healing of the anatomical changes takes place, in the less highly developed form of the disease, by a gradual restoration of the mucous membrane to the normal state, by resorption of the submucous infiltration, and the formation of a new epithelium in place of that which has peeled off. Moreover, moderately extensive ulcerations may doubtless heal, without leaving a trace of their existence, by the glueing together of the undermined mucous membrane, and the transformation of the open ulcer into the tissue of mucous membrane. In the higher degrees the loss of substance can only be remedied by the formation of a cicatrix. Fibrous bands and cords, often of a tolerably regular circular form, interlace with one another, and encroach upon the caliber of the canal in the shape of hard, valvular projections, giving rise to stricture and its consequences.” (Wunderlich, vol. iii, pp. 959-61.)

We shall follow our author briefly through his observations on the symptoms and treatment of dysentery; thus allowing our readers to see the points of resemblance and contrast between this disease and colitis—an affection with which it is not unfrequently confounded, not only by practitioners but by medical writers.

Dysentery may either be preceded for some days, or even weeks, by diarrhoea, or it may burst forth at once in all its strength. Common, non-malignant cases begin with slight colic-like pains in the abdomen, which are generally increased by pressure, and are localised for the most part in the sigmoid flexure and the rectum. They are at first remittent, but subsequently become permanent. There is great tenesmus, and as it were a feeling of heavy weight about the lower part of the bowels, and a tearing or burning feeling about the anus. The dejections are at first fæcal, but they soon become scanty, thin, and mixed with blood. The tenesmus is most urgent, but after straining for a long time the patient only passes a few drops of blood or bloody mucus; and the desire is almost immediately re-established. Sometimes the discharges are of a purulent character, and are almost entirely or quite devoid of blood. In this case the disease is termed *dysentery alba*. Sympathetic irritation is often set up in the bladder, and there is urgent dysuria.

Amongst the general symptoms the most prominent are rigors, loss of appetite, nausea, and not unfrequently vomiting, a coated tongue, very urgent thirst, a dry, hot skin with sometimes roseolar eruptions, a tendency to extreme debility, and sleeplessness. If in the course of two or three days the local symptoms do not moderate, there is usually a great augmentation of febrile disturbance; and in addition to the above symptoms in a more aggravated form, we have tenseness of the abdomen, a small and frequent pulse, and coldness of the hands and feet, while the rest of the body is burning hot. There may be also slight delirium and great nervous excitement. If the local affection is still advancing unchecked, collapse ensues, the stools are either no longer passed or are passed involuntarily, the belly becomes more tympanitic, the body cold, the pulse almost imperceptible, the tongue black, and the patient finally sinks into a state of great emaciation. Where, on the other hand, the case progresses favorably, the tenesmus gradually diminishes, the stools again

become pultaceous and afterwards solid, the fever disappears, and the skin regains its proper condition.

Wunderlich commences his remarks on treatment with some introductory observations on the importance of due attention to hygienic measures whenever this disease is epidemic. We pass over these, and proceed to the treatment to be adopted when the disease actually develops itself.

If, during the prevalence of an epidemic, a person complain of loss of appetite, general *malaise*, and diarrhœa or colicky pains, a warm flannel bandage should at once be applied, while a rigid, chiefly mucilaginous, diet must be prescribed, all spirituous and heating drinks forbidden, and the horizontal position enforced.

If, from the commencement of the attack, the patient complain of nausea and a tendency to vomiting, of a bitter taste in the mouth, and of a sensation of fulness in the region of the liver, an emetic often cuts short the disease: if, however, this be ineffectual in checking the disease, a few large doses of calomel may be given. These medicines, with some mucilaginous drinks to appease the thirst, and due attention to warmth, rest, and diet, are usually all that are required. If, however, the pain and the febrile symptoms increase, local or even general bloodletting must be had recourse to. If the heat of the body and the general irritability are not too great, warm cataplasms may be applied over the abdomen. When the pain and the number of the dejections still continue to increase, ipecacuanha in moderate doses (gr. vii—xv in the course of the day), and opium (in grain doses), as also the use of the warm bath, are indicated. When the local symptoms run very high, and the peritoneum becomes implicated, local bloodletting and the inunction of mercurial ointment into the abdomen and thighs must be had recourse to; and calomel is also serviceable, especially when there is an accumulation of fæcal masses in the upper part of the intestine.

When deep prostration ensues, and we have all the symptoms of an adynamic condition, we must prescribe quinine, simaruba, wine, ether, camphor, &c., and, at the same time, try to check the dejections by opium clysters, or, where they are very profuse, by astringents, as rhatany.

The most essential points in the treatment of the chronic forms of dysentery, are the application of mild mucilaginous clysters, and, in a later stage, of narcotic and astringent clysters; also the cauterization of such ulcers as can be reached.

Wunderlich's fifth great division of the diseases of the intestinal canal, accompanied by anatomical changes, is devoted to the subject of typhus, which is admirably treated in the sixty pages devoted to its consideration; and we regret that our limited space prevents us from making any extracts from this section. Whether he is justified in placing typhus among the diseases of the intestinal system, is a question on which we need not at present enter.

We must also pass over the succeeding division on intestinal tuberculosis, and proceed to the seventh great division—*cancer of the intestinal canal*.

In his remarks on cancer of the stomach, he confirms the observations which have been previously made, that this disease may exist for a long time, and make very considerable progress, without giving rise to any characteristic symptoms. The occurrence of symptoms depends not so

much on the actual progress of the disease, as on special conditions, which are only in part connected with the essential development of the degeneration, and in part on subordinate and incidental relations.

Generally at the commencement, and sometimes during a long period of the course, of cancer of the stomach, the symptoms are those of chronic gastric catarrh, which is present as a complication; they are dyspepsia, acidity, pyrosis, impaired digestion, a feeling of pressure on the stomach, thirst, loss of appetite, constipation, hypochondriasis, and febrile disturbance. If these symptoms become exacerbated, the disease presents the features of acute gastritis, as is often observed when, towards the end of the case, softening begins to take place. The chronic pains are in general greater and more obstinate when the stomach is displaced, either by other organs or by depression into the abdominal cavity.

The occurrence of further symptoms depends, according to Wunderlich, upon the following circumstances:

1. On the degree of hardness, and on the rapidity of the extension of fibrous cancer; in proportion to its hardness and the rapidity of its growth is the severity of the pains. Soft cancer slowly extending itself may be perfectly devoid of pain.

2. On the degree in which the cancerous infiltration and vegetations impede or prevent the passage of the contents of the stomach through the pylorus: it is upon this condition that the distension of the stomach, the pains after the ingestion of food, the vomiting of food, the constipation, the diminished nutrition, and the marasmus depend.

3. On the size of the cancerous tumour, on its seat, and on the position of the stomach; on their condition the objective symptoms ascertained by palpation and percussion depend. While these are the only means that enable us to decide with perfect certainty regarding the existence of this disease, we must recollect that it is only when the cancerous infiltration and nodules have attained a certain extent and hardness, and when they are situated on the anterior wall of the stomach, or on the great curvature; or when, if the pylorus be affected, it is not overlapped by other organs, that they can be detected by palpation. While palpation usually affords diagnostic signs in this disease earlier than percussion, there are occasional cases of extensive cancer with a flat superficies, which can be detected by the dull sound they yield on percussion, but which palpation would fail to recognise. We must be on our guard not to confound cancerous tumour with enlarged glands, fecal masses, or an enlarged pancreas; and must not overlook the circumstance, that in these cases the stomach is often more or less displaced, and that the tumour may thus be found in a position where it would hardly be expected. The stomach should be carefully examined both in its full and its empty state.

4. On the ichorous disintegration of cancerous masses, giving rise to the vomiting of matter resembling coffee-grounds or of ichorous matter, to the cachectic appearance, and to hectic fever.

5. On the erosion of blood-vessels in the cancerous mass undergoing this ichorous disintegration, giving rise to gastric hæmorrhage.

6. On imperfect nutrition, giving rise to general debility, anæmia, marasmus, and serous infiltration of the extremities.

7. On the extension of the cancer to other organs, giving rise to many symptoms, of which the most important are jaundice (when the disease



extends to the neighbourhood of the gall-bladder, to the duodenum, or the liver), dyspnœa, cough, hiccup (when the diaphragm is affected), distension and pain of the abdomen, ascites (in cases where it extends to the peritoneum), and dysphagia (when it extends to the œsophagus).

8. On perforation of the stomach, in which case we have severe pains, collapse, and a hardly perceptible pulse; if the perforation take place into the peritoneum, symptoms of septic peritonitis are rapidly established; if, on the other hand, the perforation point outwardly, a tumour on the abdominal walls is soon perceived, emitting first a dull, but subsequently a tympanitic sound, with emphysema and ichorous destruction of the abdominal parietes. In either case death rapidly supervenes.

9. On the occurrence of general cancerous cachexia, the patient assuming a dull earth-coloured, grayish-yellow tint, the skin being attenuated, dry, and peeling off in flakes, or even covered with true pityriasis, while the strength rapidly diminishes and marasmus supervenes.

10. On the manifold complications with diseases of other organs which may incidentally supervene.

There is nothing novel in Wunderlich's observations on the treatment of this disease. He expresses his doubts whether iodine, calendula, nitrate of silver, cicuta, &c., exert any action on really existing cancer, and believes that we can do little more than treat the individual symptoms and complications.

We pass on to the ninth division in Wunderlich's arrangement—the processes connected with *mortification*, namely, ulcers, softening, and gangrene.

The ulcers which are found in the different parts of the intestinal canal may arise from very different morbid processes; as, for instance, from capillary apoplexy, from hyperæmia and inflammation, from typhous infiltration, from tuberculous and cancerous deposition and softening, or from syphilitic infection.

Simple gastric ulcer (Rokitansky's perforating ulcer, and Cruveilhier's simple chronic ulcer of the stomach,) is a somewhat common disease. In the examination of 2330 bodies in the Prague Hospital (recorded by Jaksch), round gastric ulcers were found fifty-seven times, and the cicatrices of such ulcers fifty-six times; hence this form of disease, or its traces, may be met with once in every twenty or twenty-one bodies. All observers agree, that it is more common in the female than in the male sex, and that it is rare before the period of puberty. It is often associated with other serious affections—as Bright's disease, pulmonary emphysema, heart-disease, cancer (especially cancer of the stomach), and most frequently, according to Jaksch, with pulmonary tuberculosis.

It may run its entire course without giving rise to any marked symptoms, either till perforation and a sudden fatal termination ensue, or till it cicatrizes and heals. It is usually very chronic in its progress.

The following are the general principles of treatment as laid down by Wunderlich.

The greatest attention must be paid to the diet, which should be principally composed of milk, except in those cases where a milk-diet obviously disagrees with the patient. In some cases fruits only can be borne, and in others only thin broths or the mildest forms of animal food.

Strong bodily exertion and mental excitement are hurtful; while on the other hand, gentle exercise and a composed state of mind are serviceable. The bowels should be made to act at least once daily, and this is better effected by clysters than by medicines administered by the mouth. Painful exacerbations must be checked by local bloodletting, warm fomentations, and sinapisms to the legs and feet; and in very severe cases ice may be given internally, and the diet must be restricted to thin mucilaginous drinks, and to milk mixed with water and surcharged with carbonic acid.

He regards the use of medicines in this disease as problematical. A moderate and judicious use of narcotics (hydrocyanic acid, morphia,) appears, however, to allay the extreme irritability and sensibility of the stomach; and zinc and bismuth, calomel, acetate of lead, and alum, deserve a trial in severe cases. Carbonated mineral waters, with a very slight impregnation of iron, are useful when the stomach is not in a very irritable condition. Jacksch recommends the milder Carlsbad waters in this disease.

In cases where signs of improvement have taken place, and there is every reason to believe that cicatrization has ensued, the greatest attention must still be paid to the diet. The stomach should never be over-loaded, perfect rest should be maintained during the process of digestion, and the state of the bowels should be duly regulated by clysters and the mildest form of aperient medicine.

We must pass over Wunderlich's excellent chapter "On Softening of the Œsophagus and Stomach" with the single observation, that it contains an admirable epitome of all that has been written on the subject from the year 1811, when Jäger\* first described softening of the stomach as an independent disease, to 1847, when Dietterich† published his comprehensive essay on this disorder.

Gangrene occurs more commonly in the intestines, than in the stomach or œsophagus. Amongst its principal causes we may place extreme and absolute *stasis*, induced by a mechanical impediment to the venous circulation (as in incarceration, volvulus, &c.), excessive distension of a portion of the canal, and inflammation induced by the local or general action of poisons, or occurring in certain morbid conditions of the blood (pyæmia). Pre-existing ulcers in the intestine may also sometimes give rise to it. In the large intestine it not unfrequently occurs at spots where cancerous matter has been deposited, and after instrumental or very tedious labours; and in cases of uterine cancer it may destroy the recto-vaginal septum and thus attack the gut.

There are two ways in which, under favorable circumstances, gangrene is capable of healing; either by simple sequestration of the slough and cicatrization of the subjacent ulcer, or by the detachment and ejection of a whole loop of gut, in which case the upper and lower portions are united together, either directly or by the formation of a distinct peritoneal pouch. It may also be said to heal, although imperfectly, when, after rupture of the intestine has taken place, there is a persistent opening between the canal and the surface of the body (artificial anus).

Wunderlich notices the extreme difficulty that there often is, in distinguishing between intense stasis and gangrene. If during life a portion of

\* Jäger, in Hufeland's Journal, vols. xxxiii and xxxvi.

† Dietterich, Die krankhafte Erweichung und Durchlöcherung des Magens und Darmcanals, 1847.

intestine suspected to be gangrenous lies exposed before us (as in a hernia during an operation), and by gentle motion or pressure of the finger the blood can even for a moment be removed from the part, but sooner or later returns into the emptied capillaries, then there is no gangrene; the failure of this experiment is, however, no proof that gangrene is actually present. Even after death, the distinction is often by no means easy.

We must here close our notice of "The Diseases of the Intestinal Canal," in so far as they are treated of by the authors the titles of whose works are prefixed to this article. We trust, in an early number, to present our readers with a similar sketch of their views regarding "The Diseases of the Respiratory Organs."

#### ART. IV.

*A Treatise on the Etiology, Pathology, and Treatment of Congenital Dislocations of the Head of the Femur. Illustrated with Plates.* By JOHN MURRAY CARNOCHAN, M.D., Lecturer on Operative Surgery, &c. &c. —New York, 1850. 8vo, pp. 235.

WE believe that a pathologist might go through the various museums of the metropolis, and many of those in the provinces, without being able to find a single specimen illustrative of congenital dislocation of the head of the femur. We know many surgeons of great experience who have never met with such a case, and others who have only seen the displacement in the foetus, or in some infant which scarcely survived its birth. We conclude, therefore, that such dislocations must be very rarely met with in practice. Dr. Carnochan, on the other hand, believes that "the congenital form of luxation of the femur, more especially that variety where the femur ascends upon the dorsum of the ilium, is of as frequent occurrence, if not more so, as the corresponding dislocation caused by external violence during extra-uterine life." (p. 35). This is really absurd. There is not a large hospital in London, in which several examples are not annually admitted, of traumatic dislocation of the head of the femur upon the dorsum of the ilium. Dr. Carnochan seems to think that the congenital variety is not recognised, because it is a "pathologic condition of the joint not generally understood." This may be the case in America; but we can assure him that in this country, since the publication of Dupuytren's memoir on this special subject, in 1826, surgeons have very generally kept in view the possibility of the condition in question, when examining cases of disease or injury about the hip-joint. Dupuytren met with twenty-five cases. Dr. Carnochan says he has seen "at least twenty" since 1840. We wish he had favoured us with the exact number, the sex, and the ages of the patients, the supposed causes of the displacement, and a summary of the appearances observed. Such a contribution to practical surgery would have been a far more valuable one than the sort of essay he has compiled from the memoirs of Dupuytren and Pravaz, without one single word respecting the treatment of the "at least twenty cases" which fell under his own observation.

Various causes have been assigned for the intra-uterine dislocations of the femur. Petit attributed them to external violence acting through

the mother upon the foetus ;—Dupuytren, to a primitive alteration in the germ, or an aberration of formative power ;—Breschet to an arrest in the development of the bones forming the acetabulum ;—Parise and others to disease of the joint during uterine life ;—while Chaussier, and more recently Guérin, look to a primitive alteration in the nervous centres. Dr. Carnochan assumes that the cause is “a pathological spasmodic retraction of the muscular tissue, resulting from a perverted or disturbed condition of the excito-motor apparatus of the medulla spinalis ; especially of that portion which is in direct relation with the nervous branches distributed among the pelvi-femoral muscles.” (p. 50.)

The most prominent symptoms of this form of dislocation are, that lameness has been observed from the first attempts to walk, and that it has not been preceded by injury or disease of the joint. The gait is *hobbling*, the abdomen prominent, the lumbar region hollow. There is generally a similar dislocation on both sides of the body. The great trochanter is unusually projecting, and the head of the femur ascends and descends upon the surface of the ilium through an extent of nearly three inches. Motions of the femur do not cause pain. This assemblage of symptoms is perfectly characteristic.

In the chapter on the Pathology of the dislocation in question, we find some records of personal observation. In a female, about fifty years of age, whose body was examined by the author—

“The head of the femur had passed through the upper part of the capsular ligament, and rested upon the external surface of the ilium, about an inch from the border of the acetabulum. A semicircle of new osseous material springing from the ilium, towards forming a new acetabulum, prevented the femur from ascending farther in this direction. The cotyloid cavity, although nearly circular, had become more shallow, but was sufficiently large to allow the head of the femur, which was not much under the natural size, to pass into it. Owing to the disappearance of a lunated portion of the upper circumference of the acetabulum, the head of the femur could readily pass from this cavity upon the ilium, and reduction could thus easily be effected, but in this condition of the parts it could never have been rendered permanent.” (pp. 151-2.)

In another case, the acetabula had become triangular, and were nearly filled up with “softish *adipo-osseous* tissue.” In a third they were contracted to a very small size, and were “filled entirely with a *semi-osseous* tissue.”

The various shapes and degrees of deformity assumed by the head, neck, and shaft of the femur, are described at some length, with the alterations of the ligaments. The following observation upon the new capsule and new acetabulum are worthy of attention :

“*New Capsule.*—When the head of the femur has escaped from its natural capsule, and become placed in contact with the surface of the os ilium, a new set of phenomena takes place. Reparatory efforts are made to restore the head of the bone to something like its former condition, and nature attempts to form a new capsular ligament, which on the one hand is attached to the dorsum and contiguous portion of the ilium, and on the other, to the outer and posterior surface of the old capsule, and to the margin of the perforation through which the head of the femur had made its escape. Or, the head may never have escaped upon the ilium, and in that case, in proportion as the head becomes atrophied and disappears, the sides of the old capsule become coalesced and glued together, as previously remarked. To the posterior part of the cord thus formed, a set of radiating fibres, springing

from the dorsum of the ilium, become attached, and fortify it. These fibres probably represent, in another form, a recent capsule, which, under other circumstances, as when the head has pierced the old capsule and rests upon the dorsum ilii, would assume a regular capsular form.

*"New Acetabulum or Socket."*—By the formation of the new capsule, a false articulation is partly accomplished, which, to be complete, requires a new arrangement upon the dorsum of the ilium, to represent a new acetabulum. This end is attained in two different ways. In one, there is a simple glenoid depression scooped out, as it were, upon the dorsum of the ilium. In the other, nature increases her efforts, and, as often happens after traumatic dislocations, new osseous matter is thrown out upon the ilium, which, at times, assumes to some extent the cup-like form of an acetabulum. In the first case, the new capsule attaches itself to the margin of the depression; in the second, to the osseous border of the new socket.

"M. Guérin has stated that it was an invariable sequent, for a new osseous socket to be thrown out upon the dorsum ilii, as soon as the head of the femur, in congenital dislocations of this bone, has escaped from the articular capsule, and becomes placed in circumstances somewhat similar to those attendant upon the traumatic dislocation of the femur, and he has founded a theory of treatment upon this supposed fact. The formation of a new socket by osseous effusion, is not an invariable result of the perforation of the articular capsule and the escape of the head of the femur. The drawings of a pelvis, now in my possession, establish the presence of only a simple depression upon the dorsum of the ilium, notwithstanding that the head of the femur has on both sides passed through a large aperture at the upper part of the capsule, and has become placed in immediate contact with the osseous structure of the ilium.

"The secondary depressions, or the sockets, intended to hold a certain relation with the displaced head, after it has passed from its original capsule, are generally lined by a thin layer of tissue, depending probably upon the periosteum of the ilium; and this may at times present a smooth surface, somewhat analogous to the synovial membrane. At other times, from age and continued friction, an appearance of eburnation is present; at others, again, a rough surface. The osseous tissues of the pelvis and of the femur, when thus deprived of the intervention of any soft tissue, come into immediate contact, and when moved upon each other, produce a harsh grating sound.

"Another modification which the capsule is seen to assume, is that in which the head of the bone has never escaped from the capsule, but has become absorbed, and the walls of the capsule coalescing, have finally become conglomerated into a firm, dense, ligamentous cord, running between the remains of the old acetabulum and the stunted upper extremity of the femur. Here there is neither depression nor new socket upon the ilium, and the numerous auxiliary ligamentous fibres, which seem to occupy the place of a new capsule, take their origin from the plain surface upon the ilium, and run forwards, converging, to become incorporated with the posterior aspect of the ligamentous remains of the ancient capsule." (pp. 156-9.)

Approximation of the points of origin and insertion of the pelvi-femoral muscles leads to retraction and partial fibrous transformation of the muscular tissue. This offers such impediments to reduction, that Guérin has proposed and practised subcutaneous section of the affected muscles. Contrary to the assertion of Dupuytren, Dr. Carnochan found that the pelvic diameters were "materially changed from the normal standard."

The chapter on Treatment is illustrated by plates of the instruments employed by M. Pravaz in the reduction of these dislocations, and of a machine by which erect exercise can be taken before even crutches are allowed; but, as we said before, Dr. Carnochan favours his readers with no account of his own methods, nor of his own trials of those of M. Pravaz. Dupuytren believed that there was no curative remedy for their affections,



and no palliative treatment of much value. Bouvier attempted reduction, but failed, and arrived at the same conclusion as Dupuytren. MM. Humbert and Jacquier published some cases called successful in 1835, but grave doubts have been raised as to the authenticity of their reports. One case of a boy, 7 years old, in which the dislocation was reduced by M. Pravaz, was reported on in 1838, by a commission of the Royal Academy of Medicine, as completely successful. The work of M. Pravaz on the subject was published in 1847. M. Guérin has followed in the same course, but he also divides retracted muscles, and practices subcutaneous scarifications around the acetabulum, in order to produce an effusion of organisable matter to remedy the defect in the cotyloid cavity, and thus retain the head of the femur within it. The result in three cases was most favorably reported on by a committee nominated by the Council General of the Civil Hospitals of Paris in 1843. It would appear that in young children, reduction may be accomplished, and the bone retained in the acetabulum. A successful case has been recorded in which the patient was fifteen years old. But in adults it is very questionable whether the confinement necessary for the treatment should be endured to obtain an uncertain good. Even if reduction be accomplished, a halting gait always remains; so that the confinement after all does not effect a cure, but only an improvement. M. Pravaz believes that we may restore the head of the femur to the rudimentary cotyloid cavity, whence it had been dislodged before birth, and maintain it there until it forms for itself a kind of artificial articulation. He employs a frame upon which the patient is laid and fixed, extension being kept up by means of a weight upon the leg, hanging over a pulley for from two to six months, or until the head of the femur is brought below the antero-inferior spinous process of the ilium. Thus he attempts reduction by abducting the limb and pushing the head of the femur from above downwards, and from without inwards. Success is rendered evident by the improved conformation at the joint, and sometimes by an audible sound, as in reduction of traumatic dislocations. Sometimes the head of the femur "can only be brought into its anatomical position and retained there, until the plastic power of the organism be aroused, and the elemental parts of the joint be gradually and mutually adapted to each other." (p. 226.) This process generally requires from five to six months, before the head of the bone and the articular cavity become fitted for each other. The muscles assume a more natural disposition, but several months must still elapse before the limb can support the weight of the trunk without danger of the recurrence of dislocation. The patient takes exercise, in the mean time, in a sort of chair on wheels, resembling a child's *go-cart*, the weight of the body being instanced by a frame passing beneath the axilla. This is replaced by crutches, and lastly, the patient walks unassisted.

We are not of opinion that Dr. Carnochan's contribution to medical literature possesses any remarkable value; but his wish to bring the subject before American and English readers is praiseworthy; and those who do not read French, and are curious respecting hip-diseases, will do well to peruse his book. The French scholar, however, will obtain far more information from the work of M. Pravaz, and from the report of the Commission to which we have alluded, on the cases of the last-named surgeon.

## ART. V.

*Medico-Chirurgical Transactions.* Published by the Royal Medical and Chirurgical Society of London. Vol. XXXIII (Second Series, Vol. XV).—London, 1850. 8vo, pp. 360. With Six Plates and Ten Wood-engravings.

THE receipt of the new volume of the 'Transactions of the Royal Medical and Chirurgical Society,' gives us the opportunity of noticing a few points which have lately obtruded themselves in public notice. This Society, though not the oldest, is by far the most distinguished of the medical societies of Great Britain. Its 'Transactions' contain the most valuable series of single Papers and Essays that the world has ever seen; its Library merits the epithet of "noble;" its funds are considerable and amply sufficient for the purposes to which they might be devoted; and of its Fellows, it may be observed, that there is hardly a distinguished name of the present century, which either is not or has not been enrolled upon its list. With such advantages, the question naturally suggests itself, what has the Society done for the advancement of Science. It were uncandid not to admit that it has done much—very much; but it would be equally uncandid to give to it praise to which it has no legitimate claim. The Transactions which constitute its chief merit are not, it must be remembered, the works of the Society; they are the contributions of its individual members. It might be expected, therefore, that those who have most largely benefited the Society by their communications, would be selected for its most prominent and honorable positions.

It is notorious, however, that such is not the case.—To contribute to the Society's Transactions appears to constitute a very small claim in the Society's good graces; and it is not, in our opinion, an irrelevant allegation against the present system of management, that many of those who compose the Council, have never contributed a single line to this purpose; while others, who have long and thanklessly worked to advance its reputation, are excluded in the most unaccountable and arbitrary manner. Out of doors this injustice, for it is nothing less than injustice, has produced its natural fruits; and wherever we turn, nothing is to be heard but complaints of exclusion, of cliqueism, and defective management. Many of these complaints may be, and no doubt are, unfounded or exaggerated; for any person who has taken the trouble to write a paper for the Society, doubtless considers it worthy of publication, and feels aggrieved at its rejection. We *know* that highly important and scientific papers have been excluded from the Transactions, and therefore, that these complaints are not altogether unfounded; but we do not give to them undue weight. We admit that it is better to keep the Society's annual volume select, even at the expense of an occasional act of injustice. An indignant author is less inimical to the Society's welfare, than an indifferent volume; but when an author's labours have surmounted the fiery ordeal of references and Council, it is rather hard that he should be passed by, while others, who have done nothing, are preferred and elevated above him. It is not our purpose, however, to throw the apple of discord into the Society; we would rather soothe the angry and exhort the weak and erring. We would recommend both the Council and the Fellows to remember

that they are the repositories of the former reputation of the Society, and the joint guardians of its interests. To preserve the one, and to advance the other, is equally the duty of both. Mistrust and dislike are the most effectual means that could be devised for destroying the Society, and with it an institution that might rank with the Académie de Médecine in Paris, and which ought to be a potent means of reconciling differences amongst the various grades into which the medical profession is divided. Science should know no distinction between physician, surgeon, and general practitioner. To carry mutual jealousies into the precincts of her temple, is to insult the goddess, and to prove ourselves unworthy to lay our offerings on her altar.

There is one measure for the improvement of the Society, which has often been urged on the Council, but hitherto without success. We refer to the appointment of Committees for the investigation of disputed points, whether scientific or practical; just as is done, and with great success, amongst our French neighbours, by the Académie de Médecine; and, in matters of general science, by our own British Association. We are quite sure that there are numbers of the younger and more active members of the Society, who would gladly give their time and talent for the purpose, secure of their reward in the consciousness of doing good, and the certainty that they were thereby laying the foundation for their own advancement to the posts of honour in the Society. We would not always pin our faith upon the Report of a Committee, however impartially selected; but there are many points which no one individual, however intelligent and industrious he may be, can efficiently work out, in which associated labour would be almost sure of success, in supplying the means (to say the least) for the settlement of disputed questions. The subjects for such investigation are innumerable; but we may just name as examples,—in medicine, the treatment of rheumatism by lemon-juice;—in surgery, the perinæal section in obstinate stricture of the urethra;—and in midwifery, the question of simple ulceration of the os uteri. Upon each of these points there exists the greatest diversity of opinions, which no one individual can ever reconcile, but which might, without any great difficulty, be set at rest by the labours of an active Committee. Such reports would go forth to the world under the authority of the Royal Medical and Chirurgical Society; and would speedily acquire a high reputation for their authors.

None but those who are altogether blind to the progress of human affairs, can fail to see the necessity of remodelling many institutions, whose constitutions have been admirably suited to the times in which they were constructed, but do not enable them even to maintain their ground, far less to keep themselves in advance, or prepare the way for further progress, without essential modifications. The inevitable consequence of opposition to such changes, or of procrastination in making them, is the formation of new societies, on which the younger and more energetic blood bestows its strength, whilst the older lose their vitality and decline. In the scientific world, we have seen a large proportion of the functions which the Royal Society was originally destined to discharge, gradually transferred to a number of distinct associations; each with its separate building, library, officers, and publications; each burdened with heavy expenses which might have been avoided by their combination, as *sections* of the same General Association; and the entire scientific com-

munity thus deprived of its legitimate influence, and the different departments of research being most disadvantageously isolated from each other. Let any candid man, who is acquainted with the working of the "Royal Society" and the "British Association," say which has done the most for the advancement of British Science during the last fifteen years; we feel assured that the comparison will not be to the disadvantage of the younger institution, hampered though it is by its dependence for its funds upon the amount of *popular* support which it can obtain. The governing body of the Medico-Chirurgical Society should take warning by the experience of other bodies, and not neglect the "signs of the times." Already we have a separate "Pathological Society," and "Epidemiological Society," pursuing objects which, in our estimation, it ought to have itself included; and, if it does not speedily extend the sphere of its operations, or, at any rate, show a disposition to do so, we venture to predict its decadence, notwithstanding the *prestige* it still retains, and the advantages it possesses.

The appointment of officers of the society is also a subject of great importance, to which we have before alluded. The Council should be careful in preparing their House Lists, that they not only select efficient Fellows, but that they also select them from sources as much diversified as possible. To have several officers or members of the Council from one hospital, and none from another, savours of partiality, the least appearance of which it is essential to avoid; and we sincerely trust that the Council will have seen to this in proposing their new List, a month before this friendly caution will make its appearance.

The present volume of the Transactions will fully maintain its reputation for the high character of its contents. The Papers are as numerous, as diversified, and as important as those of any former volume; but many of them are disfigured by unpardonable negligence in the correction of the proof sheets; and we feel sure that the fault lies in the want of adequate supervision, on the part of the individual or individuals, to whom the *Editorial* duty has been intrusted. Whoever has undertaken this duty is either inexperienced or indolent; and we think the Fellows have just cause for complaint on this score. It should be an understood rule, that each author should have the opportunity of revising the proof-sheet of his article before it is finally printed. We know that this course was formerly adopted; why it has been at all deviated from on this occasion, we are not aware; but the consequences are sufficiently evident, and not very creditable to those who have omitted what is little more than an act of courtesy.

*Case of a Foreign Body Impacted in the Orifice of the Third Branch of the Right Bronchus; with Remarks.* By John Gregory Forbes, F.R.C.S., Surgeon to the Western General Dispensary.

This is a well-written paper, and details an instructive case; not the least remarkable feature of which, however, is, that the author's reasoning is diametrically opposed to his practice. Mr. Forbes argues excellently well for the necessity of resorting to early and persevering attempts to extract, by mechanical means, any foreign body which is proved to be lodged in the bronchial tubes; and his own case, in which the foreign material was allowed to remain unmolested, and killed the patient after seven weeks of suffering, forms the best commentary upon his reasoning that could

possibly have been selected. Cases of impaction of foreign bodies in the air tubes are not very uncommon, but are most alarming to the patient and embarrassing to the surgeon. Trustworthy accounts of these accidents are valuable, as confirming, or, it may perhaps be said, as *establishing*, the rule of practice; and therefore this paper properly finds a place in the 'Transactions of the Medico-Chirurgical Society.'

On the 10th of May, 1849, Mrs. W—, a delicate female, 46 years old, swallowed a piece of bone out of some broth, and immediately discovered that it had passed into the windpipe. The next day she was visited by the author of the paper, and some of his colleagues. The stethoscope determined the existence of a foreign body in the air-passages, and its probable situation in or near the right bronchus.

"On ausculting the chest, a marked difference was found to exist between the two sides. On the right, the breath-sound was obscured, the natural vesicular murmur being scarcely perceptible, and a prolonged and peculiar rhonchus was heard throughout the lung, but most distinctly over the point to which the pain was referred, and was more audibly marked during expiration. On the left side the respiratory sounds were feeble, but free from rhonchus, and both inspiration and expiration were lengthened." (p. 7.)

For three days she suffered very severely, and on the 14th of May was seen in consultation by Mr. Arnott and others. The question of operation was then mooted but decided *against*, upon the following grounds:

"1st. There was a doubt as to the nature of the offending substance. If gristle, it might be softened and coughed up, and would necessarily give rise to less irritation and mischief than bone.

"2dly. The clear sound on percussion under the clavicle, and the fact of respiratory sounds being heard there, did not indicate any great amount of obstruction to the entrance of air into the lung.

"3dly. Though a peculiar rhonchus was heard over the right bronchus, its weight, as an adverse symptom, was materially diminished by the comparatively free respiration in the upper part of the lung." (p. 9.)

This was accounted for, after death, by the circumstance that the piece of bone was impacted in the division of the bronchial tube which goes to the middle lobe of the lung. From this time forward, the patient seems to have gone on from bad to worse; the foreign body producing as much irritation as a stone does in the bladder, and as much more mischief to the general health, as the greater delicacy of the lungs would account for. On the 21st of June it is stated that—

"The symptoms underwent considerable remission. On the 8th the pulse was but 80; she had less fever; and was able to sit up in bed and do some work. Her tongue was clean; her cough on the whole less troublesome; and her strength improved. The 'expiratory puff' continued, and she expressed her conviction that the foreign body had not undergone any change of position. She was now allowed a little wine, and on the 17th some quinine was prescribed for her." (p. 11.)

After this, however, the expectorated matter assumed a different character; it became purulent and dingy in colour; and on the 24th the patient spat up two teacupfuls of offensive purulent matter. Appropriate medicines were ordered, but of course without permanent benefit, and on the 5th of July death terminated the scene, and relieved the patient from sufferings which are described to have been most intense, from the violence of the cough and the urgent dyspnoea.



Mr. Forbes's narrative of these sufferings is graphic, and worthy of transcription.

"Apthous eruptions appeared on the tongue and mucous membrane of the mouth, which prevented her from taking medicine, or even nourishment, for some days. She was only able to sip small quantities of milk or brandy and water. She could obtain no ease but in the erect posture, in which she was supported with pillows. The violence of the cough, and the offensive odour of the purulent matter which she expectorated, induced retching and vomiting, which added to her distress and exhaustion. Her nights were passed sitting up in the bed, with scarcely any rest, and at times extreme restlessness. Her brain, however, remained unaffected; and her lips were not livid, though the face gradually put on a waxy, death-like appearance. The chief auscultatory signs now were loud mucous râles, and dullness on percussion over the right side of the chest, with blowing respiration in parts. These were, however, less marked under the right clavicle, the air appearing to enter with more freedom into the upper part of the lung than elsewhere.

"In this state she continued till the 5th of July, when I found her lying in the horizontal posture in her bed, unable to speak; and her death, which had been daily expected for a week, took place at midnight." (pp. 12, 13.)

The *post-mortem* examination was made 16 hours after death. In the right bronchus,—

"At the distance of an inch and a half from the point of bifurcation of the trachea, and five inches and a half from the lower border of the thyroid body, a small piece of bone, weighing, when dry,  $3\frac{1}{2}$  grains, was found, having a concave smooth facet, and a convex rough one, and one very sharp edge, its breadth being three eighths, and its length a quarter of an inch. It was firmly impacted in the orifice of the third branch, given off from the bronchus, which passed into the middle lobe, and this accounted for the comparative freedom with which the air appeared to enter the upper part of the lung throughout the case, as evidenced by the auscultatory signs. The mucous membrane around it was of a vivid red colour, and highly injected, but gradually became paler towards the left bronchus, where, as well as in the trachea, it presented its usual colour.

"The lower two thirds of the right lung were of an ashy slate colour, of dense consistence, very offensive odour, and infiltrated with a purulent fluid. Small portions of it sunk in water, and when washed it had much the appearance of coarse dark sponge, though no distinct cavities containing pus were visible." (p. 14.)

The right carotid artery pursued an unusual course in the neck, passing in front of the trachea, and leaving a space of only one inch between it and the lower border of the thyroid body. This would have complicated the operation of tracheotomy and the searching for the piece of bone, but does not militate against the propriety of the operation, any more than an occasional abnormal position of the obturator artery does against the operation for strangulated hernia. It is not our desire to criticise the treatment of this case; the *post-mortem* appearances speak for themselves; and the author himself seems to admit that he is not wholly satisfied with it. The paper, we repeat, is pleasingly written, and is illustrated by a well-executed lithograph.

## II. On Section of the Tendo Achillis in some Cases of Fracture of the Bones of the Leg. By Campbell de Morgan, Surgeon to the Middlesex Hospital.

The subcutaneous division of the Tendo Achillis is more frequently resorted to for the cure of permanent than of accidental deformities; but

it is an operation that has sometimes been usefully performed in cases of dislocation of the astragalus in which the heel is much elevated; and the object of this paper is to show, that its division, in some cases of fracture of the leg, is not without substantial benefit.

Mr. de Morgan states, that it has never been resorted to in this country, except in the two examples which he relates; although it has been employed in France by Messrs. Meynier, Berard, and Laugier.

Four years ago, Mr. Shaw admitted a patient into the Middlesex Hospital, who had fractured both the bones of his leg just above the ankle-joint, and in whom there was great distortion and twisting of the foot outwards. The foot was easily enough restored to its right position, but could not be retained there by any mechanical contrivance whatever, owing to violent spasm in the muscles of the calf, which threatened every instant to force the bone through the integument. Under these circumstances, the tendon of the gastrocnemius was divided, and with the satisfactory result of quieting the spasm, and permitting the case to progress to complete recovery. It is worthy of observation, that the spasms were at times so violent, as to implicate the muscles of the thigh as well as those of the leg, and to prevent flexion of the knee.

In March, 1849, the author of this paper resorted to the same proceeding, in an almost exactly similar accident, which occurred to a Billingsgate fishwoman, and with the like amount of success. He concludes, therefore, that in these cases there was the twofold benefit of relief to pain and suffering, and of removal of opposition to the replacement and retention of the fractured bones in their proper positions, from this simple and harmless operation. He closes his short, well-expressed, and practical paper, a very model for similar ones, by alluding to M. Bonnet's proposal of dividing the tendon in certain cases of diseased ankle-joint.

III. *On the Identity or Non-identity of the Specific Cause of Typhoid, Typhus, and Relapsing Fever.* By William Jenner, M.D. (Lond.), Lic. Royal Coll. Phys., Professor of Pathological Anatomy in University College, &c.

The author commences his paper with the remark, that some diseases have certain characters so much in common, that they naturally group themselves into a single class, whose subdivisions are compounded of these several diseases. The class is that of "acute febrile diseases," and the maladies included in it are smallpox, measles, scarlet fever, typhus fever, typhoid fever, and relapsing fever. That the three former affections are distinct from each other, although related by certain common characters, is admitted by every one; and the object of the author is to prove, that the three latter diseases are equally non-identical.

In the 'Monthly Journal' of last year, Dr. Jenner attempted to determine the question of identity by an analysis of the course, symptoms, and post-mortem lesions in a certain number of fatal cases of fever; in the present paper he continues the argument, by examining into the identity or dissimilarity of the specific cause. For this purpose, all the cases in which more than one inhabitant in a house were admitted into the London Fever Hospital during three years, have been examined, with a view to determine whether the cases received from the *same house* were of the

*same form* of fever (the divisions of the author being admitted), or were compounded indifferently of all the forms of fever.

In the first place, however, the author enumerates the symptoms of each fever, which he considers to be essential to the diagnosis. He diagnoses relapsing fever from the early feverish symptoms, followed by profuse sweating and apparent restoration to health, on about the seventh day, with a relapse on the fifth to the eighth day, reckoning from the apparent convalescence. In severe cases, jaundice also is present. The diagnosis of typhoid fever is drawn from the eruption (the rose spots): and the diagnosis of typhus fever is also made from the eruption, of which an excellent description is given, and which, from its colour, is denominated the "mulberry rash." The "*rose spots*" are usually scanty in number; each spot has a diameter of about two lines, is slightly elevated, rounded or lens-shaped, never acuminate, disappears completely under pressure, and has an average duration of about two days; successive crops of these spots appear from the seventh to the twelfth day of the disease, to the twenty-first or twenty-eighth. The "*mulberry rash*" of typhus fever appears on the fifth to the seventh day of the disease, and lasts till the end; it consists of (1st) distinct spots, and (2d) what the author terms a "subcuticular rash." The *distinct spots* vary in number, are sometimes very few, but are generally numerous: each spot may have three stages; in the first stage it is slightly elevated, red, and disappears under pressure, so that it cannot often be distinguished from the "rose spot;" in the second stage, which takes place in two or three days, the *mulberry spot* becomes flat, grows darker in colour, and does not disappear on pressure, but only fades, that is, becomes paler. Some spots, but not all, reach a third stage, in which their centres, or the whole spots, become dark purple, and are unaltered by the firmest pressure; changing, in fact, into petechiæ. The *subcuticular rash* seems to be formed of very pale, confluent spots, and is seen indistinctly, as if situated beneath the cuticle; a mottled appearance is thus given to the skin, and the distinct darker spots are seated upon this dingy ground.

These being the diagnostic marks which Dr. Jenner postulates, the following are the facts from which it is supposed that the specific causes of the diseases thus diagnosed are not identical. In 1847, there were twelve houses which furnished two or more cases of one or other of these fevers, but these twelve houses did not furnish indifferently typhus fever, typhoid fever, and relapsing fever; but it was found, that in each instance in which a house furnished a typhus, a typhoid, or a relapsing case, the other case or cases from that house were of the same description of fever, and not of either of the two other forms. Thus, of the twelve houses, five furnished cases of typhus only, two of typhoid only, and five of relapsing only. In 1848 the inquiry was continued on an extended scale. Thirty-four localities furnished two or more cases of fever, one of which in every case was typhus; the other cases or case were also typhus in every instance but one, in which the second case was typhoid. So that, in 33 cases, when the fever was propagated from the first case in a house to other patients, the succeeding cases were also of the same form as the first case. In the thirty-fourth case, the first case had typhoid, the second typhus; but this apparent exception is explained by Dr. Jenner on the

ground, that the subject of the first case had been absent from the locality at the time he was taken ill. These thirty-four localities yielded altogether 101 cases, and of these 100 were typhus. During the same period, one quarter of the whole number of cases admitted into hospital were of typhoid fever; therefore the author argues, that if typhus and typhoid are mere varieties of the same disease, and if one will produce the other, one-fourth of the above 101 cases should have been of typhoid fever, whereas only one was so. In 1848, also, five localities admitted two or more cases of typhoid; not one of the second cases was typhus, although three-fourths of the cases admitted at the same period were typhus. In 1849, the same results were obtained. Every locality which furnished more than one case of fever, furnished the second or third cases of the same kind of fever as the first case; four localities furnished typhoid only; and eighteen localities furnished typhus only.

Having thus proved, that if a case of typhus, typhoid, or relapsing fever occurred in a house, the next case was (with a single doubtful exception) always of the same kind of fever as the first case, the author remarks, that although, in the three years under review, the relative prevalence of typhus and typhoid fevers varied considerably, there was no evidence of any transition cases between the two affections; in 1848, when typhus fever was very prevalent, the comparatively few cases of typhoid fever were as easily distinguished as before; and in 1849, when the epidemic of typhus had nearly subsided, the cases that did occur were as well defined as ever. The author then adverts to some special cases, which support strongly the inference deducible from the above facts. We shall cite only the following very striking case:

“At the latter end of October, 1848, a boy, 14 years of age, went to reside with a family named Mitchell, in Adden Place, St. Pancras. The Mitchells were at that time in health. The boy left his own home because his brothers were ‘down with the fever.’ This lad was, early in November, admitted into the hospital, suffering from typhus fever. Early, also, in the same month, the man Mitchell, aged 29 years, with whom the boy lodged, the man’s daughter, aged 7 years, and a female lodger, aged 22, were also admitted with typhus fever. The other members of Mitchell’s family, expelled from Adden Place, then removed to 21, Hertford Street, at least a mile from their former residence. At this time, so far as I could learn from personal inquiry, there was no fever in Hertford Street, and *certainly* none in the house in which they had taken up their residence. On November 22d, the two sisters of Mitchell’s wife, aged respectively 14 and 22, who had removed from Adden Place with Mrs. Mitchell and her infant, aged 4 years, were received into the hospital, both suffering from typhus fever. On December 8th, the landlady of 21, Hertford Street, was admitted with very severe typhus fever; and on December 20th, the son-in-law of the landlady was also admitted with the same disease. I subsequently saw Mitchell’s infant, aged 4 years, at its own home; it was similarly, but very slightly, affected. The only member of the family who escaped was the woman Mitchell, and she had had ‘spotted typhus fever,’ according to her own voluntary statement, some few years before. Here was a group of persons, whose ages varied from four to sixty years, and whose constitutional predispositions must also have varied infinitely, for there were several of them unconnected by blood, exposed to the poison of typhus fever (introduced among them by the lad aged 14), at a time when typhus fever was only twice as prevalent as typhoid fever. What was the result? Did one third of the number have typhoid fever? No; not one.” (pp. 37-8.)

The facts thus brought forward by Dr. Jenner appear to be of the highest

importance: the method he has adopted is the only correct mode of examining into this part of the inquiry into the identity or otherwise of these fevers. As we trust soon to enter fully into the subject of the fevers of cold countries, we shall not at present do more than express our great satisfaction with the philosophical and logical manner in which Dr. Jenner has treated the point in question in this communication. We must not omit to mention, that plates are given of the eruption, which are tolerably characteristic, although we think, in his anxiety to avoid exaggeration, the author has hardly made the colouring of the mulberry rash sufficiently deep. It would also have been perfectly easy to have found a case of typhus, with a much more abundant crop of spots than the patient presented, from whom Plate III was drawn.

*IV. A Case of Complete Intestinal Obstruction arising from Disease of the Sigmoid Flexure of the Colon and the Rectum, in which the descending Colon was successfully opened in the Loin.* By Frederick Field, M.R.C.S., of Birmingham.

*V. A similar Case.* By Josiah Clarkson, M.R.C.S., of Birmingham.

Although these cases form the substance of different papers, they are so similar as to admit of consideration together. The first is that of Joseph Ridding, a robust, hard-working man, a coach-axle forger by trade, and of not very temperate habits. After suffering for several months from irregularity of the bowels, his symptoms became greatly aggravated, and for nine days before applying for medical assistance he had scarcely any evacuation, and none unaccompanied with pain and straining. Mr. Field thus reports his condition when first seen:

"The abdomen was now greatly distended and tympanitic; pain was chiefly felt in the situation of the transverse colon, and was much increased by pressure, and at this part some bulging was also observed; he described the pain as of a "twisting" character, and shooting down to the umbilicus. At short intervals the pain was aggravated in violent paroxysms, accompanied with strong tenesmus, which continued about a minute, and then as quickly subsided. He vomited almost everything which he took. The pulse was rather increased in frequency, full, strong, and compressible. Tongue coated with a thick, pale fur, and not dry; much thirst." (p. 44.)

It is unnecessary to follow the author through the detail of the treatment, which was of the ordinary description; suffice it to say, that there was no lack of ingenuity in varying the remedies that were ordered. The colon-tube, passed up the rectum on several occasions, always met with an obstruction about eight inches up; and galvanism, which was tried, by introducing one wire into the rectum, and applying the other to the abdomen and spine, only excited painful contractions of the abdominal muscles without relieving the bowels. For sixteen days he bore up remarkably well against his complaint; but then began to sink so rapidly, as to leave no doubt that he would die unless some relief was obtained for him. In this unpromising condition it was determined to open the descending colon in the loin, as recommended by Amussat. We insert the account of the operation, which is just similar to that which was had recourse to in the next case, and with the like result—complete relief to the abdominal symptoms:

"The same day, at 4 p.m., the operation was performed, in the presence of Dr. Fletcher, Mr. Crompton, Mr. Alfred Baker, and Mr. Clarkson. As the patient lay



on his back, no indication of the precise nature or situation of the obstruction was observable. The abdomen was equally swollen on both sides, presenting everywhere the same elastic resistance to pressure; the sound on percussion being generally clear, was duller as it approached the loins. As the patient lay on his belly no bulging was observable in the lumbar region of one side more than in that of the other; percussion elicited a rather duller sound on the left than on the right side. The patient being extended on a bed with his face downwards, a transverse incision was made on the left loin, beginning at the ridge which marks the external margin of the erector spinæ muscle (about two inches from the spine), and carried directly outwards. This incision was five and a half inches long, and was situated a finger's breadth above the crest of the ilium, it passed through the skin and fat nearly one inch in depth, down to the latissimus dorsi muscle. This muscle, and the quadratus lumborum were now divided to the extent of the incision of the skin, and a layer of fat bounded on the inside by the external margin of the erector spinæ muscle was brought into view. On dissecting this away to the depth of about half an inch, a thin transparent membrane was exposed. From the appearance of this membrane, which it was conceived might be the intestine, it was thought advisable to pass sutures through it, to retain it in its position, and subsequently to affix it to the edges of the wound. However, on penetrating it with the knife, a mass of soft granular fat started through the incision. A very large quantity of this fat was cautiously dissected away, and the finger was then introduced to search for the bowel, but no precise indication of it could be felt; the finger, when pressed upwards, rested on the lower part of the left kidney, while downwards it came in contact with the inner margin of the crest of the ilium. The wound being now of considerable depth, it was necessary to proceed with great caution, and clear away the fat little by little, which proceeding, from the looseness of the nature of the fat, was rather difficult, and occupied some time.

“At length the bowel was brought into view, at the depth of about four inches; it was highly vascular, and having been cleared of fat, sutures were passed through it and held by assistants. An incision half an inch in length was made into the bowel, and an immense quantity of light-coloured fluid fæces immediately escaped. The patient had been vomiting similar fæcal matter during nearly the whole of the operation, but this vomiting now entirely ceased, and he was relieved of all his symptoms. The opening in the bowel was fastened by sutures to the skin; a large bread poultice was placed over the wound, and retained by a bandage passed round the body, the patient being enjoined to lie on the left side to facilitate the escape of the fæces. Scarcely two ounces of blood were lost during the operation.” (pp. 48-9.)

The colon-tube passed downwards through the wound, or upwards though the rectum, always met with an obstruction after it had passed either way about eight inches, and consequently about the situation of the sigmoid flexure. This obstruction could never be overcome, so that all the fæces escaped through the wound, but without causing so much inconvenience as might have been anticipated from the existence of an artificial anus, and without the least interference with the patient's usual health, for he became in a short time as stout and as strong as ever. Throughout there was some disposition in the wound to contract and close entirely, but after a time this inconvenience ceased. Afterwards his intemperance induced hepatic disease, and eventually he died, worn out and exhausted, a year and nine months after the operation. After death there was discovered to be acute peritonitis, but no intimation is given as to where and in what manner this had commenced:

“The strictured portion of the intestine was situated at the sigmoid flexure; it was about four inches in length and three fourths of an inch in diameter, through-

out the whole extent of the contracted part. When cut into, the whole of this portion was found to be filled with a plug of apparently firmly coagulated lymph, which entirely obliterated the canal." (p. 56.)

On maceration, this plug came away from the bowel, and displayed its coats but slightly thickened and diseased. The plug itself seemed to be formed by successive layers of lymph or fibrin, or "*inspissated cancers*," a disease with which our readers will now become acquainted for the first time.

The next case, related by Mr. Clarkson, occurred in a young and healthy woman, but is so similar in character to the last one, that we need not here enter into its minute history. The colon was opened in the same manner as practised by Mr. Field; and the result was so far satisfactory, that it prolonged the patient's life for fourteen months, for some part of which period she continued in very tolerable health. The chief difficulty in the after management of the case was the tendency of the artificial anus to contract, and the sluggish state of the whole alimentary canal. Sponge tents, and division of the cicatrix by the scalpel, remedied the one, but no efforts could overcome the other, and eventually the patient sunk in a very emaciated condition. At the *post-mortem* examination the peritoneal cavity was found studded with tubercles, and the intestines glued together by long-continued inflammatory action, which doubtless occasioned the obstinate constipation. We subjoin the account of the condition of the parts concerned in the operation:

"The obstruction was situated about six inches from the lower termination of the rectum, and on a level with the fundus of the uterus. It consisted of a dense cartilaginous substance, surrounding the intestine in this spot, and completely obliterating its canal. It appeared to have originated externally to, or on the outer surface of, the bowel, as this latter, at the point of obstruction, was pinched in, as though it had originally been tied with a ligature. This diseased structure was about the size of a pullet's-egg, the greater part of it being situated anteriorly, whereby it had pushed the fundus of the uterus forward, producing slight anti-version; it was also firmly connected with this organ. On laying open the bowel, and dividing the growth in a line corresponding with it, it was found that the channel was obliterated for the distance of half an inch, and this occlusion was so complete, as even now to prevent the possibility of establishing a communication between the upper and lower portions of the intestine, except by the aid of the knife.

"The appearance of the opening into the bowel from the loin was very satisfactory. Situated about six inches above the obstruction, it was sufficiently large to admit, with ease, the finger; the edges were rounded and smooth, and the mucous membrane, both above and below, was in a perfectly healthy state. The lungs were free from tubercles." (pp. 65-6.)

We regard these cases as exceedingly creditable to Mr. Field and Mr. Clarkson, who seem well qualified to support the reputation of the Birmingham School of Surgery.

VI. *Chemical Researches on the Nature and Cause of Cholera.* By Robert Dundas Thomson, M.D. Lecturer on Chemistry in the University of Glasgow.

These researches were made on the blood and discharges of cholera patients in Glasgow.

1. *Stage of Collapse.* The following are the observations on the *blood*.

The specific gravity of the whole blood was in three cases 1074, 1068, 1065; the specific gravity of the serum in two cases was 1058 and 1042. In eight analyses of blood drawn during life, the mean of the water of the blood was 724 parts, the solid being 276; in three cases of blood drawn after death, the average amount of the water was 694 parts per 1000. The fibrin in two cases was increased, being 4.54 parts per 1000. The globules and albumen (and we should remark also the extractives, which our author never once alludes to) in the same two cases, averaged together 270.96 per 1000. It is considered that the several organic solids of the blood preserve among themselves the same quantitative relations, although how the author can draw such a conclusion, when he has never purified his albumen, that is, separated it from the extractives, we are at a loss to determine. With regard to the salts of the blood, the mean of seven cases was 8.56 parts per 1000; in three other cases, in which the soluble and insoluble salts were separated, the mean of the soluble was 7.18, and that of the insoluble 1.87 per 1000. It thus appears that (as was already well known) the soluble salts are relatively diminished when compared with the other solids, although, from the immense loss of water, they are, when compared only with the water, actually increased. The insoluble salts follow the same rule as the organic constituents, and are with these proportionably increased. The chlorides of sodium and potassium appear to be more diminished than the other soluble salts, but the author remarks that these results are "approximative, only being drawn from limited sources."

The analyses of the *intestinal discharges* are distributed under two heads.

Under the first head three analyses are given; the mean quantity of organic matter was 6.75, that of the salts 6.75, and that of the water 987.6 per 1000. The organic matter was found to consist of a certain amount of albumen, and of other matters not specified.

Under the second head three analyses are given, in which there was a greater quantity of organic matter, amounting in one case to 59 parts per 1000. In this case the quantity of albumen was so great as to cause the liquid to become solid by heat.

Dr. Thomson remarks, "it does not appear that the true nature of the *fæces* in cholera has yet been deduced from accurate chemical experiments," a rather unexpected observation, considering the analyses that were made during the epidemic of 1832, and those which have been published lately by Parkes and Becquerel. Dr. Thomson also states that careful examination under the microscope has demonstrated the flocculi of the stools to be chiefly epithelial scales, although this too is in direct contradiction to the observations of Gairdner and Parkes.

2. *The stage of reaction.* In the *biliary* stage (the stage of reaction), some analyses of the *blood* are also given; the specific gravity of the serum in five cases averaged only 1027.7; the solids in the blood on a mean of three cases were 203 parts per 1000; the fibrin (mean of two cases) was 2.76; the globules, albumen, and extractives (mean of three cases) 196.10, and amount of the salts (mean of three cases) was 7.44 per 1000.

The *evacuations* of the biliary stage were examined in three cases; the mean of the solids was 89 parts per 1000; the salts in one case amounted

to 10 parts, but it is not stated what proportion of these were soluble salts.

The *urine* in the biliary stage was vaguely observed in five cases; the specific gravity varied from 1004 to 1018. In one case, 5 parts per 1000 of albumen were found. No accurate analysis of the urine appears to have been instituted.

3. *The atmosphere.* The *atmosphere* of Glasgow during the prevalence of cholera was very carefully examined, although some exception might be taken to the manner in which it was done. In the first set of experiments, air was passed through a bottle of acidulated water; it was expected that by this means any solid matter existent in the atmosphere would be arrested; the water was then evaporated, but appeared to have gained nothing but a little ammonia, the quantity of which was estimated as 0.319 grains of caustic ammonia in 1000 lbs. of air, or .45 parts in 10,000,000; being less than half the amount determined by Fresenius. By another set of experiments, it was decided that no carbon or hydrogen existed in the atmosphere, otherwise than in the form of carbonic acid and water. The result of these experiments was therefore quite negative.

VII. *Case of Stricture of the Œsophagus, fatal, two years and three months after accidentally swallowing Soap-Lees.* By William Basham, M.D., Physician to the Westminster Hospital.

The nature of this communication is sufficiently evident from its title. Anne S—, a healthy young woman, æt. 21, accidentally swallowed some soap-lees, which had been employed in cleaning marble, and five days afterwards was admitted into the Westminster Hospital as a patient of Dr. Basham. She had not swallowed more than a mouthful, and did not experience any ill effects until two hours had elapsed, when severe vomiting set in. This continued unchecked until she came to the hospital, and it was then accompanied by severe pain in the epigastrium and between the shoulders. The mucous membrane of the mouth and fauces was red and deprived of its epithelium, but not ulcerated. The Œsophagus-tube was passed into the stomach without more inconvenience than is usually experienced, and in the course of ten days the patient left the institution apparently well. Eleven months had elapsed when she was readmitted, suffering under urgent dysphagia, and emaciated to an extreme degree. For many weeks she had not swallowed any solid food, and for the preceding eleven days had regurgitated even the mildest fluids.

A No. 8 gum-elastic catheter, passed into the Œsophagus, encountered an obstruction an inch or two below the cricoid cartilage, which it required some force and perseverance to overcome. There was also a slight impediment higher up, which was passed without great difficulty. By constant care and attention, a larger tube was made to pass through the stricture; but such was its disposition to contract, that on three different occasions; the patient was admitted into the Hospital with great aggravation of her sufferings. On the 10th of September, 1849, two years and six weeks after the accidental swallowing of the soap-lees, she again applied for relief, with complete obstruction of the Œsophagus, and in a pitiable condition of starvation and weakness. No instrument could be passed into the stomach, and all means of nourishing the patient failed, so that on the 19th of the same month she expired, literally starved to death.

At the post-mortem examination all the internal organs were found healthy, but the œsophagus was thickened through its entire length, and strictured in two places where the obstructions had been discovered during life. Just below the first stricture was the cicatrix of an ulcer about the size of a sixpence, beyond which the tube was dilated, and then contracted again so completely as to be impervious to a probe passed from above downwards.

The author refers to two similar cases, related by Sir Charles Bell, in one of which the patient survived twenty years; and also to a few other examples, in which also the patients died from inanition consequent on the obstruction. He concludes that, after the first inflammatory symptoms have subsided, the bougie should be daily used for many months together, even though there be no stricture, but merely as a matter of precaution.

The paper is clearly but inelegantly written, and contains no less than four errors. In his opening sentence the author says, that "a similar succession of symptoms and consequences *have* been observed," and in three different places the term cricoid cartilage is printed "circoid cartilage."

VIII. *On the proximate cause of Albuminous Urine and Dropsy; and on the Pathology of the Renal Blood-vessels in Bright's Disease.* By George Johnson, M.D.

After some preliminary remarks on the changes in the renal secreting cells, the author passes on to state an hypothesis of the proximate cause of albuminous urine. This hypothesis is based on the experiments of Dr. John Reid on asphyxia, and on the explanation of these, by Dr. Alison's well-known doctrine, that the normal chemico-vital actions of a part are essential to its proper circulation. If these necessary actions are impeded, the passage of the blood is also proportionably arrested. In the case of the kidney, the circulation of certain foreign substances with the blood is supposed to cause an attempt at elimination by the secreting cells of the kidney; the cells are formed and shed rapidly, and it is stated that their formation in large numbers tends to block up some of the tubes completely, and thus, by impeding the secretory process, to hinder the circulation in the vessels of the inter-tubular plexus, and in the Malpighian capillaries. This hypothesis is supported by some facts observed by the author respecting the renal vessels in Bright's disease. He has found the coats of the *arteries* thickened to as much as three or four times their normal amount, although the channel of the artery is undiminished until the structure of the kidney is almost entirely destroyed; the *Malpighian capillaries* also are thickened, although until the last stages they also remain pervious; the inter-tubular vessels are not thickened, but appear less numerous than in health. The author explains these changes, and again states his hypothesis, in the following words:

"There is an imperfect elimination of the urinary constituents, in consequence of changes in the secreting cells, produced by an effort which they have made, to excrete abnormal products. Deficient excretion at once leads to impeded circulation,—the obstruction occurring at the very point where the excretion should be effected, viz., in the inter-tubular capillaries. The impediment reacts backwards upon the Malpighian capillaries, which in a sudden acute attack become ruptured, and allow the blood to escape into the urinary tubes, but when the disease has been of longer



duration, they become thickened, and permit only the serum of the blood to escape." (pp. 116-17.)

The Malpighian capillaries are supposed to be thickened in order to enable them to bear the increased pressure; while the thickening of the arteries "seems to have for its object to assist in driving the blood onwards through the inter-tubular vessels in which the impediment exists." These explanations apply to the *final* not to the *physical* causes of the changes in question; and the author has not shown that the thickening is due to an hypertrophy of the proper contractile elements in the arterial coats, instead of to simple exudation and subsequent fibrillation.

The dropsy in Bright's disease is referred also to obstruction in capillaries from diseased blood, only the capillaries affected are in this case the systemic and not the renal. "It follows," says Dr. Johnson, "that an albuminous condition of the urine, and dropsical effusion into the areolar tissue and serous cavities must result from precisely analogous conditions, viz., an arrest of poisoned blood in capillary vessels." (p. 119.) Dr. Johnson very ingeniously likens this obstruction to that which Dr. Reid found by the hæmadynamometer to exist in the capillary circulation, in the early stage of asphyxia, when the supply of blood to the systemic arteries was not yet diminished, but that blood was imperfectly aerated. Comparing renal dropsies with those arising from disease of the heart, lungs, liver, &c., he shows that they all thus correspond in one important particular; an impeded circulation preceding and accompanying the dropsical effusion.—We consider this paper a very ingenious one; the doctrine on which the author's hypothesis is based, we regard as a most important physiological truth; and we quite accord with the author in the belief, that "if the facts are true, and the inferences just and rational, they must be applicable in a greater or less degree to diseases of other parts."

#### IX. *On Fatty Diseases of the Heart.* By Richard Quain, M.D.

The author distinguishes two forms of fatty diseases of the heart, viz., one in which there is an hypertrophy of the normally existent fat which encroaches upon the walls of the heart, and sometimes causes, or is coincident with, atrophy of the muscular fibres, and fatty degeneration of the muscular fibre itself. In the first form, which is termed "fatty growth on the heart," the muscular fibre may remain intact; in the second form, "fatty degeneration," the sarcous elements disappear, and globules of fat are found within the sarcolemma.

1. *In fatty growth*, the muscular fibres, although they preserve their structure, are, in all advanced cases, more or less distorted and convoluted, by the pressure of the masses of fat. The growth of fat commences first in the groove between the auricles and ventricles, and follows subsequently more or less the course of the vessels.

2. *The fatty degeneration* is most common in the left ventricle, then in the right; then in the right auricle, and is least frequent in the left auricle. When present to any amount, it produces changes in the physical appearance of the organ; the colour is pale, yellowish-brown, or buff, or muddy pink. This buff appearance is seen generally in points or patches, so that the heart has a mottled appearance. The consistence of the heart is also lessened, it becomes flabby, and feels sometimes like a piece of wet chamois

leather or a wet glove. Under the microscope the cross markings of the fibre are found to have more or less disappeared, and dark highly refractive points of fatty matter are crowded within the sarcolemma, or are distributed linearly along it. If this change occurs in consequence of a local affection of a branch of the coronary artery, it is often extreme; if in consequence of some general systemic condition, it is often less in amount, but more equally diffused. In this last case, the whole organ is flabby and soft, but does not appear fatty to the naked eye. The microscope, however, reveals the real state of things.

After this sketch, the author proceeds to allude to previous observations on the same subject; we shall not follow him through this part, but merely remark that he will find the various forms of fatty heart, more extensively known on the continent, in Germany especially, than he has indicated.

The *causes* of "fatty growth" are those which favour obesity generally; advanced age is favorable to it, and according to Dr. Quain, it is four times as common in males as in females.

The causes of "fatty degeneration" are somewhat different. In considering them, the author first advances arguments to show that the fatty matter in the fibre is the product of changes in the fibre itself, and is not a deposit from the blood.

This change in the fibre ensues under two series of conditions: 1. From general or constitutional causes: among 25 cases reported by Dr. Ormerod, 3 had suffered from hæmorrhage, 3 from phthisis, 1 from delirium tremens, 1 from cancer, and 2 from fever. 2. From local modification of nutrition: in 13 of 33 cases of degeneration reported by Dr. Quain, the coronary arteries were more or less ossified or obstructed. In other cases the local modification is a sequence of endo- or peri-carditis.

Fatty degeneration is more common in males than females (39 to 16); and in old than young persons, nearly one half of the cases being in persons over 60, and only one case in the series being in a person under 20.

The *effects* of these fatty diseases are next considered.

Those of "fatty growth" are thus enumerated; languid and feeble circulation, a sense of uneasiness and oppression in the chest, distress in breathing, syncope, angina, coma, sudden death, rupture of the heart. Among 15 cases, 14 died suddenly.

In fatty degeneration, the effects are comprised under the heads: (a) Impairment of structure. (b) Impairment of function. Under the former head, the author places the frequent increased size of the organ from hypertrophy, and suggests the possibility of the hypertrophy resulting from a natural effort to compensate for loss of power. But in opposition to this it may be doubted whether the sequence is not here reversed, and whether the hypertrophy is not often prior to the degeneration. The texture of the heart, as already said, is softened, and hence frequently ensues the most formidable effect of fatty degeneration, viz., *rupture*. Rupture occurred in 25 of 68 cases, and was complete, that is permitted bleeding into the pericardium, in all but six. From partial rupture also results apoplexy of the heart. Aneurism of the heart also appears to be connected with this condition.

Under the head of impairment of function, the author enumerates the following effects of fatty degeneration: vertigo, syncope, and coma; dyspnœa, either permanent or produced by ascending heights, occurred in

half the cases ; pain in the region of the heart was present in 26 out of 83 ; in some cases the attacks of pain came on in the manner described by systematic writers under the term *angina pectoris*, and the author conjectures with much plausibility, that in many cases termed *angina*, the real disease has been fatty degeneration.

The *diagnosis* of fatty degeneration is to be drawn from these symptoms ; the pulse also is weak, often irregular, frequently also it is slow. The *physical* signs are few, the impulse is feeble, the first sound feeble ; the cardiac dullness, if the heart be enlarged, extensive,—there are not necessarily any murmurs. The great practical importance of a knowledge of this condition is shown by the fact, that of 83 cases of fatty degeneration, fatty growth, or of “pale flabby heart” (probably from fatty degeneration), no less than 68 died suddenly.

Dr. Quain is entitled to great praise for the assiduity and success with which he has cultivated this subject. His paper is one of the most valuable contributions to Pathology which has recently fallen under our notice. We trust he will pursue his researches on fatty degeneration of other tissues ; he will find ample matter for investigation.

X. *A Case of Gunshot Wound, and subsequent Extraction of a Bullet from the Bladder.* By E. M. Macpherson, Assistant Surgeon to the Ninth Royal Lancers. Communicated, with notices of similar cases, by James Dixon, Assistant Surgeon to St. Thomas’s Hospital.

At the battle of Chillianwallah, a soldier received a bullet in the situation of the left ischiatic notch, which could not be found at the time, but occasioned no impediment to the healing of the wound. Soon after the wound had been received, the man began to pass ropy mucus from the bladder, with great accompanying pain, and other symptoms of a foreign body in the bladder. The sound detecting a hard substance in that viscus, it was successfully extracted by the lateral operation, and proved to be an iron ball weighing one ounce and thirty-eight grains, covered with a thin sandy deposit. Mr. Dixon adds to this paper a table of eighteen other cases, in which bullets made their way into the bladder, the details of which, scattered through different works, are thus rendered easy of reference.

XI. *An Account of a Case of Scrofulous Abscess of the Anterior Mediastinum, communicating with both sides of the chest, the pericardium, and trachea ; forming a tumour above the clavicle, and simulating aneurism of the Innominata Artery or arch of the Aorta. With Observations.* By Daniel MacLachlan, M.D., Physician to the Royal Hospital, Chelsea.

This is one of the most interesting papers in the volume, and is a good example of inductive medical reasoning. It is impossible, however, within a short compass, to give an intelligible account of the case which it relates ; suffice it here to state, that the difficulty in the diagnosis was considerable, on account of the tumour simulating an intra-thoracic aneurism.

XII. *Case of Mollities and Fragilitas Ossium, accompanied with urine strongly charged with animal matter.* By William Macintyre, M.D., Physician to the Western General Dispensary.

A tradesman, aged 45, came under Dr. Macintyre’s care on the 30th of October, 1845. He had suffered for upwards of twelve months from

excruciating pains in the chest, loins, and back. When first seen he was much emaciated; the pain had become fixed in the left lumbar and iliac regions, and the patient, in consequence of the great agony produced by moving the body on the thighs, was constantly in a semiflexed position. There was exquisite tenderness on pressure at the brim of the pelvis in front. Nothing very satisfactory could be made out regarding the abdominal and thoracic organs, except that the cardiac impulse was too strong, and the precordial dullness too great, without cardiac murmur. The patient attributed his illness to a strain, but it appears that for some time previously he had been subject to wandering pains about the breast and shoulders. When he came under Dr. Macintyre's notice, a singular and unique reaction was discovered on examining the urine. The specimen of urine examined was of high specific gravity (1035), acid, and opaque; when boiled, a coagulum of animal matter occurred; that this differed, however, in some way from ordinary albumen, was shown by the remarkable action of nitric acid; when the urine was treated with this agent, the previous opacity disappeared, and the urine became quite clear; in about an hour and a half, however, if the urine was cold, the whole portion formed into a firm yellow mass, which at once liquefied on the application of heat, and again solidified on cooling. Dr. Bence Jones has already given an account of this substance in the 'Transactions of the Royal Society for 1848,' and has determined it to be a deutoxide of albumen.

The severe pain in the lumbar region continued, and the urine remained still loaded to excess with this peculiar animal matter till the patient's death, which took place without any satisfactory opinion having been arrived at, as to the nature of the case.

On a *post-mortem* examination, the sternum, ribs, and cervical dorsal and lumbar vertebræ were found soft and brittle; the cancellated structure loaded with a blood-red, soft, gelatiniform substance, which, under the microscope, was found to be composed of granular matter, oil globules, nucleated cells, with a few caudate cells and blood disks. The thoracic and abdominal organs presented nothing unhealthy.

The author makes some very interesting remarks on the diagnosis, origin, and progress of this remarkable case.

XIII. *Case of a very large Hæmatocele of the Spermatic Cord, proving fatal after ten years.* By William Bowman, F.R.S., Professor of Physiology, &c., and Assistant-Surgeon to King's College Hospital, &c. &c. To which is added, *A Case of very large Hæmatocele of the Tunica Vaginalis, in an Old Man, terminating fatally.* By Thomas Blizard Curling, F.R.S.

Hæmatocele of the spermatic cord is so rare a disease as not to be alluded to by Sir A. Cooper, in his work on the Testis, though, from a case he records in page 210, it is probable he had met with an example of it, which was mistaken for hæmatocele of the tunica vaginalis. Mr. Pott relates three cases of the disease, and Mr. Curling notices two others. With these exceptions, surgical works are generally silent on the subject. The case related in this paper by Mr. Bowman is probably the most complete, as it is the severest on record.

A stout, healthy farmer, ten years before his death, was thrown from his horse against some railings, and directly afterwards perceived a swell-

ing in the inguinal region. This was confined to the inguinal canal, and at first sight resembled a hernia, though it had no impulse on coughing, and was not accompanied with symptoms of strangulation. Uninfluenced by treatment, the swelling remained unaltered, of about the size of a hen's egg, oval, and of a solid but elastic feel, and did not occasion the patient any particular uneasiness. Seven years afterwards, during exertion in walking, the tumour became suddenly larger and heavier; and, on examination, this was found to depend on a vast effusion of blood into the inguinal canal and scrotum. After some time, when some ecchymosis which was present had disappeared, Mr. Paget, of Leicester, punctured the swelling, and gave exit to a free gush of mixed arterial and venous blood. This circumstance was alarming at the time, from the fear that the swelling might be a vascular tumour, of malignant nature; but the opening made by the trocar healed kindly, and without bad result. It continued to increase in size, and was again punctured by another surgeon, and with the same result. So it remained, getting gradually larger, we take it, for a space of three years,—but the dates are not very minutely given,—until the 7th of December, 1848, when Mr. Bowman was called in.

“On going down to see the patient on the 7th of December, I was very much struck with the enormous dimensions and weight of the tumour, far exceeding anything I had before seen. The patient had long been confined to his bed, from sheer inability to drag so great a substance about with him, and he was now also much reduced in strength, and had a worn and haggard countenance. He had been lying habitually on his back, with the tumour resting on the right thigh, and it had become moulded by constant pressure to the shape of that part. It reached to the patella, had an oval shape, and was so heavy, that it required two hands and no slight effort to raise it from its bed. Its neck ran up into the inguinal canal, but there was no appearance of any glandular disease either there or in the abdomen. Its surface was crossed by many very large veins, several as large as writing quills, which occupied deep grooves that might be felt in the hardened tissue of the dartos. The majority of these vessels took a nearly transverse course over the tumour, about an inch apart, and in somewhat regular parallel lines. The skin was dark coloured and congested, and presented the cicatrices of the two punctures which had been made into it. At the seat of the more recent one, and also in another spot, there might be felt through the skin an opening or deficiency in the wall of the dense and apparently membranous investment of the tumour.

“These seeming openings were circular, larger than a crown piece, and felt like holes punched out of a piece of parchment, so sharply defined was their margin under the integument, and so soft and elastic the substance of the tumour within their limits. In most other parts, the feeling was that of a dense, solid mass, but in some there was a doubtful sense of fluctuation. One thing, however, was quite clear; the right testicle, with the epididymis, was at the very lowest extremity of the tumour, most distant from the abdominal ring, and rested on the knee-joint. It appeared large from congestion, but slipped about naturally, and formed no part of the diseased mass, and I have no doubt the tunica vaginalis was free from adhesions. Percussion gave a tympanitic sound over the more elevated portions of the tumour, and on shaking it, a proceeding rendered difficult by its great size and weight, the sound of air and fluid mingling was very audible.

“This last sign, with the low irritative fever under which the system was labouring, rendered it probable, that the contents since the last puncture had fallen into a state of decomposition; and therefore, notwithstanding some doubt still hanging over the case, it was deemed desirable to lay open the integuments to a limited extent, in order to afford vent to the gas and other putrid matters. For this par-



pose, a spot was chosen where the covering was thinnest, and a trocar being thrust in between some of the large venous channels, there was at once an escape of much fetid gas and of some dark brown blood of the consistence of treacle, evidently putrid. As no hæmorrhage occurred to interrupt this proceeding, the opening was now enlarged with a bistoury to an extent of three inches, with the effect of allowing issue to a further great quantity of the same kind of blood, mixed with large masses of old coagulum, not unlike that met with in aneurismal sacs, altogether nearly filling two large wash-hand basins.

"It now became evident, that the tumour was an immense sac, situated in the course of the spermatic cord, and filled with blood, and that it had arrived at that stage when the only hope for the patient was in the complete evacuation of its contents. As for a summary removal of the entire tumour, the proposal could not be entertained. The reduced and almost typhoid condition of the patient, the extent of the scrotal covering, and especially the extraordinary size and number of the veins imbedded in the wall of the sac, and the firm and universal adhesion of the sac to the skin, quite precluded it. Had it been attempted, I am convinced the issue would have been immediately fatal. We therefore contented ourselves with making a counter-opening at the lower part, for the further drain of the contents.

"After all this had been done, so firm and solid was the wall of the cavity, that it collapsed but little, and large masses of coagula of various degrees of solidity remained within it, adhering to its inner surface. A poultice was then applied. I need hardly add, that the testis was left at the lower end of the tumour, lying unmolested in its natural coverings.

"Four days afterwards, Mr. Paget writes: 'I saw our patient with hæmatocele to-day. He seems much altered in appearance since Thursday, is reported to have had much pain and faintness for some hours after we left, followed by vomiting, or rather eructation of fluid, so that scarcely any nourishment has been retained. There has also been profuse sweating; breathing hurried; tongue loaded, dark, and dry. I removed more putrid coagula and sanious fluid, and flatus escaped the while.'" As was to be expected, he did not survive more than another day." (pp. 236-9.)

No inspection was made of the body after death; but the exact nature of the disease is sufficiently clear without it. Mr. White, in his book called '*Cases in Surgery*,' relates two similar cases, in which the diseased masses were successfully extirpated.

Mr. Curling's case is that of a large hæmatocele of the tunica vaginalis, which, after remaining quiet for many years, eventually inflamed, was opened, gave exit to great quantities of coagula, and finally killed the patient.

Mr. Bowman draws the practical conclusion, that "hæmatocele has little tendency to undergo spontaneous cure, and that, sooner or later, it will probably enlarge and prove fatal. There should, therefore, be not unnecessary delay in carrying out the ordinary practice of laying open the cavity by a free incision, where the persistence of the swelling and other circumstances indicate the presence of extravasated blood." (p. 241.)

Mr. Curling's case adds another to the list of errors; for "penis" is printed "*pain*."

XIV. *Case of Disarticulation of the Left Condyle of the Lower Jaw, with excision of nearly the left half of the Bone, on account of a very large cartilaginous Tumour growing from, and occupying the site of all this part of the bone, save the condyle and neck.* By William Beaumont, Professor of Surgery in the University of Toronto, Canada.

The lower jaw has been so often removed, both in successive portions,

and in its entirety, that the *mere operation* is not calculated to excite any great amount of surprise or attention; and as the case related by Mr. Beaumont has nothing remarkable in it, we do not know that it presents any feature deserving of an extended notice from us. Indeed, we must regard the case itself as of very little value in a practical point of view, for the tumour, whose removal required this formidable operation, is stated to have been but three months in its growth; and a very little longer period had elapsed since its removal, when the account was published; so that it is impossible to say whether the disease is malignant and will return, or not; and that to us seems the most important question in the matter.

Even this paper, short as it is, is blemished by an error; at page 247, "cuspidating" is substituted for "cuspidatus."

**XV. *On Excision of the Os Calcis, in Incurable Disease of that Bone, as a substitute for Amputation of the Foot.* With a Case. By William Bousfield Page, Surgeon to the Cumberland Infirmary, Carlisle.**

This paper would more correctly be styled, "An Account of a Case of Excision of the Os Calcis," &c.; it not being, as the title leads one to expect, a dissertation or treatise on the operation. The case itself is a very creditable one to Mr. Page, and adds another to the list of successful examples of removal of some single bone, in strumous disease of the foot, as a substitute for the more terrible operation of amputation.—The eye of the critical reader will be arrested by the following passage at page 253. "No proper estimate, however, can be formed in this case of the value of the operation, as disease of a similar nature to that in the foot 'has since appeared in the hip-joint, and is also affected with phthisis.'" Phthisis coxæ is a new disease.

**XVI. *A Case of Stricture of the Rectum, wherein an Artificial Anus was successfully established in the left Lumbar Region.* By J. Wilson Croker Pennell, M.B. (Lond.), M.R.C.S., Licentiate of the Faculty of Medicine of Rio de Janeiro, formerly Lecturer on Anatomy and Physiology at the Westminster Hospital School of Medicine. Communicated by Benjamin Phillips, F.R.S.**

This is a case of obstinate stricture of the rectum, in which the sufferings of the patient for several years were of a most intolerable description, and were greatly increased by the existence of a false passage into the urethra and bladder, through which the intestinal contents almost entirely escaped. The colon was opened in the left loin, as in the preceding cases related in an earlier part of the Volume, and with great relief to the sufferer; but the ultimate result, of course, remains doubtful. The operation itself was not attended with difficulty, and will soon become an established one in surgery.

**XVII. *On the Use of the Speculum in the Diagnosis and Treatment of Uterine Diseases.* By Robert Lee, M.D. F.R.S., Fellow of the Royal College of Physicians, Physician to the British Lying-in Hospital, Physician Accoucheur to the St. Mary-le-bone Infirmary, Lecturer on Midwifery and**

the Diseases of Women and Children at St. George's Hospital. (With a Woodcut.)

We are sorry to state, that while we deplore the low state of morals of those practitioners who are daily degrading themselves and bringing discredit on our profession by the most disgusting abuse of the speculum, we do not think Dr. Lee's paper is likely to correct, though it is certain to irritate them. Some of the circumstances which he relates are startling enough; and one history, communicated to him by Dr. Copland, is so shocking as to be almost incredible; but there is in the whole paper an evidence of partizanship and special pleading, which will go far to neutralise the good it might otherwise have done. An essay of this kind should not be what Dr. Lee's paper has elsewhere been called, "A counterblast against the speculum," but a calm inquiry into its real value, founded not upon isolated cases and gossiping reports, but upon severely sifted facts and numerical details. The practitioner alluded to by the author may make erroneous diagnoses, or he may wilfully deceive his patients,—unfortunately in every department of the profession such conduct is too common;—but we cannot admit that this exposure has done anything towards settling the question, or that it altogether brings conviction home to our own minds. There are very different estimates among the best informed as to the value of the speculum; but we are inclined to think there are few who find it so little necessary as Dr. Lee; and it was evident enough during the discussion which followed the reading of the paper, that some of the first obstetricians in London have come to different conclusions as to the necessity of employing it in the diagnosis of uterine disease.

The paper commences by an historical sketch of the rise and progress of the speculum, from which it appears that a trivalve speculum, of which an engraving is given, was dug up among other surgical instruments in the ruins of Pompeii, and that it was sparingly employed at all times by the ancient surgeons. It is in France, however, that the abuse of the speculum originated; and the history of its employment is indeed a melancholy one. Whether the system has ever been carried to the same extent in this country, we are not aware; but we incline to think it has not, and that the cases related by Dr. Lee are at all events not very common ones.

XVIII. *Supplement to a Paper on Fibro-Calcareous Tumours and Polypi of the Uterus. Published in Volume XLX of the Medico-Chirurgical Transactions.* By Robert Lee, M.D. F.R.S., &c.

The objects of this paper appear to be threefold:—1st. To establish the vascularity of fibrous tumours of the uterus,—a fact that has been denied by some persons. Dr. Lee says:

"In the Museum of St. George's Hospital, there are various specimens of fibrous tumours of the uterus, in which both the arteries and veins have been injected. From these it is seen, that when the arteries reach the tumour, they do not at once plunge into its substance, but pass into the fissures or grooves on the outer surface of the membrane forming the sheath; and as the arteries run along these fissures between the lobes, small branches are given off to the surrounding parts. Veins of very considerable size are seen passing from the central parts of these tumours to their surface in a winding manner, and gradually enlarging till they terminate in the uterine veins. In these preparations, the veins of fibrous tumours have been filled with injection, thrown into branches ramifying throughout the substance of the tumours, and also from trunks of the uterine veins. I have repeatedly met

with coagula of blood, which extended from the uterine veins into the veins of fibrous tumours, by which the continuity of these vessels could be demonstrated, and the course of the circulation of the blood through such tumours clearly determined." (p. 280.)

2dly. To demonstrate that large fibrous tumours sometimes contain cavities filled with viscid fluid, and that they are, when in this condition, liable to be mistaken for, and treated as ovarian swellings. The author mentions two cases, in which such tumours were extirpated, in the belief that they were ovarian, and refers to others, in which they were tapped and the fluid drawn off.—And 3dly. To relate a case in which suppuration occurred in the substance of a fibrous tumour. A married lady, 40 years old, was directed by her medical attendant to introduce into the vagina, at bedtime, a medicated mass or ball; for what purpose does not seem very evident. After four of these balls had been used, the pain became so severe that their use was discontinued; and, on some surgical manipulation being resorted to by the same practitioner, inflammation of the uterus was set up, and proved fatal in a short time. We subjoin the account of the post-mortem examination.

"The uterus was about the size of a cricket-ball, and hard. The ovaria and fallopian tubes on both sides, and the uterus and rectum, adhered together by old false membranes. Having removed the uterus from the body, an incision was made through its anterior wall, when there flowed out a quantity of greenish-yellow fetid pus. This matter had escaped from an irregular cavity in the centre of a fibrous tumour, which still contained a portion of purulent fluid. The whole lining membrane of the uterus was red and inflamed, and near the cervix appeared softened and disorganised. The anterior lip of the os uteri was of a peculiarly livid colour. The sac of the peritoneum presented no trace of recent inflammation." (p. 282.)

Upon such a case as this comment is needless.

**XIX. Case of Fatal Poisoning by Sir William Burnett's Fluid.** By H. Letheby, M.B., Lecturer on Chemistry in the Medical School of the London Hospital.

Sir William Burnett's fluid, the chloride of zinc, has only lately come into use as a common deodorising agent. It is a highly poisonous salt, and serious accidents from its use may be occasionally anticipated; this renders the first fatal case of poisoning by it, which is recorded by Dr. Letheby, very interesting.

A child aged 15 months appears to have got hold of a bottle containing the fluid, (which had been obtained for the purpose of deodorizing the house, in which fever prevailed,) and to have swallowed some of its contents. It was immediately seized with violent sickness, and when seen by a surgeon ninety minutes afterwards, was in a state of collapse, with a pale anxious countenance, a quick fluttering pulse, a cold and clammy surface, and a rapid respiration. The lips were swollen, and their inner surface and the lining membrane of the mouth presented an opaque white appearance, as if they had been acted upon by some caustic substance. On attempting to drink cold water, the water returned through the nostrils. Death occurred in ten hours. On post-mortem examination, the lining membrane of the lips, mouth, fauces, and œsophagus were of an opaque white colour, the stomach felt hard and leathery, and contained one ounce and a half of a fluid that resembled curds and whey; the mucous

membrane was corrugated, opaque, and of a dull leaden hue; this appearance ceased at the pylorus, and the intestinal mucous membrane was healthy. On examination Dr. Letheby detected the presence of chloride of zinc, both in the fluid taken from the stomach, and in the tissue of the organ.

Dr. Letheby was led from this case to make some interesting experiments upon animals with this fluid, and from these he draws the following conclusions: The first effect of the chloride on the stomach is that of an irritant and caustic; it then passes in greater or less quantity into the circulation, and produces acceleration of the pulse and respiration, feebleness, and ultimately paralysis of the hind legs, and remarkable dilatation of the pupil; the paralysis increases, the surface gets cold, the pulse imperceptible, and the animal dies comatose. Dr. Letheby has detected the metal in the blood of the heart, in the tissues of the body, and in the urine.

**XX. *A Case in which the Urachus remained open, and a ring-shaped Calculus, formed upon a hair in the Bladder, was extracted through the Umbilicus.*** By Thomas Paget, F.R.C.S., Surgeon to the Leicester Infirmary. Communicated by William Bowman, F.R.S.

A man, 40 years of age, suffered for about a year from symptoms of stone; and, on being sounded by Mr. Paget, a calculus was detected in the bladder. The man presented also the congenital malformation of an opening at the umbilicus, through which a small quantity of water constantly escaped; and, as the opening was large enough to admit a finger, the surgeon determined to try whether it would not escape by plugging the orifice, and then distending the bladder with warm water, when it was anticipated that, on withdrawing the plug, the gush of water would carry out the calculus. However, Mr. Paget first of all carried his finger through the umbilical opening, and was lucky enough to entangle the stone and withdraw it in this manner. It proved to be a ring-shaped calculus formed round a hair, which protruded at one end, and is conjectured to be one of the pubic hairs which had found its way into the umbilical opening. The following is the description of the malformation, which is of a very interesting kind:—

“There is a circular deficiency in the linea alba an inch in diameter, its margin being thickened and of cartilaginous hardness. Through this protrudes a hernia of the size of a goose's egg, which, in lieu of ordinary integument, is covered by mucous membrane, the surface, however, becoming dry when exposed for any length of time, as that of a vagina when inverted by complete prolapse of the uterus.

“He never makes his water while the hernia is out; for when called to an effort for that purpose, the first act of the bladder is gradually to draw into the abdomen the whole of the protruded substance; its first contractions have no other effect, and it seems not to have power to force the urethra until that is accomplished. At the latter part of this act, at the instant of the disappearance of the hernia, there occurs a rather forcible jet of urine from the opening. The flow by the urethra also commences at this juncture, and the bladder is emptied in the usual way, the jet from the umbilicus ceasing, not to be renewed except by a violent accelerating action of the expulsor muscles. He can retain a pint of urine.

“By watching the first contractions of the bladder, it becomes evident, that to the thickened margin of the umbilical aperture are attached the muscular fibres of the bladder extending along the urachus; in fact, that the bladder and urachus are



formed into a urinary receptacle, which in shape may be compared to a curved-necked cupping-glass; the urethra passing out at its lower end, and its mouth being attached by muscle to the circular aperture in the linea alba. It becomes evident also, that the pouch of the hernia is formed by evasion of the posterior part of the neck only, which is of course attached to the upper half of the aperture, and when protruded, presses upon the hard edge of the lower half sufficiently to prevent the escape of urine, except under straining efforts of the abdominal muscles." (p. 295.)

**XXI. *A Case of Ileus complicated by Hernia.*** By Samuel Solly, F.R.S., Assistant-Surgeon to St. Thomas's Hospital.

On the 27th February, 1850, Mr. Solly was summoned to see a young man who was suffering from pain in the abdomen. On arriving at the house, he found his patient to be a clerk in the City, æt. 21, a remarkably fine and well-made person. It appeared that, ten days before this time, he had obstinate constipation, which was with difficulty relieved by the administration of strong aperients, but that the symptoms had quite subsided, and the patient had returned to his usual good state of health. Three days before Mr. Solly saw him, he was seized, after an excess in eating, with extreme pain in the belly, and subsequently, during the straining at stool, forced down a hernia, which was acutely strangulated. As the taxis failed to return the bowel, the operation was performed four hours after the descent of the rupture, and the intestine found in a tolerably healthy state. After the operation, the symptoms did not subside, but the vomiting continued, and although there were some evacuations from the bowels, they were of an unsatisfactory and unhealthy character; and, despite of judicious treatment, the patient gradually sank, and died just thirty-two hours from the commencement of his symptoms. On inspection, the external and internal abdominal rings were found quite free, but there was about a foot and a half of small intestine enormously distended, and nearly black, lying in the pelvis, and girt by a band about an inch and a half long, which was connected with the cæcum on one side, and with the peritoneum covering the iliacus internus muscle, close to the internal abdominal ring, on the other. This band was not so tight as to have strangulated the gut in its undistended condition, but effectually did so when the attack of indigestion had caused an accumulation of flatus. The case, therefore, adds another to the difficulties experienced in the management of intestinal obstructions.

**XXII. *Two Cases of Absence of the Thyroid Body, and Symmetrical Swellings of Fat Tissue at the Sides of the Neck, connected with Defective Cerebral Development.*** By Thomas Blizard Curling, F.R.S., Surgeon to, and Lecturer on Surgery at, the London Hospital.

In the Idiot Asylum at Highgate, Mr. Curling lately saw a female child in whom there existed on the outer sides of the neck, external to the sterno-cleido mastoid muscles, two tolerably symmetrical swellings, which had a soft doughy inelastic feel. In front of the axillæ there were similar but less defined swellings, and after death the whole proved to be loose lobular fat without any capsular envelope.

Some time afterwards he saw another female child, also an idiot, who presented similar anomalies; and in both cases there was complete absence of the thyroid bodies.

Mr. Curling believes these cases to be unique, and points out the circumstance as favouring the belief that bronchocele and cretinism have only an accidental relation, inasmuch that these are examples of a directly opposite condition, viz., cretinism and absence of the thyroid bodies.

**XXIII. *Account of a Case in which an Abscess formed in the Vesicula Seminalis, and proved fatal by Perforation of the Bladder and Extravasation of Pus into the Abdominal Cavity.*** By Mitchell Henry, Assistant Surgeon to, and Lecturer on Morbid Anatomy at, the Middlesex Hospital.

The patient in question, a sailor, 20 years old, was under the care of Dr. Crawford, in the Middlesex Hospital. He was admitted on the 19th of December, 1849, with symptoms simulating hip-joint disease, and extreme pain in the loin of the left side. The urine was very thick and scanty, and caused burning pain in its passage along the urethra. There was much febrile disturbance, with a quick, small pulse, constipated bowels, and a loaded tongue. Leeches to the groin, with calomel, opium, and colchicum internally, alleviated his symptoms for the time, but they afterwards returned with increased severity. The day after his admission, the urine was observed to be loaded with pus, which the patient stated he had noticed at intervals for three weeks before. He had never suffered from gonorrhœa, or from any form of syphilis, and had usually no difficulty in passing his water. On the 22d his febrile symptoms ran so high, that eight ounces of bled and cupped blood were drawn from the arm, with some relief. On the evening of the same day, blood passed with the urine in considerable quantities, and continued to do so in spite of gallic acid and other medicines which were given to check it. On the 29th violent purging came on, and continued until the next day. It was preceded by pain in the abdomen, but there was no rigor. Medicines and leeches relieved the pain in some measure, and stopped the purging; but on the 3d of January the abdominal tenderness returned, and about twelve o'clock he died in much agony, retaining consciousness to the last.

The *post-mortem* examination was made twelve hours after death.

The abdomen alone presented traces of disease. It was filled with cream-coloured pus, evidently from an abscess, and not the produce of peritonitis. The peritoneal covering of the liver and intestines was inflamed, but not in an extreme degree; the abdominal organs, including the kidneys, were healthy, and natural in appearance, except that they were pale. The bladder, which contained some purulent fluid, was perforated by an ulcer as large as a sixpence, but the source of the pus in it and the abdomen was not at first clear. At last, however, after carefully examining every locality from which it could have come, the remains of a large abscess were discovered in the vicinity of the left vesicula seminalis, which was partially destroyed by suppuration. The abscess had perforated the bladder by a small rounded hole, and discharged its contents into that viscus; and as at the same time the peritoneum reflected from the side of the bladder was implicated in the ulceration, the pus had found its way into the abdominal cavity, and excited the fatal peritonitis. What had excited the inflammation in the vesicula, and produced the abscess, is not clear; but the perforation of the bladder must have taken place long anterior to death,

whilst the pus was being constantly discharged with the urine. The interest of the case lies principally in this; that the vesiculæ seminales, though not exempted from disease, are rarely affected with acute inflammation recognisable during life; but there is no reason why the more chronic inflammation, described by Lallemand as extending to them from the ejaculatory ducts in severe spermatorrhœa, may not under favouring influences become acute, and result in abscess. Such may be the source of some of those obscure diseases, which go under the name of intra-pelvic abscesses. In like manner, abscess of the vesicula seminalis may be confounded with abscess in the prostate gland; and lastly, the excessive pain at the neck of the bladder, which occasionally accompanies acute gonorrhœa, and is attended with shivering and discharge of pus with the urine, may result from the formation of such an abscess, which is not of itself of course a mortal disease, unless it chance to discharge its contents into the abdominal cavity.

To this case is appended another by Mr. Cock, of Guy's Hospital, where abscess of the vesicula seminalis was diagnosed during life, and punctured through the rectum; but as the patient got well, the accuracy of the diagnosis could not be tested. The symptoms, however, were very like those detailed in the case above, especially in the existence of pain in and around the hip-joints.

At page 310 of this paper, a negative is omitted, which entirely changes the meaning of the sentence in which the omission occurs. It is intended to state that the vesiculæ are *not* exempted from acute inflammation, but the printer has left out the "not." What also is meant by *gravitating* a tumour? We presume that, owing, perhaps, to the illegibility of the MS., this word has been substituted for "puncturing."

XXIV. *On a Case of Albuminous and Fatty Urine.* By Henry Bence Jones, M.D. F.R.S., Physician to St. George's Hospital.

This is part of the account of a case of the so-called "chylous" urine; the chemical details are published in the 'Transactions of the Royal Society,' and the fatty matter has been specially examined by Dr. Hoffmann. The present paper contains only the history of the case, the microscopical examination of the urine, and the effects of diet and medicine.

The patient, aged 32, born in Trinidad of a Scotch father and coloured mother, was by trade a harness-maker. He was sober and had led a steady life. At Christmas, 1848, he observed the urine to be occasionally thick and white, and he suffered at the same time from pains in the loins, and general debility. On the 29th of May, 1849, he was obliged to leave off work. He came under Dr. Jones's care in October, 1849. The urine presented various appearances, according to the circumstances under which it was formed. Sometimes it contained only a few globules of oil; at other times it contained fibrin and coagulated spontaneously, and at the same time gave evidence of albumen, fat, and blood-corpuscles. It was then thick and white. Occasionally it remained clear and non-chylous for some hours or days, and then became again thick and coagulated. It was clear for eleven days after giving up work. It was most clear before breakfast, and clearer on a vegetable than an animal diet, but not in any great degree. The effect of firm pressure over the region of the loins seemed beneficial to a certain degree, but the effect was not very marked. Matico was de-

cidedly useful; but the medicine which had the greatest effect, and which in fact cured the disease, was gallic acid, which was taken to the extent of a drachm daily, and was continued for fifty-three days.

The following table by Dr. Jones shows at once the various effects of diet and medicines :

		In 1000 observations.	
		Chylous in different degrees.	Free from chyle.
1. On animal food the proportion is	.	968 times	32 times.
2. Vegetable food	„	910 „	90 „
3. Pressure belt loose	„	667 „	333 „
4. Pressure belt tight	„	628 „	362 „
5. When matico was taken	.	474 „	526 „
6. When gallic acid was taken	.	17 „	983 „
7. After gallic acid	.	0 „	1000 „

**XXV. Observations on the Prolonged Expiratory Murmur as a Sign of Incipient Phthisis.** By Theophilus Thomson, M.D. F.R.S., Physician to the Hospital for Consumption.

The author is a partizan of the opinion which holds that the prolonged expiration is a valuable sign of early phthisis; he says, that in the ordinary healthy state, the expiration is little more marked at one part of the chest than at another; he has never recorded it as abnormal, unless it was equal in duration to one third of the inspiratory murmur. It was detected in 195 men and 113 women applying for relief at the out-patient department of the Consumptive Hospital. Withdrawing from these patients 20 who had bronchial disease, in almost all the remainder the prolongation of the expiratory murmur was observed at the apices; in 108 on the right side; in 56 at the left; in 124 on both. Many of these patients were afterwards lost sight of; some became decidedly tuberculous; but the author seems to consider that the facts warrant him in believing, that this sign was in all really dependent on actual tubercular deposit. In proof of this he remarks, that there was hæmoptysis in 91 patients of the 288 referred to, or in 31 per cent. Now hæmoptysis, he says, occurs in the first stage of phthisis, in 43 per cent.; and the author therefore concludes, that—

“ Whilst, therefore, on the one hand, the coexistence of hæmoptysis in patients with prolonged expiration is sufficiently frequent to strengthen the supposition, that there is an essential connection between this altered murmur and phthisis; on the other hand, the frequency of hæmoptysis in these cases is so much below its average in consumptive patients generally, that when viewed in connection with the frequently trivial character of accompanying symptoms, we feel justified in drawing two important conclusions: namely, first, that the prolonged expiratory murmur takes precedence of other characteristic signs; and secondly, that hæmoptysis more frequently follows than precedes the deposit of tubercle.” (pp. 347-8.)

Dr. Thomson's opinion on this point must be admitted to be of value; but we do not see how the point in question can be accurately determined from facts of this kind. What we require is, first, a range of extensive observations, proving that this sign does not occur in persons who, from their symptoms at the time, and from the subsequent condition of their health, can be proved to be free from tubercle; and secondly, a corresponding extensive series of observations, proving that this sign existed

in persons who, from their symptoms or from the subsequent progress of the case, can be proved to have been tuberculous at the time. We confess, in opposition to Dr. Thomson, to a very strong belief, that prolonged expiration is frequently heard in the most healthy persons, both at the right and left apices, though most frequently in the former situation, and more in front than behind. But the subject well deserves a complete and final examination.

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We cannot bring this notice to a close, without adverting to another Paper, which, in our opinion, ought most decidedly to have found a place in the present volume of the 'Medico-Chirurgical Transactions.' We refer to the *Case of Hemiplegia, with Cerebral Softening, in which Loss of Speech was a prominent symptom*, which was communicated to the Society by Mr. Robert Dunn, but which, not having been deemed worthy of admission into the 'Transactions,' has been placed before the public by its author through another medium.\* We have already had occasion to advert to the insensibility exhibited by the managers of this Society, to the peculiar merits of Mr. Dunn's communications (Vol. V, p. 474). Possessing, in an extensive range of general practice, a very wide field of observation, and having a peculiar taste for that department in which Pathology and Psychology come into relation, Mr. Dunn has transmitted to the Medical and Chirurgical Society, from time to time, such cases as appeared to him specially interesting, from the light which they seemed to throw upon that most intricate branch of inquiry, the Physiology of the Encephalon. These cases have been well observed, well recorded, and well reasoned on; and amongst those who are most competent to estimate their merits, from the attention they have themselves given to this class of inquiries, we know that a high opinion is entertained of their value. Yet of these *three* cases, the Council have only selected *one*; and that one assuredly not the most important in its bearings. We must own ourselves at a loss to understand the meaning of the exclusion. Does it depend upon ignorance of the real value of such facts? Or does it arise from an indisposition to receive such contributions from a gentleman, who avowedly belongs to the "amphibious" order of "general practitioners?"

In either case, the cause is equally discreditable to the governing body, and equally prejudicial to its interests. We find "cases" innumerable admitted into the 'Transactions,' merely because they are rare or unique, having scarcely any direct bearing on practice, and still less on science,—in fact, mere curiosities of medical or surgical experience. We are far from objecting to the admission of such cases; but we claim an equal right of admission for a class, of which the rarity may be somewhat less, but the scientific importance is far greater; and of which every one that is well and faithfully observed, has a permanent value that entitles it to a distinguished place in the records of medicine, instead of being consigned to the *limbo* of a weekly journal, whose crowded pages receive alike the good, the indifferent, and the bad. What *principle* governs the selection, we must own ourselves quite at a loss to understand. The last volume contained a paper by Mr. Rainey on the minute structure of the Lung of the Bird, which, however valuable in itself, seemed to us not a little out of

\* See the 'Lancet' of Oct. 26 and Nov. 2, 1850.



place, being a piece of pure comparative anatomy and physiology; from the present we find excluded a case, which helps to solve one of the most intricate questions in human cerebral pathology. Let our readers judge for themselves, which has the best claim to admission into the 'Medico-Chirurgical Transactions' on the score of its *subject*. And if it be suggested that in the manner of treating it, either in a scientific or a literary point of view, there is anything inferior to the usual standard of the 'Transactions,' we venture to affirm that such is by no means the case, and that in style, as well as in matter, this paper is superior to some which are contained in the volume before us. If, on the other hand, it be made a principle to decide upon contributions, according to the order of practitioners from which they emanate, the Paper of Dr. A. being inserted because he is Physician to X— Hospital, and the Paper of Mr. B. being received because he is Surgeon to Y— Hospital, whilst the paper of Mr. C. is rejected because he is not connected with any Hospital at all, but is one of the "amphibious" grade,—such a rule ought in common justice to be made known. If the virtual governors of the Society would be ashamed to announce such a rule as that on which they act, they ought to be much more ashamed of acting upon it without announcement, and of professing to invite the assistance of gentlemen in maintaining the most important part of the Society's operations, whom they mean to take every opportunity of keeping at a distance.

We are far from intending to set ourselves up as the champions of the "rejected" of the Medical and Chirurgical Society; but this is not the only case of marked injustice which has come to our knowledge. We could mention a case in which a paper drawn up with great care, by one of the ablest clinical observers in this country, embodying the result of an extended experience, touching points of great practical importance, was rejected, for no other reason that we ever heard, than because its author dissented from conclusions advocated by a most distinguished physician, and former president of the society, who would himself have been the last, we feel confident, to desire the suppression of any evidence tending to the improvement of medical science and art, even if it should prove him to have formed a mistaken judgment. If such practices be continued, we think they will constitute a most powerful argument for the substitution of a system of *open* in place of *secret* reporting; we shall not hesitate to expose any similar cases that may come to our knowledge.

Having said so much upon the general questions which the rejection of Mr. Dunn's paper has brought up, we must be brief on account of the case itself. The subject of it, a lady advanced in life, had suffered from three attacks of apoplexy. The first of these, occurring in October 1844, seemed "congestive" in its character, and passed away without any other permanent consequences than this, that she continually used one word for another, not applying appropriate names to the things or persons she desired to signify. The second attack, in May, 1847, left her permanently hemiplegic on the right side, the power of voluntary motion being completely abolished, and but little sensibility being preserved, though reflex movements could be excited in the lower extremity by tickling the sole of the foot. This patient, we may observe, was one of the subjects of Dr. Todd's experiments on the muscular irritability of paralysed muscles, recorded in vol. xxx, of the 'Medico-Chirurgical Transactions;' which gives an additional

interest to her case. For the remainder of her life she remained altogether incapable of speech, not being even able to say *yes* or *no* in reply to a simple question, and never getting beyond the utterance of the monosyllable *dat-dat*; yet all her special senses were intact; *the motions of the tongue were free, and there was no difficulty in deglutition*. She did not seem to have lost any of her intellectual powers, but her emotional excitability seemed to be rather increased. Her general health continued good, up to the time of the last fatal seizure, which occurred in April, 1850, without any premonitory symptoms.

On *post-mortem* examination, the anterior and upper surface of the left hemisphere was observed to be much depressed and shrunken; and the vacuity was filled by serous effusion into the arachnoid sac. On further inspection, the upper two thirds of the anterior lobe of the *left hemisphere* was found to be in a state of complete destruction, with colourless softening, while the middle and posterior lobes were sound and healthy. The *right hemisphere* was healthy. But the greatest change was in the ganglionic masses at their base, and in the commissural structures. The upper half of the corpus striatum on the *left* side was destroyed by softening; the optic thalamus was shrunk to less than half its natural size, its upper surface being greatly wasted; while on the *right* side, a small and recent apoplectic clot was seen on the upper and anterior surface of the corpus striatum, the whole of the upper half of which was in a state of ramollissement, while, on the outer surface of the thalamus, also, were noticed some indications of white softening. The corpus callosum was destroyed, except at its anterior and inferior reflexion; and the anterior commissure and fornix were gone. The softened parts were microscopically examined by Dr. Todd, who found in them an abundance of compound cells, and of fatty matter in the capillaries.

We must pass over many other points worthy of notice in Mr. Dunn's commentary on this interesting case, to introduce his view of its bearing on the hypothesis of the localization of the faculty of speech in the anterior lobes of the cerebrum; a doctrine which has been made the subject of much discussion among French pathologists, and of which the affirmative has been sustained by M. Bouillaud. It has been objected to this view, that in several cases in which the loss of speech has been a prominent symptom, no lesion was found after death in the anterior lobes, the pathological change having been either confined to the corpora striata, or implicating with these the middle and posterior lobes of the cerebrum. The following remarks of Mr. Dunn's are well worthy of attention; and we believe that they will assist in clearing up the obscurities of the subject, even though they do not altogether dispel them:

"Now, in the consideration of this subject, it is never to be forgotten, that the perfect power of speech, that is, the power of giving utterance to our thoughts and ideas in suitable and appropriate language, *depends upon the due relation between the centres of intellectual action, and of the encephalic motor centres, through which the volitional power is exercised*. Thoughts or ideas may be moulded for expression in the seat of intellectual action, but the due agency of the volitional power, to give them utterance, requires the integrity of the commissural fibres, and of the motor centres, through which the volitional impulses of thought operate in speech. The imperfect power of articulation which we so constantly meet with, in hemiplegic patients, I have no doubt is owing to some structural lesion in the integrity of the motor centre of volition.

“It is admitted that there is no point in physiology more clearly made out, than that the cerebral or great hemispherical ganglia—that vast sheet of vesicular matter which crowns the convoluted surface of the cerebral lobes—is the centre of intellectual action, of perception, volition, and of all the intellectual operations and processes; and my own mind rests in the conviction, that the amount of pathological and other evidence which has been amassed, irresistibly establishes the position of Gall, that in speech *the power to frame the thought and mould it for expression has its seat in the anterior lobes of the cerebrum*. I hold it to be as indisputably established, that the corpora striata are the *motor centres of the volitional power* of the encephalon. *They are not the seat of volition itself, for that is an attribute of the intellect*, but the motor centres through which the volitional impulses of the cerebral hemispheres operate—the *connecting link of thought with action*. Their commissural connection with the centre of intellectual action, the great hemispherical ganglia, is so intimate and so extensive, that they are evidently subservient, at all points, through the agency of innumerable radiating commissural fibres, to the mandates of the will, or volitional power of the hemispheres, on all occasions, and in every voluntary act or effort; and hence does it not necessarily follow that loss of speech or power of utterance will alike result from *disease of the anterior lobes, or of such parts of the corpora striata as are in direct relation with them?*

“There is not, I believe, a single instance on record in which the power of utterance was retained *intact*, however sound and healthy the great hemispherical ganglia may have been found, where the corpora striata were both diseased. The apparently conflicting evidence which has been adduced as to the seat of the faculty of speech, admits of a satisfactory explanation, when thus considered in relation to the centres of intellectual action and motor centres of volition.

“In the present case, which I have narrated, it is quite evident that with the disorganisation of the left anterior lobe its functional power was entirely abolished; and though the right hemisphere was healthy, and there is every evidence, from the history of the case, that it maintained and exercised its functional power as a centre of intellectual action, still the volitional agency was wanting to give utterance to the passing thought, for the corpus striatum was not in its integrity.”

We think that we have now furnished sufficient reason for our notice of Mr. Dunn’s paper in this place, as one that ought to have had a place in the present volume of the ‘Medico-Chirurgical Transactions.’

## ART. VI.

*A Theoretical and Practical Treatise on Human Parturition.* By H. MILLAR, M.D., Professor of Obstetrics and the Diseases of Women and Children in the Medical Department of the University of Louisville.—*Louisville, 1850. 8vo, pp. 463.*

HAVING reviewed, in our last Number, Dr. Millar’s description of the mechanism of the second stage of labour, in connection with the obstetric properties of the pelvis, we shall now return to the first part of the work,—his account of “the uterus considered as an organ destined to expel the foetus,”—in which the mechanism of the first stage is considered. “The efficient cause of labour” and “the determinative cause of labour” occupy three important chapters. In describing the anatomy of the uterus, Dr. Millar discards the usual division into fundus, body, and neck; and prefers the simpler, and what appears to him the more natural, division into body and neck, the fundus being considered as a part of the body. In his account of the muscular parietes of the

uterus, Dr. Millar, to a certain extent, adopts the descriptions of Sir Charles Bell and Mad. Boivin, rejecting that of Dewees, who, "I know not by what authority, invests the uterus with a *complete suit* of circular, as well as longitudinal fibres, extending from the os uteri to the fundus, and was for his pains exceedingly puzzled to make the creation of his fancy behave itself seemly during labour." He recognises three sets of muscular fibres: first, the external layer of fibres diverging from the round ligaments, and forming fan-shaped muscles that spread over the fundus; second, the internal layer, forming concentric circles around the orifices of the Fallopian tubes; and thirdly, the circular fibres of the neck. The first two sets of fibres have been generally admitted; but the last, although described by Mad. Boivin, have not been so clearly proved. Sir Charles Bell could not find them out; and Dr. Millar, who is a firm believer in Mad. Boivin, thus comments on his want of success:

"Sir Charles Bell informs us, that 'he has not succeeded in discovering circular fibres in the os tincæ, corresponding in place and office with the sphincter of other hollow viscera,' but he does not tell us what he *has* discovered. I will endeavour to supply the omission. If we lay open the cavity of the uterus of a woman who has died during parturition, or at an advanced period of pregnancy, when the neck is unfolded, and look towards the os tincæ, we shall see that its external orifice is surrounded by a series of circles, enlarging as they recede from the orifice. Each circle belonging to this set is composed of four segments, united upon the mesial line anteriorly and posteriorly, and at the sides of the neck, and we have no difficulty in recognising these circular fibres as the penniform fibres, rendered horizontal by the expansion of the neck, which their peculiar arrangement is apparently adapted to favour. Such is the disposition of the fibres of the neck, as it appeared to me in the examination of several gravid uteri, even *without the aid of dissection*, and as one preparation in my collection will serve to show." (p. 62.)

Sir Charles Bell must certainly have been very blind if he could not find fibres, which were so palpable, that they could be seen without the aid of dissection; but upon this point Dr. Millar seems to be unusually clear-sighted, because he has found them equally in the virgin uterus, where he admits that "Nature, always economical in her endowments, allows it (the muscular coat) to remain in a rudimentary condition." He observes:

"Upon laying open the cavity of the neck, especially of the virgin uterus, a longitudinal column of fibres will be observed upon the mesial line of its anterior and posterior walls, and two others, not so prominent as these, at the sides of the cavity. From the mesial columns, little fasciculi of fibres proceed upward and outwardly, on both sides, in a penniform manner, to meet and unite on the lateral columns." (p. 59.)

Now, if by the term "fibre," as here used, we are to understand muscular fibre, it is clear that muscular fibres are much more obvious in the neck of the virgin than the gravid uterus, because the arbor vitæ in the virgin is thrown out in strong relief; but if we are to attach to it a different meaning, that these fibres consist of a certain structure, a parenchyma that is not muscle, but may contain the germ of muscular fibre within it,—a germ that buds only when conception takes place,—then it is quite possible that Sir Charles Bell may have been confused in endeavouring to separate true muscular fibre from the nidus that contained it, and failed to discover it in the abundance of the surrounding fibro-elastic tissue. We might even go further, and venture, without presumption, to

hint our suspicions, that Dr. Millar has mistaken the last traces of this arborescent structure, which he observed without dissection in the gravid uterus, for true muscular fibre,—that the penniform rugæ of the virgin uterus, stretched out into expanding circles as the neck began to form part of the great uterine cavity, were believed to be really circular muscular fibres.

If we dissent, then, from the anatomical fact, as stated by Dr. Millar, we are certainly not in the least disposed to admit the theory which the assumption is supposed to explain. Adopting the doctrine maintained by Stolz, Dubois, Cazeaux, and Chailly, that the neck of the gravid uterus forms no part of its cavity until the last fifteen days of pregnancy, or at least until the commencement of the ninth month, Dr. Millar assigns to the neck, as one of its offices, the guardianship of the ovum :

“In the second place, the cervix uteri acts as a sentry over the product of conception, by not participating in the development of the body until gestation is considerably advanced, and then only in a gradual manner, and after a fashion peculiar to itself.....If, then, the neck contributes nothing to the cavity destined to contain the foetus, nothing to the reception or maintenance of the ovum, but shuts up the cavity and opposes the exit of the contents, it is truly the *sphincter* of the uterus, for such is the very definition of the term. What avails it, then, to object with Sir Charles Bell, that there are no circular fibres in the os tincæ, ‘corresponding in place and office with the sphincters of other hollow viscera,’ or that the loosening of the orifice and softening and relaxation which precedes labour is quite unlike the yielding of a muscular ring. Such a sphincter as that of the rectum or urinary bladder would be altogether unfit for the uterus, which has to retain its contents for nine months. The uterine sphincter ought to be constructed and endowed with reference to a more persistent exercise of its office, else premature expulsion of what it is appointed to keep must, as we have seen, be the consequence. A mere ring of muscular fibres, like that of the rectum or bladder, would be inadequate to the retention of the contents of the gravid uterus for nine months. Again, the uterine sphincter must not yield to the expulsive efforts of labour, as readily as that of the rectum or bladder in defecation and micturition, else the woman and her offspring will be exposed to the dangers that are known to attend precipitate delivery. Hence the necessity of a sphincter that will offer considerable resistance to the escape of the child,—precisely such a one as the uterus possesses.” (p. 75.)

We are surprised that Dr. Millar did not perceive the *petitio principii* contained in this argument against Sir Charles Bell. He insists that the neck of the uterus is a sphincter, though it differs altogether from the sphincters of hollow muscular viscera. It disobeys all the laws of sphincteric action in expulsatory organs, it yields very slowly, it is an antagonist to the expulsive power, it is not “a mere ring of muscular fibres which would be inadequate to the retention of the contents of the gravid uterus for nine months;” it differs in all these respects, and yet it is a sphincter—*sui generis*. This mode of reasoning might prove anything, and we are sure Dr. Millar will at once see, that the very facts he has stated become the strongest arguments against the popular opinion that the neck of the uterus acts as a sphincter muscle. A mere ring of muscular fibres is certainly inadequate to retain the contents of the gravid uterus for nine months; something else must assist, and if so, the expansion of that tissue cannot be the relaxation of a muscle. Again, Nature is too economical of her power to waste it by placing two forces in absolute antagonism; the sphincters of the stomach, rectum, and bladder, soon yield to the



power exercised against them, or, if they do not, the worst consequences ensue: and in a similar manner, if the neck of the uterus were a sphincter, it should either yield at once, and "the woman and her offspring be exposed to the dangers that are known to attend precipitate delivery;" or it should become the antagonist of the fundus, and a rupture of the uterus, perhaps, be the consequence of a struggle between these opposing muscular forces. No such opposition takes place; the resistance offered to the muscular power of the uterus arises from a tissue that is not muscle, that yields not suddenly but gradually, that is prepared to yield not only by becoming "softer" and "loosening," but by an active dilatation, an expansibility that is not confined merely to the neck of the womb, but is observed equally in all the tissues of the vagina and perinæum. The effect of muscular contraction opposing the action of the uterus, may be noticed in those cases in which a spasmodic constriction of the neck takes place; for whether it occur in the first stage, before the head has passed from the uterus into the pelvis, or afterwards, in the second stage, the effect is the same, labour is arrested. Hence, spasmodic contraction of the cervix uteri has been pointed out by authors as an efficient cause of protracted labour. Dr. Millar alludes to it in the second stage. "A far more serious cause of protracted labour, peculiar to vertex presentations, consists in the permanent contraction of the cervix uteri about the neck of the child." (p. 286.) Hamilton refers to it in the first stage, and its effect has been thus graphically described by him:

"Resistance to the dilatation," he observes, "in consequence of an undeveloped band of the cervix uteri, is happily of rare occurrence; if not understood, *it is productive of a degree of distress which can hardly be described*. The author has been called in to cases where patients have been suffering from this cause for above thirty hours, and where the symptoms were truly alarming. Great heat of surface; frequent pulse; constant nausea; and occasional tremors resembling convulsions, and distinguished only by the consciousness remaining entire; are the symptoms produced by the protraction of labour from this cause." (Hamilton's 'Practical Observations,' p. 131.)

Just as in the bladder and rectum, so in the uterus, any antagonism between muscular forces causes the greatest distress; and consequently we believe that "a sphincter that will offer considerable resistance to the escape of the child," would be most detrimental to the safety of the patient, and we must add, "is precisely such a one as the uterus" does *not* possess. Holding then these opinions, we must dissent from the doctrine laid down by Dr. Millar in his chapter on "Labour—its Efficient Cause." It is thus expressed: "But the neck *resists*, and resistance to the escape of the foetus is the alone effect of the contraction of its fibres; it is this that makes parturition necessarily *laborious—hoc opus, hic labor est*. Labour, then, according to our view of it, is a contest between the body and neck of the uterus,—the former aiming to expel the foetus, and the latter to retain it." (p. 87.) Apart, however, from the principle here laid down, this chapter is quite unobjectionable. The efficient cause of labour is uterine contraction. Dr. Millar properly divides this into *muscular contractions* that alternate with relaxation, and *tonic contraction* that is permanent, resulting from contractility of tissue, "a vital property that pervades all animal textures." Dr. Millar lays rather more stress than authors generally do, on the action of the external fan-shaped or penniform

muscles; and in place of considering them, with Sir Charles Bell, as holding a secondary office, viz., "to bring down the womb in the first stage of labour, and to give the uterus and head of the child the right position with regard to the axis of the pelvis," he assigns to them a chief part in shortening the axis of the uterus. He demonstrates their action to prove, that the effect of these oblique fibres is the same as that of longitudinal fibres would be, passing directly from above downwards. *The concentric circles of the body of the uterus* act simultaneously with these, and have the effect of causing "the walls of this part of the uterus to approximate so as to diminish its cavity in every direction. The fundus of the uterus must at the same time be depressed by them" (with the concentric circles round the fallopian tubes), "so that they assist the oblique fibres in diminishing the length of the uterus." (p. 85.) "Thirdly, the contraction of the circular fibres of the neck," according to Dr. Millar, "diminishes its caliber, and closes and diminishes its vaginal orifice." (p. 86.) "In virtue of the *tonic contraction*, the uterus constantly tends to resume its unimpregnated volume, whenever the cause that distends it is removed." (p. 88.) And its effect is thus illustrated by Dr. Millar:

"After the rupture of the membranes and escape of a portion of the waters, the tonic contraction has a fairer chance, if we may so express it, to display itself. The cavity of the uterus is not then filled by the foetus, and its walls would hang flabby and relaxed about it but for the tonic contraction, which tightens and brings them into contact with the foetus, and succours the muscular in expelling it. This succour is rendered by maintaining the ground gained by each successive muscular contraction, without which it is difficult to conceive how the expulsion could be achieved at all. . . . In order that any number of muscular contractions may expel the foetus, it is, therefore, necessary that some means be devised to secure the advance made by each, and the tonic contraction is the means appointed for this purpose, which, besides rendering this indispensable aid, protects the woman against hæmorrhage afterwards, by diminishing the caliber of the utero-placental vessels." (p. 91.)

"The contractions of the diaphragm and abdominal muscles, as accessories to labour," Dr. Millar considers as a second class of muscular contractions that contribute to the expulsion of the child. Having alluded to Cloquet's and Bourdon's opinion, that the diaphragm does not exert any *active* pressure upon the superior part of the uterus, he dissents from their view:

"I am not able to understand on what grounds an *active* agency can be denied to the diaphragm. If it be admitted that its contraction is simultaneous with that of the abdominal muscles, it must press downwards or at right angles with those muscles, and thus cause the foetus to move in the diagonal of the two forces, which, as already stated, is in the direction of the inferior aperture of the pelvis." (p. 93.)

Dr. Millar further points out their influence in supporting the uterus while it is in action:

"But besides this propelling agency, the abdominal muscles and diaphragm are subservient to labour, by embracing and supporting the uterus while it is in action. The support thus rendered is a great protection against ruptures, while it excites the organ to increased energy of parturient action. Who does not know that firm pressure upon the uterus, through the abdominal walls, is our main reliance to excite its contraction, in cases of hæmorrhage from inertia after delivery?" (p. 94.)

These muscular contractions, then, taken collectively with those already

alluded to, constitute the efficient cause of labour. We shall not dwell on the discussion in the following chapter "On the Determinative Cause of Labour," lest we arrive at the satisfactory conclusion of Avicenna: "at the proper time, delivery takes place by the grace of God," which has been quoted again and again as a sufficient answer. It reminds us, however, of the answer given by an under-graduate of one of our Universities, who when asked, "Upon what did the motions of the heavenly bodies depend?" replied, "Upon the will of God."

Thus the great first Cause becomes a convenient subterfuge to conceal our ignorance of causation. Dr. Millar is not satisfied with such truisms, but having examined the learned and ingenious speculations of Baudelocque and Adelon, is disposed to adopt the theory proposed by Dr. John Power, that the exciting cause of labour is irritation of the cervix, and especially of the os uteri, arising from the contact of the ovum with it. The arguments he advances in favour of this opinion are, First: "The peculiar manner in which the uterine neck is unfolded during pregnancy." . . . "Its unfolding, so as to admit the ovum into contact with it, is deferred to a very late period of pregnancy, until, in fact, a short time before labour sets in. What other use can be assigned for this singular deportment, than that of guarding the neck from premature irritation, which might endanger premature expulsion of the ovum." (p. 103.) Secondly, "The rectum and bladder being excited to expel their contents by irritation of their orifices, afford strong ground of presumption that the uterus is excited to action on the same principle." And, lastly, "The uterus can be excited to expulsive contractions, especially in the latter months of pregnancy, by artificial irritation of its orifice." For these reasons, Dr. Millar adopts the theory of Dr. Power, as being the nearest approximation to the truth.

Dr. Millar next proceeds to consider "the phenomena of the first stage of labour," viz., the pains, the show, the dilatation of the os uteri, and the formation of the membranous pouch. With regard to this last, Dr. Millar alludes to the fact observed by Müller and Wigand, "the apparent elevation of the presenting part of the child at the beginning of each pain, and its depression before the pain goes off." He objects to the doctrine Müller and Wigand founded upon this fact, viz., "that the uterine contractions appear to commence at the os uteri, to be propagated toward the fundus, and again to return thence toward the mouth of the uterus." He considers that, "as the pouch fills with the waters of the amnion during the pains, it withdraws from the presenting part, which makes this appear to rise up; or, if there be actual elevation, it may be owing to the reflux of the waters from the pouch, until its equilibrium is re-established by the steady persistence of the pain, when this part descends again." (p. 118.) Wigand's explanation is adopted by Rigby and Churchill, but opposed by Murphy, who makes very nearly the same objection as Dr. Millar.

"In Wigand's explanation, the influence of fluid pressure seems to be altogether forgotten. The immediate effect of contraction commencing at the fundus would be to compress the liquor amnii, which of necessity forces its way before the head or mouth of the uterus. The fluid in this position reacts against the head with the same power that it is compressed, and therefore pushes it up until the increasing contraction of the fundus forces the head down again."\*

\* Murphy's *Lectures on Parturition*, pp. 52-3.

In his view of the agency of the membranous pouch, Dr. Millar opposes strongly the explanation of Denman, that "it forms a soft pillow, which at the time of every pain, acting *upon the principle of a wedge*, operates with increasing force according to the size it acquires." The wedge-like action of the membranes Dr. Millar very properly objects to, inasmuch as a great portion of the dilatation is accomplished before the wedge is inserted, that is, before the membranes get within the os uteri. He rather considers that the liquor amnii acts upon the cervix as an expansive force.

"The expansion of the inferior segment of the ovum distends the cervix uteri in such a manner as is tantamount to pulling asunder its parietes and stretching its vaginal orifice. . . . The first effect of each uterine contraction is to propel the ovum toward the os uteri; but this being resisted by the os, the inferior segment of the ovum begins to expand, and the starting point of this expansion is at the os uteri, from which it spreads in all directions, operating as a great number of cords pulling from the os toward the circumference of the cervix. . . . When the resistance of the cervix is considerably subdued, and the external orifice is opened to some extent, a portion of the membranes becomes insinuated within it, and the pouch thus formed contributes materially to complete its dilatation." (pp. 120-1.)

We are, however, rather surprised to learn from Dr. Millar, that "British writers, since the time of Denman, have cleaved to the wedge-like operation of the pouch with peculiar pertinacity." Burns alludes very slightly to the mechanism of dilatation, but what he does say by no means conveys such an impression. "Whilst the os uteri is beginning to dilate, the membranes *have little tension, they scarcely protrude through the os uteri* until it be considerably opened."\* Blundell observes,

"When first an examination is made, the os uteri being little dilated, the membranes *with the waters not protruding*, perhaps the cyst cannot be felt, and in your obstetric noviciate, deceived by this circumstance, you may imagine that the water is already discharged, but as labour advances the fluid collects about the mouth and neck of the womb, and first the aqueous cyst is felt within the uterus, and afterwards, tense and overcharged during pain, it pushes down through the *dilated* os uteri, forming there within the vagina a hemispherical swelling, the gathering of the waters, in form like the heart, but without softness."†

Rigby, in allusion to the formation of the bag of the liquor amnii, states, "When the os uteri has dilated more or less, a quantity of liquor amnii begins to collect between the head and the membranes, so that when a pain comes on, they form a tense, elastic, and conical bag, which presses firmly *against the os uteri and protrudes through it* into the vagina, and from its form and elastic nature greatly facilitates the speedy dilatation of it."‡ Murphy, in speaking of the effect of the liquor amnii, guards against the notion that its force is wedge-like, acting only in one direction. "The force conveyed by the fluid, you are aware, does not act in one direction only, but is distributed to every part of the surface to which the fluid is applied. The force, therefore, which is exerted to expand the mouth of the uterus, being communicated by a fluid, is *not only directed against the os tincæ, but against the fundus and sides of that organ*."§ The views here given by British writers of the action of the

\* Burns, p. 355. London, 1832.

† Blundell, by Castle, p. 233. London, 1834.

‡ Library of Medicine, vol. vi, p. 100. Lond.

§ Murphy's Lectures, p. 51.

membranous pouch are not very unlike that of Dr. Millar himself, and certainly do not convey the idea of a wedge forcing open the os uteri. Dr. Churchill certainly speaks of the bag of waters forming "a firm and equable wedge," and Dewees, an American writer, in his zeal against Denman, argues with his usual energy against this mechanic power, which he assumes to be a British conceit; but beyond this we have no evidence that our obstetric authors hold such an opinion. It is probable, therefore, that Dr. Millar, who may have read Churchill and Dewees more attentively than other authors, has formed his opinions upon their authority. Be this as it may, however, we are satisfied that nearly all British obstetric writers will agree with Dr. Millar, that "the formation of the pouch is the consequence not the cause of the dilatation, or at least, the dilatability of the os uteri." (p. 122.) The value of the liquor amnii, as a fluid adapting itself to the shape and inequalities of the cervix, equalizing pressure, preventing undue irritation, and conveying the propelling force in the most favorable manner, has been well pointed out by Dr. Millar, who has taken much pains to prove the absurdity of Dewees's doctrine, that the membranes and liquor amnii are useless, so far as the dilatation of the os uteri is concerned.

The most important part of Dr. Millar's work, because it is the most practical, is his discussion on *the treatment of labour*. His views of the causes of labour or its mechanism may or may not be true; but these questions after all being only theoretical, the student may discuss them without any great apprehension of danger if he should err. It is not so with the treatment of labour; an error here leads to practical mischief; and consequently the value of Dr. Millar's work to the student must be tested by the correctness of his practical opinions. In the fulfilment of our duty we must examine them impartially, and criticise freely whatever is amiss.

Dr. Millar, even in his preface, declares himself the pupil of Hamilton and Burns; and, we infer, the opponent of Denman, Blundell, Ramsbotham, and the British school of midwifery; in other words, he discards the section of expectants who advocate patience in protracted labours, and adopts, with Hamilton, the rule of interference to limit their duration.

In the discussion of Dr. Millar's opinions, therefore, we are necessarily drawn into a sea of controversy, even now tumultuous from the tempests that have passed over it. We shall endeavour to avoid these collisions of opinion, while we seek to gain the haven of truth. Dr. Millar thus lays down, in his treatment of the first stage, one of the questions we have before us: "The first is the most dilatory of all the stages of labour, it being the preparation for what is to follow, and preparation in most things usually requiring a longer time than execution. It may, therefore, be reckoned a prime duty of the accoucheur to wait patiently, as a general rule, until this stage is naturally accomplished. But is there no limit to the patience he must exercise? Is there no dilatoriness on his part which is reprehensible? Must the first stage of labour be always left to take its course, unless there be some uncommon and palpable necessity of interference?" Dr. Millar then contrasts the doctrine of Denman with that of Hamilton, adopting the rule of the latter, "that the termination of the first stage of labour should be secured within twelve or fourteen hours from its actual commencement;" by the adoption of which rule, our author asserts



confidently, that "no patient under his charge for the last thirty-five years has been above twenty-four hours in labour, and except in cases of disproportion, none so long." So far, this is an encouraging support of Hamilton's opinions; but it is necessary for us to require a precise definition of this limit of time, to render the rule intelligible, and to avoid unnecessary differences. Upon this point, Hamilton is remarkably ambiguous. "In prescribing the limits of the first stage, he is careful to guard against any mistake or abuse that might grow out of the restriction: there must be a continuance of regular pains for the period specified, 'for it sometimes happens that, after regular pains have commenced, the agitation of the patient, or the mismanagement of the attendants, occasion a suspension for some hours. If there be no injurious pressure upon the passages during that suspension, the patient's strength is recruited, and the duration of the first stage is to be reckoned from the recurrence of the pains.'" (p. 130.)

"And then again," observes Dr. Millar, "spurious pains are to be discarded from the estimate; these may precede the true ones for hours or days, producing no tightening of the edges of the os uteri; and unless this is present, labour has not really commenced." (pp. 130-1.) According to this definition, then, we cannot date the period of the first stage, from the primary dilatation characterised by the show, to its termination in the complete dilatation of the uterus, because within that time there may be, and often are, one, two, or more suspensions of uterine action, suspensions that, if they occur at night, may occupy very nearly twelve hours. Such instances of protraction in the first stage must be omitted from the definition. We are confined to those cases where the action of the uterus is continuous for twelve hours and upwards. This brings the question within very narrow limits, and excludes as exceptional a large number of cases that are protracted beyond the specified time. For instance, it has occurred to us to meet with cases something of this character. The pains and show were observed to commence during the night, say at midnight. The pains were slight, returning at long intervals; but in the morning, perhaps about eight o'clock, became more frequent and active. The labour may now be said to have set in, and the action of the uterus continues steadily for ten hours, when it is suspended, the os uteri being as yet but slightly opened. In two hours more the action is renewed, but less actively, and again ceases in an hour or two, when the patient falls into a sound sleep, from which she is roused in the morning at five, six, or eight o'clock, by a renewal of active labour, and is delivered, perhaps, in four hours from that time; the dilatation of the uterus, which occupied the whole previous day without making any remarkable progress, being now completed in an hour or two.

If such a case came under the observation of Denman, or any of his followers, they would date the labour from midnight, when the show appeared, carry it through the whole of the following day and night until the hour that delivery took place in the morning, that is, from twelve o'clock midnight, to twelve o'clock midday of the day next but one following, being thirty-six hours, of which period the first stage occupied thirty-four hours. On the other hand, Dr. Hamilton and Dr. Millar would only commence their calculation at eight o'clock in the morning, when the pains became frequent and regular; we presume neither would interfere within the ten

hours that these active pains were continuing; afterwards they are suspended, return for a short time, the patient falls asleep, and it is only in the morning that active labour recommences, which is concluded in four hours. This long interval of suspension interrupts the calculation, which must now be dated from the *recurrence of the pains*. The whole labour is completed in four hours; the first stage in two hours. Thus it will be perceived, that the duration of the very same labour might be set down by one party as being thirty-four or thirty-six hours, and by the other as being only two or four hours. It is very essential to point this out, because it affects the very elements of the arguments that are derived from statistics. Thus we find Dr. Millar perfectly astounded by the statistical evidence brought forward by Dr. Churchill, to prove, that in protracted cases of labour, the duration of the first stage occupies the chief portion of the period, the duration of the second stage being in inverse proportion to the first. Dr. Millar, however, takes advantage of a paragraph in Dr. Churchill's work, to prove that there is an error in the elements of the question:

"I was, I confess, greatly astounded when my attention was first directed to these statistics, not because they militated against a favorite doctrine or theory, (that I could have easily surrendered at the summons of truth;) but because, if they are to be relied on, I have yet to learn the alphabet of practical midwifery . . . . . In rehearsing my experience in general terms, I have been careful to say 'liable,' because there is no rule without exceptions, and it does sometimes happen that a tedious first stage is followed by safe and reasonable promptitude of the second. These exceptions are not, however, numerous enough to account for Dr. Churchill's one hundred and forty-three cases, which he says expressly were not called for the occasion. How, then, are they to be accounted for? I could never have answered the question, if, in turning over his pages, I had not stumbled on his definition of the first stage, which is as follows: 'extending from the commencement of labour to the passage of the head through the os uteri.' When the head has passed through the os uteri, it is, I opine, near its journey's end; for nothing is more common than for the anterior edge of the os uteri to be felt in advance of the head, a few moments before it emerges, although the pains may have been decidedly expulsive, that is, the second stage existed, for hours previously." (pp. 134-5.)

Thus Dr. Millar attempts to reconcile Dr. Churchill's statistics with his experience, and so may we venture to bring Dr. Millar's statement, that "no patient under his charge for the last thirty-five years has been above twenty-four hours in labour," within the limits of probability. It is the more necessary to point out the possibility, that the difference in estimating the duration of labours may depend on a difference in the data of the calculation, much more than on the labours themselves; because the practitioner who meets with tedious cases may be sadly embarrassed, and perhaps may incur some degree of odium, if he cannot bring his laborious duties to a conclusion within the period here laid down. The boast of Millar, like the boast of Hamilton, stares him in the face, and completely disconcerts him.—What we have been considering, however, is only a preliminary question; the treatment of this stage, and the means by which it must be brought to a timely conclusion, can only be considered in detail, by examining the means proposed to remove the several causes of delay. One of these is obliquity of the uterus. Dr. Millar having alluded to the usual methods of changing the position of the patient, applying a bandage to the abdomen, &c., proceeds: "Should strict

attention to posture, continued for a reasonable time, fail to correct the obliquity, and the labour in the meanwhile make but tardy progress, it is proper to *hook the os uteri, by inserting the extremity of the finger within its orifice*, and draw it toward the centre of the pelvis in the intervals of the pains." (p. 138.) If the uterus were made of wood or of copper, as we have seen in some models, we could understand how its too great obliquity would be corrected by hooking a finger in the mouth, and drawing it toward the centre of the pelvis; but inasmuch as it is not composed of such inflexible materials, but on the contrary bends so completely on itself as to produce this displacement, the only effect which such manipulation could have is to increase the obliquity. The fundus of the uterus hangs too much over the pelvis, and the os uteri is consequently directed too much towards the sacrum. Dr. Millar would correct this by hooking the os uteri and drawing it forwards, thus doubling the neck more on the body of the uterus than it was before. Unintelligible as this may seem, Dr. Millar has succeeded, and Dewees also, in correcting by such means this displacement; but we are greatly inclined to suspect that it was after the manner that Dewees corrected face-to-pubis presentations. Dewees held the man incompetent to practise midwifery, in its best manner, who could not detect and change this malposition of the head; but Dr. Millar since has "experimentally allowed nature to take her course, and found that the desired mutation is generally accomplished about as well without as with my assistance." (p. 256.) So in the case before us, Dr. Millar quotes a confession of Velpeau very much to the purpose. "M. Velpeau, not to mention others, confesses that, whereas he once faithfully laboured to correct obliquity, an incident which occurred in his practice convinced him that he had been spending his strength for naught. 'One day,' says he, 'I was under the necessity of leaving a case in charge of a pupil, who neglected the instructions I had given him. I returned in three hours and found the cervix completely dilated, the membranes ruptured, and the head well engaged. Since that time I have done nothing in such cases, and the organism has always succeeded in bringing everything right.'" (p. 141.) Nature can overcome many difficulties, and we are persuaded she has succeeded in bringing everything right in many of Dr. Millar's cases, in spite of his hooking process; more especially when the fundus of the uterus has been properly supported, in order to increase the efficiency of its action. So far, therefore, as the correction of obliquity is concerned, we do not consider the rule of Dewees and Dr. Millar of the least value; while as a means of exciting unnecessary irritation in the os uteri, we much deprecate the practice, and agree with Ramsbotham, Churchill, and the great body of British practitioners, that such cases are precisely those in which "a meddlesome midwifery is a bad one."

*Inefficient action of the uterus* is another cause of tediousness, which Dr. Millar corrects with his finger. He sets out with the principle, that "If any morbid state of the system can be reasonably assigned for this condition of things, it ought to be corrected; if the pulse is full and strong, blood should be abstracted; if the bowels are confined, they ought to be relieved by an enema or a dose of castor-oil. If these means fail, or not being indicated are not resorted to, the proper remedy is, *irritation of the uterine orifice by means of the finger, for the purpose of exciting*

*more efficient contractions of the organ."* (p. 144.) Dr. Millar then describes the manner of doing this :

"It is rarely necessary to employ more than the index finger, the extremity of which is to be introduced within the orifice, in the absence of pain, with its pulp or feeling surface turned toward the anterior lip.... When a pain comes on, or after the lapse of the usual interval, whether there be pain or not, and for the purpose of exciting one, pressure is to be made with the finger moved slowly around, so as to bear successively on every part of the anterior semi-circumference of the orifice. Having described a half circle in one direction, for example, toward the right side of the patient, the finger is to be moved in the same manner in the opposite direction, and these movements are to be continued during the pain, or, if there be no pain, for a minute or so. It is then to be withdrawn from the orifice, but retained in the vagina, or if kept within the orifice, it must rest from its work for a few minutes. When pain recurs, *or should recur*, the finger is to be used in the same manner, and so on until the uterine contractions become stronger and act with more efficiency upon the orifice, when it is to be withdrawn from the vagina. The invigorated contractions may finish the dilatation of the orifice without any further assistance. Should they flag, however, or progress slowly, the finger may be reintroduced, from time to time, to freshen them.... To produce the requisite orificial irritation, the finger must press with different degrees of force, in different cases ; but in all cases the pressure should be gentle at first and gradually increased, and it is never allowable to use such a force as would be required literally to stretch the os uteri. The mere contact of the finger is not sufficient to excite the os uteri ; it is therefore necessary to make pressure with it ; but it must be remembered that this pressure is intended to *stimulate*, not to *force open*, and that it acts upon a *vital*, not upon a *mechanical principle*." (p. 145.)

We have given, at a greater length than otherwise we should have thought necessary, Dr. Millar's digital method of shortening the first stage of labour, of bringing it within the prescribed limits, and of removing inefficient action of the uterus, lest we might do him an injustice by abridging it. We trust our readers will form a fair estimate of its value, when they bring to their recollection those cases of labour where frequent vaginal examinations have been made. If, for the mere purpose of ascertaining the progress of labour, we are cautioned against making examinations by touch too frequently, lest irritation, inflammation, and arrest of the labour be the consequence, we can judge what must be the effect of this more than exploratory process, on a uterus already acting inefficiently. Does Dr. Millar imagine that he can nicely discriminate between the pressure that only stimulates, and that which produces irritation ? Does he believe that the theory of orificial irritation justifies this use of his fingers ? Is there no probability that the correspondence, the beautiful harmony, which exists between the irritability of the cervix, and the action of the fundus may be utterly deranged, if not destroyed, by such rough manipulation, the periodic action of the uterus become irregular, and as irritation increases, altogether cease ? We ask these questions, because Dr. Millar, resting upon Dr. Power's theory and his own experience, sets all authority at defiance upon this point. We humbly submit that the theory does not justify the practice, and Dr. Millar's experience stands alone against that of the obstetric world, not even excepting Dewees, who, at the bedside, was certainly not an admirer of Job. Dewees says :

"When the os uteri remains unyielding for a long time, it is an evidence that the natural processes which so beautifully, kindly, and safely effect this change, have for some cause or other been interrupted. And *though mechanical force may*

*be made to usurp the organic function*, it nevertheless will always be at the expense of health, or even the integrity (be this more or less) of that portion of the uterus to which the force is applied. So well assured am I of this fact, that I never employ force to open the os uteri. Nor do I hold the argument 'that no mischief has been seen to follow this plan' of the slightest weight, as we have it not in our power, at the moment, to determine satisfactorily any consequence but the proximate or immediate effect of the violence, which may be, and most probably is, but slight or even unappreciable at the instant it is committed. But can we with any certainty declare that many of the severe and dangerous chronic affections of the neck of the uterus do not owe their origin to this cause?" (pp. 148-9.)

Dr. Millar replies to this by charging Dewees with inconsistency, as being an advocate for the hooking-process, and observes: "Now I leave it to the reader to decide which of those manipulations is most likely to do violence to the os uteri, and plant the seed of future disease, which spring so luxuriantly before the excited imagination of the doctor." (p. 149.) We really cannot decide, inasmuch as we look upon the one and the other in the same light; but with regard to the question before us,—the relieving insufficient action of the uterus, in the manner described, by irritation of the cervix,—we must pronounce upon it a sentence of most unqualified condemnation, as being not only unscientific in its principle, but unsafe in its practice, and especially because the rule is calculated to encourage most mischievous meddling.

*Impeded action of the uterus*, the result of premature rupture of the membranes, is another cause of delay that Dr. Millar relieves by manual assistance. The case is that which Dr. Hamilton described as "the interposition of a portion of the cervix uteri between the head of the infant and the bones of the pelvis." Dr. Millar has certainly succeeded here, not only in proving the advantage of digital aid, but in giving a more rational explanation of the cause of this partial descent of the os uteri than preceding writers. He objects to the opinion that the cervix, in such instances, is caught between the head and the pelvis, by which its retraction is prevented, and puffiness and timidity takes place; on the contrary, he considers it rather to be the result of the unequal, and therefore unfavorable, action of the uterus on the cervix.

"The os uteri dilates more tediously and painfully, because the cervix is not so equally distended by the head or other presenting part of the child, as it is by the membranes with the fluid they enclose, and unless there be this equal distension, the propelling force is not transmitted to the orifice first, and then equally radiated to the circumference of the cervix, but is expended upon such points of the cervix as are most pressed upon by the head of the child.... This unequal pressure operates, moreover, to disturb the equilibrium of the circulation in the neck of the uterus; being prevented from returning across the outer cervical circles, the blood is accumulated in the os tincæ, and hence its timidity and puffiness noticed as of frequent occurrence by all practical writers." (p. 155.)

Dr. Millar objects to the view which Hamilton takes of this condition of the cervix, that—

"1st. Such interception is not necessary to explain what is observed; the unequal pressure of the head upon the cervix, without the counter pressure of the pelvic bones, being sufficient.

"2d. There is no particular reason why premature any more than *mature* rupture of the membranes should cause this interception, seeing, as has already been observed, nothing is more common than for the os uteri to continue between the



pubis and head until a short time before the egress of the latter, and yet the symptoms ascribed to this interception, are most commonly observed in cases of premature rupture of the membranes." (p. 156.)

It might be replied to Dr. Millar, that the presence of the liquor amnii, a soft fluid medium between the head and cervix, makes all the difference in the effect produced. So long as the membranes are entire, *interception* is prevented, but takes place the moment the waters are discharged; nevertheless we must agree with Dr. Millar, that the evidence of this incarceration is by no means satisfactory; and must dissent from the practice of Hamilton and Gooch *to push up* the edge of the orifice over the head of the child, to liberate the band of the cervix supposed to be incarcerated; since this is "a condition which, if it really existed, could scarcely be reached by such a procedure." We have frequently had occasion to treat such cases, and never could succeed in pushing up the anterior lip so as to relieve the supposed stricture; it invariably descended again, and not unfrequently retired of its own accord when the fruitless attempts were discontinued. Dr. Millar's explanation resolves the enigma of such cases, inasmuch as there was no stricture to remove, and so long as the position of the head was unaltered, no effect was produced. The directions of Dr. Millar for the relief of these cases appear to us, therefore, extremely judicious:

"In the treatment of tedious dilatation resulting from this cause, it will be proper to detract blood freely, if the os uteri be hot, tender to the touch, and rigid as well as tumid; but the judicious employment of the finger in aid of the uterine contractions, is much more frequently indicated, and is often the only thing that can be done to assist the patient. The fingers are not to be used to excite uterine contractions (for they are already too strong), nor to stretch the os uteri, but *to press upon its margin* during the pains, in order that their counter pressure may keep it in as firm contact with the head as the rest of the cervix, and the orifice be thus brought within the pale of the dilating influence of uterine contractions." (p. 157.)

The object of his manipulation is to bring that portion of the uterine orifice which is prolapsed, more within range of the uterine action, so that it may dilate more equally. This, it appears to us, is much more rational than merely pushing back the anterior lip, in the expectation that it will glide over and above the head.

*Morbidly resisted action of the uterus*, or in other words *rigidity of the os uteri*, is another cause of delay fully treated by Dr. Millar. We shall not dwell upon his assumption that this rigidity is "inordinate" (that is spasmodic,) "contraction of all the fibres of the sphincter or cervix uteri," we must be contented with a simple dissent, and proceed with the question of treatment; and here we confess that Dr. Millar has left us little to find fault with. His theory and his practice are totally different things, and if the student contents himself with adhering to the latter in the case before us, he will conduct his patient very safely through this difficulty. We agree with Dr. Millar, that, "With regard to the treatment of rigidity of the os uteri, it must be observed that it should be vigorously applied at as early a period as possible, for the affection is apt to gather strength by continuance." (p. 161.) We also assent to his principle that the first and most successful remedy is bloodletting, which should be so copious as to make a decided impression on the circulatory system. Still more cordially do we support him in his prudent use of the lancet. We have heard of twenty, fifty, one hundred ounces of blood being taken in succes-

sive depletions to relieve this condition. Dr. Millar's cautions on this point are well worthy of attention :

"Should venesection disappoint the expectations of the practitioner, the question will arise as to the propriety of its repetition. It is a sound principle in obstetric practice, though lamentably disregarded by some, to be as economical as possible in the shedding of blood, lest in the progress of the labour, the further unavoidable loss of it should sink the patient below the point of recovery." (pp. 161-2.)

After depletion, Dr. Millar recommends the practice of Hamilton with respect to opium, and only alludes to tartarized antimony, of which he seems to have but little experience. We regret this, because we are satisfied that he would find it valuable in diminishing the necessity for large depletions. Dr. Millar has used stramonium on the same principle as belladonna, to dilate the os uteri.

"More than twenty years ago, before I was aware that belladonna had been recommended or used by any one in the case under consideration, I was led to make trial of the stramonium in a very obstinate case of rigidity of the os uteri. The case occurred in the country; having exhausted the usual resources to no purpose, and observing the stramonium to grow in great abundance about the premises, it struck me that it might possibly affect the os uteri in the same manner that it does the pupil of the eye. A strong ointment was accordingly prepared from the leaves of the plant, and freely applied to the os uteri, with the effect of rendering it less rigid and materially promoting its dilatation. Since that period I have occasionally used the stramonium and belladonna, but they have failed oftener than they have succeeded in procuring any marked relaxation of the os uteri." (p. 164.)

Thus far we have accompanied Dr. Millar in his treatment of rigid os uteri with much satisfaction; we have just stumbled, however, over a difficulty—another variety of digital assistance proposed by Dr. Millar, that obliges us to pause and examine more carefully the ground that we are treading with him. The treatment that we have alluded to has received the support of all practical writers; but that which we have now to consider raises "*the vexata questio*," which has produced such a tempestuous controversy. Dr. Millar observes :

"Should all the means above recommended fail, or but partially succeed in overcoming the rigidity of the os uteri, and *the cervix descend in advance of the head of the child*, it is necessary to raise and support the os uteri. As this is a measure of considerable importance, I shall endeavour to explain how it is to be practised. The index finger is to be applied just underneath the anterior lip of the os uteri, and with its edge or palmar surface pressure is to be made, in the interval of the pains, so as to push up the os uteri as high as possible, or *the extremities of two or three fingers may be used in the same way*. When a pain comes on, the tendency to descent is to be resisted, unless this be so strong as to require more force than it would be prudent to employ; in that case the fingers must gradually relax its counter-pressure and allow the descent to take place. But as soon as the pain goes off, the os uteri is to be pushed up again, and its descent is again to be resisted during the next pain. In this manner, acting with gentleness and caution, but at the same time with firmness and perseverance, the os uteri must be supported until it is sufficiently dilated to allow the head to execute its rotatory movements, and emerge from under the symphysis pubis." (pp. 165-6.)

Dr. Millar's object will be more intelligible, by quoting briefly his account of the effect of rigidity on the progress of labour :

"Besides acting as a barrier to the egress of the child, rigidity of the os uteri

hinders the head from executing the rotatory movement essential to its easy escape through the inferior aperture of the pelvis, and thus perpetuates itself almost indefinitely, especially where its treatment is not properly understood. To make this apparent to any one who comprehends the mechanism of labour, it is only necessary to observe that rigidity, though it may resist the passage of the head through the os uteri, cannot prevent it from descending in the pelvis, bearing the cervix before it. Urged by the propelling contractions of the uterus, the head does in fact descend to the bottom of the pelvis, and seems to be on the point of emerging from it; but arrived there, it can advance no further, even if the os uteri were to open, without previously undergoing rotation. . . . . The condition above described, viz., the head descending low in the pelvis and held by the cervix uteri as it were in a sling, is, as it seems to me, the ultimate effect of rigidity of the os uteri." (p. 160.)

Dr. Millar's intention is not to dilate the os uteri, but to prevent this descent of the womb into the pelvis :

"The finger is not used to stretch the os uteri, as many writers direct, but to hold it up that it may be dilated by the head, which can then be pushed, by the uterine contractions, lower than the level at which the os uteri is held. The head dilates the os uteri far better than the finger could, because it acts upon the whole extent of the cervix, whereas the finger could only act on the circle of the os uteri." (p. 166.)

It is perfectly clear that there is nothing like artificial dilatation in this ; every objection, therefore, raised against the practice on such an hypothesis falls to the ground. Dr. Millar is extremely careful to point out that it is the *head*, not the *finger*, which dilates the os uteri, and that such means should only be employed when all others fail : and knowing as we do how often in these obstinate cases the cervix becomes congested, when compressed between the head and the pelvis, that congestion leads to inflammation, and inflammation to increased rigidity, we are disposed to advocate Dr. Millar's suggestion as one of great practical utility ; at the same time we are very much inclined to doubt, whether even by this means Dr. Millar's object will be accomplished, of bringing this first stage to a conclusion within the limits he prescribes. These cases stand like *experimenta crucis* to test his and Hamilton's doctrines ; and unless he calculate in the manner to which we have already alluded, we are satisfied that the experienced obstetrician will agree with us, that even this method will not always succeed in bringing this stage to a conclusion in fourteen hours.

Having thus considered the different causes of delay in the first stage of labour, and Dr. Millar's method of relieving them, we cannot but regret that he is so busy with his fingers. We have endeavoured to point out where his practice may be of use, and where it is mischievous ; but, taken as a whole, we fear that the student thus instructed will forget the nice lines of discrimination drawn by Dr. Millar, and that, impressed with the value of digital manipulation, he will go much farther than Dr. Millar ever intended, and help to revive the abuses of by-gone days, when such artificial aid was the parent of infinite mischief. This result is more especially to be dreaded, when he is impressed with the conviction that by such means labour is to be abbreviated, and that as a general rule his fingers are the panacea for all cases of protraction in this stage.—THE TREATMENT OF THE SECOND STAGE of labour is also fully discussed by Dr. Millar ; but we can only allude to it briefly. He makes it "a fixed rule of practice, in all cases to rupture the membranes, when, the dilatation of the os uteri being completed, the pains become expulsive, or even in the absence of ex-

*pulsive pains, and with a view to excite them.*" (p. 182.) His object is, by removing the liquor amnii, to allow the *tonic* contraction of the uterus to commence before the delivery of the child, and thus to avoid the dangerous relaxation that has sometimes taken place in precipitate labours when this permanent contraction was absent. By this means also, the delay produced by toughness of the membranes is avoided. Dr. Millar combats Dr. F. Ramsbotham, on the one side, for opposing this practice, and Dewees, on the other, for carrying it too far. We agree with him fully in his objections to Dewees's rule of rupturing the membranes in all cases, even before the os uteri is dilated, and think that those who imitate him "will soon have reason to repent their rashness." It is hardly necessary to enter into his discussion with Ramsbotham, inasmuch as, when the os uteri is fully dilated, we believe the rupturing of the membranes, or letting them alone, almost a matter of taste; we are neither apprehensive of hæmorrhage, if we adopt Ramsbotham's rule, nor do we fear protraction if we follow Dr. Millar's. The great maxim of midwifery, "support the perinæum," is put prominently forward. Dr. Millar's rule is, "*to support the perinæum from the time it begins to be distended, until the child is completely expelled.*" (p. 184.) And his principle is that "The counter-pressure made by the hand should be so regulated as to promote, rather than retard, the advance of the child, which is accomplished by bearing with most force upon the posterior part of the perinæum, and pressing the presenting part towards the symphysis pubis." (p. 185.) We are well aware that by this mode of supporting the perinæum, the delivery of the child's head will be more rapidly accomplished; but we are not quite so sure that the integrity of the perinæum is so well preserved; there are many labours where the advance of the child must be retarded, if we would save it from laceration; and it seems to us that Dr. Millar's own experience gives us some support. "The primary object," he observes, "of giving this support, is to prevent laceration of the perinæum, *a slight degree of which, it has been already stated, unavoidably occurs in nearly all first labours.*" (p. 185.) We have not noticed such frequency of these slight lacerations in first labours, and therefore are inclined to suspect that this mode of pushing on the head had something to do with it. The causes of protraction in this stage are divided, as in the first, into—1. Inefficient action of the uterus. 2. Impotent action of the uterus. "To one or other of these states of the parturient organ may be referred every case of labour, retarded in the second stage, except such as are owing to malposition of the foetus or deformity of the pelvis." (p. 186.) In the treatment of inefficient action "the most signal good effects may be derived from bloodletting, especially where there is any undue excitement of the heart and arteries, as shown by the force and fulness of the pulse, by heat of surface, headache, thirst, &c. If the bowels are confined, a large dose of castor-oil, or a stimulating saline enema, will often succeed in arousing the uterus to more vigorous action. But the remedy which is most usually resorted to for this purpose, is *secale cornutum*, or *ergot.*" (p. 187.) Having laid down the requisites for the administration of this medicine, Dr. Millar makes some excellent remarks on its abuse. He truly remarks, that "As there is reason to believe that this powerful article is daily employed without such restraints, and that sad havoc is committed by it, the consideration of its *modus operandi*, with its necessary consequences, may serve to inspire us with

salutary caution in its use." (p. 188.) One of these ill consequences is the destruction of the child, which Dr. Millar attributes to compression of the child's brain by the too rapid moulding of the head to the parturient passage, where there is any disproportion or even unusual resistance in the soft parts; and in evidence of this effect, he quotes several authorities from a paper published by Dr. Beck in the 'Transactions of the Medical Society of the State of New York:'

"Dr. Ward, of New Jersey, whose experience with this article appears to have been extensive, and who speaks of it as a valuable agent in many cases, nevertheless admits the danger which attends the child from its use. 'In all cases,' he says, 'in which I have given it, unless the child was expelled very soon after the powerful contractions came on, it suffered very much, and would lie for some time without breathing.' Again, he says: 'From my own observations with regard to the ergot, I am led to conclude, that, in most cases, after giving it, unless the child is expelled in forty minutes after powerful contractions come on, it will be born dead.' The late Dr. William Moore, a veteran practitioner of obstetrics in this city, after detailing some cases, gives his opinion of ergot in the following terms: 'It appears to be injurious to the child at all times, for, *in every case* in which I have seen it exhibited, the child has been still-born, and, in the greater part of them, it was not possible to restore it to life.' Dr. Holcombe, of New Jersey, says: 'More children, I am satisfied, from what I have seen and heard, have already perished by the injudicious use of ergot, during the few years that have followed its introduction into this country, than have been sacrificed by the unwarrantable use of the crotchet for a century past.'" (pp. 189-90.)

Such is the American evidence of the effects of this popular drug. Dr. Millar further quotes the experience of Dr. Davies, of London, who lost four children, and a fifth was apparently still-born, in ten cases; Mr. Chavasse, of Birmingham, who states that, in eighteen cases in which the ergot was used, the children were still-born; Dr. Patterson, of Aberdeen, who mentions, that out of eight cases, in which he used ergot, "I lost three children, than which no stronger evidence need be adduced of its extreme danger." Dr. Millar might have added to this list two more equally strong authorities: Dr. Beatty, of Dublin, who published a valuable paper in the 'Dublin Journal,' 1844, and Dr. Hardy, who made the effects of *secale cornutum* the subject of his observations in the Dublin Lying-in Hospital, and found that, in forty-eight cases, where ergot of rye was given, thirty-four children were still-born—nearly three fourths.\* Again, with regard to the mother, Dr. Millar quotes two cases in which rupture of the uterus was the consequence; to which, if we mistake not, he might have added four similar cases that came under the observation of Dr. Bedford, of New York. We, therefore, give our fullest assent to his cautions respecting this medicine, and are inclined to doubt whether its introduction into practice has not been an injury rather than a benefit. We cannot, however, equally agree with him respecting his substitute for ergot; the fingers are again brought into play:

"The manipulations which I recommended in the first stage, may also be usefully employed in the second, with marked effect. The anterior margin of the os uteri, although it be sufficiently dilated to allow the foetus to pass, may be commonly felt behind the pubes, and the finger is to be insinuated between it and the presenting part of the child, in the absence of pain, and press it upward, performing at the same time semicircular movements, as already explained.... There is yet another

\* Dublin Journal, vol. xxvii, p. 224.



means of exciting more efficient action of the uterus, preferred by Madame La Chapelle, which I have often practised with good effect, viz., pressure upon the posterior part of the labia pudendi and anterior commissure of the perinæum, by two or three fingers introduced within the vulva." (pp. 197-8.)

We cannot perceive the advantage of this variety of digital manipulation in the case supposed. The action of the uterus is inefficient to advance the head of the child. It is dangerous to excite it by ergot of rye; the finger may be insinuated between the head and pubis, why then not assist with the forceps? Judiciously employed, it is attended with much less risk than the means here proposed, which Dr. Millar admits may be very much abused.

"To guard against the *abuse* of those manipulations, I may say of them, as of those recommended in the first stage, that they are not intended *to dilate*, but *to excite*.... Notwithstanding this explanation, I do not flatter myself that ignoramuses will not abuse this valuable resource, or that the ill-natured will cease to slander it by sneering at 'official irritation,' by prating about 'rubbing down the vagina,' 'stretching the vulva,' &c. Such we must ever leave to their folly and spleen." (p. 199.)

We are much more afraid of "ignoramuses," than of the ill-natured hornets that seem to have stung Dr. Millar; and for this reason must discountenance a practice attended with such a risk.

*Impotent action of the uterus* has been rather mystified by Dr. Millar. He says:

"The terms 'inefficient' and 'impotent' are not unfrequently used synonymously; it is therefore necessary that I should explain the sense in which I employ them. By the former I mean, 'inadequately exerted,' though the ability to act exists in full vigour: by the latter, 'inadequately exerted,' because the ability to act more vigorously is destroyed. The former is the 'inertie par torpeur;' the latter, the 'inertia par epuisement' of Madame La Chapelle." (p. 200.)

This definition is analogous to the "powerless labour of Dr. Churchill," who, however, very clearly defines what he means by the term.

"Women of *weak constitution*, especially in their first confinement, not unfrequently find the uterine powers fail after some hours' endurance, and that without our being able to restore them. These are the cases, and these only, in which there is anything to fear from a prolonged first stage; *for the exhaustion produced by it, and which in healthy women is of no consequence, may be the cause of inefficient (impotent) action in the second.* In women of an *irritable nervous* temperament there is also occasionally a failure of uterine powers in the second stage." (p. 208.)

The "inertia par epuisement" of Madame La Chapelle, the "powerless labour" of Churchill, is confounded by Dr. Millar with the effects that commencing inflammation produces in the uterus. He compares its condition to that produced in the voluntary muscles by over-fatigue. "Inflammation ending in suppuration is not unfrequently thus produced in the femoral muscles of soldiers after forced marches." (p. 201.) He then proceeds to describe the result of the long continuance or severity of the parturient contractions on the uterus:

"The uterus, as far as it can be examined by the finger, has lost its natural properties: the cervix, instead of being cool, moist, and pliant, feels hot, dry, and stiff—this stiffness differing altogether from the rigidity which arises from preternatural activity of its fibres, already noticed as a cause of protracted first stage. The stiffness of which I am speaking is connected with tumidity, and gives the impression of the tissue of the part being stuffed, so that its natural pliancy is destroyed.

Both the neck and body of the organ, as felt through the abdominal parietes, are sore to the touch, and the parturient contractions are in themselves painful, that is, while they last; the woman complains of suffering, diffused over the whole extent of the uterine globe, which is not much harder than during the intervals. These contractions have no effect as to the advancement of labour—the presenting part of the child remaining stationary, and becoming swelled on account of its being begirded by the rigid circle of the os uteri.” (p. 202.)

Dr. Millar here describes accurately inflammation extending through the uterus, and its consequence, suspension of its action; but unfortunately, having in his mind the effects of fatigue on the *voluntary* muscles, he assumes this to be an analogous state of the *involuntary* muscles. We must, *in limine*, dissent from this position; there is no such analogy between the two classes of muscles. If the involuntary fibres of the uterus are fatigued and lose their tone, the result is, high constitutional disturbance, hæmorrhage, &c., but not such symptoms as Dr. Millar has described: on the other hand, if inflammation extend into the uterus, these fibres *suspend* their action on the same principle that the fibres of the bladder or rectum do under similar circumstances, short spasmodic, very painful contractions taking the place of regular action. If we dissent from the principle Dr. Millar lays down, still more do we differ from the doctrine it is intended to maintain, viz., that this condition of the uterus is the result of not interfering in the first stage. “Of the causes,” says Dr. Millar, “that strike at the second stage through the first, I have already spoken, and need not repeat what has been said. But I may be allowed, I hope, without a breach of charity, to say here, that if the doctrine referred to be sound, the *indirect* causes of this class are all those expectant practitioners who consider it treason against nature to stir a finger in contravention of any of her vagaries in the first stage.” (p. 208.)

Now if there be one point more than another which experience has made known to us, and we would say, from the energy of their protests, to the highest authorities in the profession, it is a conclusion just the opposite of Dr. Millar's, that is, that there is no more fruitful means of producing this inflammation of the uterus, and thereby suspending its action, than constantly meddling with it; and so far from believing that the expectant practitioners are the indirect causes of this state of things, it is precisely the consequence that we would chiefly dread from Dr. Millar's digital manipulation. While we differ, however, from his doctrine, we must distinguish it from his practice in other respects, and we are happy to find him estimating the stethoscope at its proper value in these difficult cases; by its means the death of the child may be ascertained much earlier, and with more certainty, than in the more usual way. We also agree with him, that time is no criterion to govern our practice:

“The march of time is not uniform in its effects on labour, any more than it is on the persons of the sex; in the lapse of a given number of hours some women will be brought into a perilous condition, while others, under similar circumstances, as far as we can judge, will be in no manner of danger. In pursuing our opinion as to the necessity of artificial delivery, our attention should be directed to the evidences of impotency (*inflammation*); in proportion as these thicken, the necessity of delivery becomes more and more urgent. It is not wise to wait until the urgency is extreme; and, in general, the earlier the woman is relieved by delivery, the better, provided this can be done with facility and safety. Suppose, for example, the head of the child is presenting and has ceased to advance, while the

uterus has evidently become impotent; suppose moreover this head is within easy reach of the forceps, and can be delivered without risk or additional pain to the mother,—what would be the use of waiting until we are driven to the operation?" (p. 213.)

None, we believe, will question this, nor Dr. Millar's further instruction, that "If the delivery be not easily and safely practicable, prudence requires that it should be deferred until the necessity of it is more pressing, so pressing, that in our judgment, it is better to incur whatever risk the operation may involve, than wait longer." (p. 214.) Such in general terms, are the directions of Dr. Millar respecting instrumental aid; but it would have been desirable had he specified a little more precisely the cases in which the different instrumental operations are required. In his chapter on "*Instrumental delivery in vertex presentation*," we have general directions for the use of the forceps and for delivery by the crotchet, but we are not informed of the precise limit to each operation. His mode of delivery by the forceps is altogether of the French school. The patient is placed upon her back, and the French forceps is used. "It is proper, I should say, that the forceps I use is the French, curtailed by reducing the length of the handle and lock, which is not essential, except for delivering above the superior strait, an operation seldom performed in this country, and which I am persuaded is too frequently performed on the other side of the Atlantic." (p. 292.) We shall not dispute Dr. Millar's, which seem to be an American, preference for the French forceps, because we believe with him, "that a skilful operator may succeed very well with any of the instruments in use, and will succeed best with the one he is accustomed to handle." We the more willingly concede this point, because by his extremely judicious directions for the use of this instrument, he proves himself to be a skilful operator. His principle is address rather than force in the management of its introduction; a careful attention to the mechanism of the head's progress in assisting the delivery, and a regard to the natural process of labour.

"The duty of imitating the process of labour suggests, in the first place, that our extractive efforts should be made with intervals of rest. The artificial force ought not to be of longer duration than the natural, nor its respite shorter; it ought not to be put forth in all its intensity in the beginning, but be gradually augmented to the required degree, and we should act in concert with the pains, if they continue to recur with their wonted frequency, though with inadequate force. It suggests, in the second place, that we follow the mechanism of labour, by causing or assisting the head to perform whatever movements remain to be executed, in order that it may emerge easily and safely from the pelvis. When, finally, the head is made to distend the perinæum, our extractive efforts must be greatly mitigated, if not altogether intermitted, lest the perinæum be torn by its too hasty delivery. By some authors we are directed to take off the instrument at this stage of the operation, as the stronger pains and the more powerful efforts of the patient, which usually occur, are sufficient to insure the expulsion of the head; but my own practice is to leave it on, as its presence can do no harm, and it may be wanting. I have, indeed, usually continued to make slight tractions with one hand, while with the other the perinæum is supported until the head is born." (p. 297.)

With regard to delivery by the crotchet, Dr. Millar is equally careful that the mechanism of labour should be attended to, and ascribes to its neglect the tremendous difficulty experienced by some practitioners in the delivery of the head.

"I have listened to the recital of cases in which so much difficulty was encoun-

tered, that the operator's own strength and *weight* too were not sufficient to cause the head to *budge*, (I ask pardon of Noah Webster,) and he was compelled to call upon the midwife to lock her hands around his waist, and be clasped in like manner by a third person, in order that by 'a long pull, a strong pull, and a pull altogether,' the child might be brought to light." (p. 306.)

Dr. Millar quotes Smellie, Denman, Burns, Ramsbotham, Rigby, and Lee, to prove that they have neglected to give precise directions as to the exact part to the cranium to which the crotchet should be applied; and even Dewees does not escape, "who has undoubtedly more influence in shaping and regulating the practice of this country than all the British authorities whom we have quoted." Even Dewees only observes, that the point of the crotchet is to be fastened in *the nearest portions of bone*. Dr. Millar adopts the rule of Baudelocque, that "it is upon the occiput that the crotchet must be implanted when the head is the presenting part, and upon the superior maxillary bone, or the forehead, when we are obliged to use the instrument in cases of preternatural presentations, after the trunk is delivered." (p. 311.) Dr. Millar insists upon this point, as maintaining the head in the most favorable position for delivery, and remarks, "if the point of the instrument be infixed into the anterior part of the base of the skull, who does not perceive, that by drawing upon it, the opposite to nature's movement will be forced on the head? The extra flexion will be defeated, and a degree of extension be produced, bringing the occipito-frontal diameter down into the pelvis first; and if the mad attempt be persisted in, and brute force usurp the place of skill, subsequently the axis itself of the head." (p. 311.)

There is certainly a good deal of truth in these observations, but Dr. Millar must recollect that in such operations the occiput is not always so accommodating as to afford a sufficient purchase for the crotchet; the instrument will slip again and again, or break up the bone into fragments. What then is to be done? unless, indeed, we adopt another rule of Dr. Millar, of the effect of which we have some doubt; that is, to place the crotchet outside, and not inside the bone:

"I will only observe further, in concluding this chapter, that notwithstanding the marked preference, which most authors manifest, for applying the crotchet upon the interior of the skull, it is generally most advisable to apply it exteriorly, because we can thus *obtain the firmest* hold, and apply it more readily to such part of the head as may be demanded by its particular position, and also by what remains to be executed of its mechanism." (p. 312.)

We have no doubt about the ready application of the crotchet to the outside of the occiput, but a great misgiving that the hold will be found anything but a firm one.

The length to which this notice of Dr. Millar's work has extended, obliges us to omit his treatment of shoulder and nates presentations, hæmorrhages, retention of the placenta, and his physiological chapters on the fœtus and its appendages. We trust, however, that we have placed before our readers enough to estimate its value: that portion of the work which seems to be the chief object of its publication has been fully considered; sufficiently, we hope, to decide whether Dr. Millar has restored "the wreck of obstetric nomenclature" that he attributes to preceding authors, and whether he has successfully reduced to order "the hotch-potch" into which, according to him, "the fœtal presentations and positions

have been thrown." We have stated the conclusions at which we have arrived from an impartial examination of this portion of the work, and confess that it is almost the only part we could have wished that Dr. Millar had not attempted. When Dr. Millar gives us his experience, he states it in that bold and decided tone that must command our respect; he is evidently an intelligent observer in practice, and judicious in the conduct of his cases; but when he begins to theorise on what he observes, or rather when he gives himself up to the theory of others, and especially of the French school of midwifery, when he attempts to explain what he sees, he takes such a flight into the regions of fancy, that he loses sight of the truths from which he has started. Hence we find him plain, clear, and intelligible in his facts and descriptions, but confused when the explanation of those facts, or the reason of those appearances, is attempted. His account of a vertex presentation is clear and accurate, his explanation of its mechanism most fanciful; his description of labour, protracted from an arrest of uterine action, is a faithful account of inflammation and its effects, but he calls this *impotent* action of the uterus analogous to powerless labour, because he fancies that the uterus is fatigued like a voluntary muscle, and exhibits similar effects. We could have wished that Dr. Millar had adhered to that shrewd observation, which, in common with his countrymen, he largely possesses, and that he had not been quite so fascinated with the idea of erecting his standard for all to rally round, and sing the anthem, that—

"Westward the star of empire takes its way."

Apart from these, which we must call obscurities in his work, we have every reason to be satisfied with its general execution, and to congratulate the American press in having brought before us, so successfully, a work whose originality cannot be questioned.

## ART. VII.

*Du Mode d'Action des Eaux Minerales de Vichy, et de leurs Applications Therapeutiques.* Par CHARLES PETIT.—Paris, 1850.

*On the Mode of Action and the Therapeutical Applications of the Vichy Waters.* By CHARLES PETIT.—Paris, 1850. 8vo, pp. 500.

WE were somewhat surprised at finding M. Dumas, immediately after his accession to the ministry in 1849,—on the occasion of his recommending the establishment of *cliniques* at the principal localities in which Mineral Waters abound, to which certain advanced students might be sent for the purpose of studying their therapeutical effects,—complaining that their medicinal powers were very little appreciated in France by the profession at large. Our acquaintance with French and German writings had led us to believe, on the contrary, that mineral waters are very highly appreciated on the continent, and are recommended with far more frequency and confidence, than is the case here; for both they and *ptisanes* prepared from indigenous and fragrant herbs, are ordered in very many cases in which it is usual here to prescribe more costly and more nauseous drugs, our addiction to the use of which is regarded abroad as one of the curious and incomprehensible Anglican peculiarities. M. Dumas has, however, in all probability regarded the subject solely from



the point of view suggested by the department of science in which he has achieved such eminence, considering the animal body too much as a mere crucible, in which the same reactions and changes may be effected as are operable in the laboratory of the Chemist; and to one actuated by such expectations, the results hitherto obtained, or probably ever obtainable, from the medicinal employment of mineral waters, must be disappointing. M. Petit is also somewhat imbued with these views, for although he has had the advantage, from his office of assistant-inspector at Vichy, of long practical study of the effects of the mineral waters of that place, and is evidently desirous of furnishing an impartial account of these, yet he expresses too exaggerated an expectation of what may yet be done, and too favorable a view of what has been done, to meet with general assent. Bearing this in mind, there is, however, in the work before us, still ample evidence of the great utility of these mineral waters in a variety of diseases. If the medical public is not already aware of this, it has been through no fault of M. Petit's; for on each disease to which the waters are considered as especially applicable, he has already published *brochures*, a collection of which, indeed, with some amplification, constitutes the bulk of the present volume; and never did knight-errant do battle against all gainsayers of his selected heroine, more courageously than has M. Petit against those who have ventured to call into question any of the numerous virtues he attributes to his favorite springs.

Whatever medicinal power the Vichy waters really do possess, is chiefly due to their strong *alkaline* properties; for although recent analyses of M. Henry show that they contain minute quantities of iodine, bromine, and a variety of other substances, yet *bicarbonate of soda* is by far the most prominent ingredient—about 75 grains of it being contained in  $1\frac{3}{4}$  imperial pint. That the other constituents exert adjuvatory or modifying effects, there can be no doubt; for the same quantity of soda substituted for that in the Vichy waters neither leads to the same results, nor is borne with the same ease; and it is in part to these effects that M. Petit attributes the fact, that he has never met with, even from the abundant use of these waters, that deterioration of constitutional power, stated by M. Trousseau to result from an excessive use of alkalies, and manifesting itself in pallor of countenance, œdematous swellings, and passive hæmorrhages. On the contrary, he has found the appetite increase, the functions of nutrition become more actively performed, and the general tone of the system improved. Even the waters themselves, transported to a distance, have not the same efficacy as when drunk at the source; which probably arises not only from the absence of other concomitant circumstances, such as a purer air, more exact regimen, &c., but also from a less quantity being borne, owing to the absence of the free carbonic acid, which renders their imbibition more grateful. In truth, the quantity borne at the spring is represented as being sometimes enormous; and although M. Petit is content if he finds the effects result from two or three glasses a day, yet, believing that all the benefit that is to accrue must be produced by what he calls “saturating” the system by the alkali (as ascertained by the addition of the urine), and the keeping up this saturation, he does not hesitate to order from 20 to 25 glasses per diem, if requisite to this end. Other practitioners complain of not being able to get their patients to take such quantities; but, on the contrary, he says he often finds *his* exceeding the

dose he has ordered, and drinking 40 or 50, and in one case 80 glasses (each containing 15 grains of soda) *per diem*, and that with advantage. In other cases, however, he has found that only small doses could be borne, the urine even becoming acid under larger ones; and when febrile action is present, small ones are alone proper. Another efficacious and rapid mode of impregnating the economy, is by the employment of the waters in baths, which are generally employed conjointly with their internal use. M. Petit is particular in impressing upon his readers the necessity of great perseverance, if good is to be achieved in chronic affections by these waters. The course need not be continued above a month or so, but it will in many cases require repeating for one or more seasons, while in the intervals, the bottled waters, or their equivalent in soda, will be occasionally required. All observers are, however, not agreed with M. Petit, either as to the possibility or the desirableness of keeping up this alkaline condition of the humours; and M. Durand-Fardel,\* who has had ample means of studying the effects of the waters at Vichy, is at complete issue with him. He found that these waters speedily removed acidity from the urine, but only induced an alkaline state of it in a certain number of cases, while in some of these, under a variety of circumstances, *e.g.*, the occurrence of diarrhoea, the urine speedily became acid again. He declares, too, that all the beneficial effects of the waters are producible by moderate doses, and are by no means proportioned to the changes induced in the urine.

In treating of the mode of action of the waters, although admitting they exert an effect upon the vitality of the organs in common with other mineral waters, yet M. Petit almost exclusively dwells upon their chemical operation. To this end he seeks for evidence on all sides, and frequently not in the exactest manner, as to the great agency which abnormal acidification exerts in inducing disease. The alkali of the water corrects this, and by its fluidifying, antiplastic, and deobstruent power, accelerates the circulation, and dissolves deposits of fibrin and albumen, if of not too old a date. It is, from this effect on the circulation, unfitting in cases where diseases of the circulatory or respiratory organs prevail. In other cases the waters seem to operate by a slower, yet no less sure effect, and then may be considered as alteratives. A primary condition for their useful employment is, that all the acute symptoms of disease shall have subsided, and especially febrile action; but the condition of chronicity once secured, they will prove operative in proportion to the shortness of time that has elapsed before their employment; and if they could be oftener had recourse to, as soon as acute disease has subsided, we should, in M. Petit's opinion, see much seldomer than we now do examples of incurable affections.

We will now briefly enumerate the affections, for which M. Petit considers the Vichy waters especially applicable. A condition of *engorgement* or hypertrophy of structure, whether implicating only mucous membrane, as in some forms of dyspepsia, or the parenchymatous tissue of organs, as in cases of chronic disease of the liver, spleen, womb or ovary, seems especially amenable to their employment; reductions of size of these enlarged organs being even sometimes effected at a far remoter period than could be hoped for, and generally occurring when the waters are resorted to early, and perseveringly continued. Scrofulous subjects, too, under their use,

\* See Brit. and For. Med.-Chir. Rev., Vol. V, p. 555.

acquire vigour of constitution, any ulcerations or inflammations that may be present undergoing a favorable modification. In young subjects these results are of rapid occurrence ; but in older ones, suffering from indurated glandular swellings of long date, they are of very slow production. Chlorosis, too, is a disease remarkably benefited, but only after long perseverance.

The Vichy waters have long enjoyed a great reputation for the relief of the uric acid deposit, commonly called *gravel*, as might be anticipated from their composition ; but M. Petit claims for them also the power of dissolving *urinary calculi* in the bladder, even after these have reached a considerable size ; and this not only with respect to the uric acid calculi, but also the triple phosphate. In fact, the ascertaining the exact composition of the calculus, often a matter of difficulty, becomes, in his view, a secondary consideration ; as the waters do not act so much by direct chemical operation upon the mineral material of which the calculus is composed, as by dissolving the connecting mucus which holds its particles together, and thus disintegrating it. Warmly as M. Petit espouses this view, we fear his evidence in its favour is defective. It may be quite true, that calculi placed in the Vichy waters out of the body, become thus disintegrated ; but the possibility or safety of keeping up an intensely alkaline state of the urine long enough to effect this, may indeed be questioned. M. Petit has adduced a few cases in which calculi once felt disappeared after a course of the waters ; but, perhaps, there is no solvent of the stone on record that has not been recommended by certain cures, which have eventually proved to be no cures at all ; and M. Petit himself informs us, that so great is the ease procured by the action of the alkaline water on the irritated bladder, that the patients consider themselves cured long before the time necessary for this. It is, moreover, very strange, that in regard to a remedy of so powerful a character in a disease of such severity and frequency as stone, the author is enabled only to reprint cases which had already been presented to the Academy of Medicine twelve years ago, and had been there received with well-founded scepticism—no new example of its efficacy having presented itself for so long a period of time !

The evidence adduced in favour of the utility of the waters in *gout and chronic rheumatism* is much stronger, being founded on no less than 80 cases, which were rigidly examined by a committee of the Academy as to the results in 1840, and a large additional number that have since been under the author's care, and the particulars of 89 of which he published in 1842. Although, according to a recent statement that no present member of the Court of Aldermen is a victim of it, gout threatens to become a traditionary disease among us, enough of it yet remains to render the statement of the great success of the Vichy waters in combating it interesting. It appears, then, that of the 169 cases reported upon to the Academy by M. Patissier, or already published by M. Petit, 75 were examples of acquired, and 94 of hereditary gout, and in 67 the affection was accompanied by gravel. In 44 there had been no return of the affection for at least two years ; in 110 the disease was most materially mitigated as regards the severity and frequency of the attacks, these, too, being easily mastered by the renewed use of alkalies ; and in 15 the waters had been, or seemed to have been, injurious. Some additional cases are detailed in the

present work, and their perusal impresses us with the conviction, that the employment of these waters is a means far superior to any other known, not merely for the relief of the attack itself, but for destroying the disposition to it, and an infinitely safer one than the resorting to colchicum. To avail himself fully of it, however, the gouty patient must be content to submit to an abstemious regimen, and to a persevering use of the waters for more than one season, taking alkaline drinks also during the intervals while at home. We fear that perseverance in abstemious living will be found to be the most difficult point of all.

In regard to the diseases we have already enumerated, M. Petit's work is only a republication of former statements, confirmed by additional experience; but he also now brings forward some evidence to exhibit the utility of the waters in *diabetes mellitus*. He relates eleven cases in which they were exhibited with apparently a curative effect; but as most of them are of very recent occurrence, it is perhaps too early definitively to pronounce upon this in a disease so liable to relapse. Still, in connection with an animalized regimen and exclusion of feculent and saccharine substances, he believes this the best means as yet devised, for cases of not very old standing.

We have extended our notice of the work to this length, because we think that M. Petit, though guilty of some exaggeration, has been desirous of giving a faithful account of his favorite waters, and has proved them to be of considerable utility in several obstinate forms of chronic disease; and because, with our present facilities of travelling, they are nearly as accessible to our countrymen as to his own; while in their bottled state they are easily obtainable here, at least by those who can afford them, and who are prevented by circumstances from resorting to the source. Whether M. Dumas is right or not, in regard to his charge of a want of due appreciation of the medicinal power of mineral waters by the profession at large in France, we feel assured that in England it holds good.

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#### ART. VIII.

*Iagltagelser anstillede under Maeslinge-Epidemien paa Faeroerne i Aaret 1846.* Af Cand. Med. & Chir. P. L. PANUM.

*Observations on the Epidemic of Measles in the Færoe Isles, in 1846.* By P. L. PANUM, Candidate of Medicine and Surgery. (From the 'Bibliothek for Lægr.')—*Copenhagen*, 1847. 8vo, pp. 74.

SANITARY reports and sanitary measures are now the order of the day. The public are at length opening their eyes to the importance of anticipating the ravages of disease, by adopting every means for promoting cleanliness both of person and of dwellings; and though we admit that, in some instances, the influence of a dirty skin, or of an unswept or undrained house, has been overrated as to the immediate production of disease, still this excess of caution must be regarded as an error (if it be one) on the right side of the question. In the Tenth Number of this Journal, we gave an account of an interesting report by Dr. Schleisner on the Sanitary Condition of Iceland; and we can now present our readers with a not less curious document, a Sanitary Report on the Condition of

the F  roe Isles. Perhaps of all parts of Europe, save the northern coast of Russia, the F  roe Isles are the least known and the least visited by English travellers. They present no attractions of scenery to the tourist, no antiquities or learned institutions for the scholar, and the adventurous naturalist passes them by on his voyage to Iceland, where he looks for a more extended field for his researches. But it is this almost total isolation from the rest of the world, which gives to the facts and observations here detailed their value and importance in the eyes of science. The extreme rarity of intercourse with foreign nations, the restrictions most unwisely imposed by the Danish government upon the commerce of these islands, all tend to promote that perfect isolation, which may almost never be attained in more southern lands, for the study of epidemic disease. Mr. Panum was sent out by the Danish government in 1846, to F  roe, on the first outbreak of an epidemic of measles ; and he remained for five months in the islands, living among the inhabitants, and travelling over almost every part, with the view of obtaining a satisfactory knowledge of the actual condition of the F  roese peasant. His report is divided into two sections ; the first, and by much the longer, is devoted to the sanitary state of the island, and contains matter of the highest interest ; while the second part refers solely to the late epidemic of measles, and chiefly, too, to the question of the contagious or epidemic nature of that malady.

The F  roe Islands lie in north latitude  $61^{\circ} 30'$ — $62^{\circ} 30'$ , thus extending nearly a degree from north to south. None of the isles are of great size, but the hills rise to the height of nearly 3000 feet, and the intervening channels are narrow, bounded by high rocks, and often extremely picturesque.

The medium temperature in summer is  $8^{\circ}$  of Reaumur ( $50^{\circ}$  F.); but twice during our author's residence there, it rose to  $16^{\circ}$  and  $17^{\circ}$  R. ( $70^{\circ}$  F), which to the inhabitants, and foreigners too, seemed almost a tropical heat. In winter the cold is not severe ; but heavy masses of snow often accumulate on the hills, and cause much destruction by falling as avalanches into the narrow valleys, where the houses are built. Fearful storms of wind repeatedly occur, so that the traveller, if he chance to be out upon the hills at such time, must lie down to escape being blown bodily over the rocks. But the most remarkable characteristic of the island is its excessive humidity ; the constant mists that envelope the mountain-tops are rarely dissipated, so that the green grass which clothes the slopes is constantly wet, and innumerable springs and rivulets course down the hill sides. The air is constantly loaded with salt vapour from the sea, and it is this, our author thinks, which prevents the growth of trees, more than the coldness of the climate. We believe, however, from what we have seen in the Shetland Isles, that the wind is the chief obstacle to the growth of trees, as where protected they will still flourish. We have not space for our author's graphic description of the stern features of the F  roese landscape, to the influence of which he, in part, attributes the great frequency of mental disease in those islands.

The raw and cold climate, as may be expected, is a frequent cause of rheumatic affections. The F  roe peasant is, generally, perhaps more from necessity than from choice, a temperate man, and his frequent attacks of rheumatism cannot, as often in this country, be ascribed to the influence of alcoholic drinks alone. The usual consequences of rheumatism are



likewise common, as heart-diseases and affections of the joints. Our author ascribes the great frequency of rheumatic affections in Färoe to the insufficient covering, especially for the feet, that is adopted by the peasantry. They wear long stockings, and a species of sandal sewed together out of a piece of sheepskin: in Shetland these are called "rivlins," and form, as we can say from experience, most excellent shoes for travelling over the soft, peaty surface of those islands, but so little durable that they are often worn out in a single day. Their feet therefore are constantly wet, and they rarely think of changing their shoes or stockings when they come home.

Chronic affections of the bronchial mucous membrane are likewise common, and chronic bronchitis is perhaps the most frequent disease in Färoe. Our author believes that this malady is frequently aggravated by the salt particles always present in the atmosphere. Death from chronic bronchitis seems to be the most usual termination of life in old people in Färoe, and often, too, hectic symptoms supervene before the final catastrophe, so that such deaths have been ascribed to tubercular disease.

"It is true that scrofula, in its various forms, is occasionally seen in the Färoe Isles, nor is its congener tubercular disease entirely unknown, but both are, at all events, rare maladies, for though I auscultated many hundred patients during my residence on the isles, I met with only two instances where I could satisfactorily diagnose phthisis. On the other hand I have seen many patients who had become hectic in consequence of chronic bronchitis, as far at least as I could judge from the stethoscopic sounds and from the history and symptoms, but I could never attain to absolute certainty, as *post-mortem* examinations were resolutely refused. Intermittent fever is unknown in Färoe; according therefore to Boudin's theory, phthisis ought to be a frequent disorder."

Our author, however, carefully distinguishes those cases in which the bronchitis was the consequence of measles. Of pneumonia he saw little; only one case presented the glutinous expectoration, out of which, by washing, he obtained the fibrinous branched bronchial coagula of Remak.

Disorders of the menstrual functions in women seem to be common in these isles. No doubt they are augmented, and often caused, by the younger females getting their feet constantly wet, when going to milk the cows upon the upper pastures; but, at other times, the females in Färoe lead an exceedingly sedentary life, in habitations by no means, as we shall presently see, calculated to improve their health. Of skin diseases, urticaria and psoriasis are the most frequent; the latter is especially common on Norderoe, where the inhabitants are unceasingly occupied upon the water.

The generally received doctrine, that mental disorders are the more frequent in a country the further it is advanced in civilization, is not corroborated by the observations of our author in Färoe. There is hardly a land, nay perhaps scarcely a metropolis, where mental disease is so frequent relatively to the population, as in Färoe. Mr. Panum has not as yet collected all the necessary data on this point; but he is prepared to assert, that mental disorder occurs in at least one per cent. of the whole population of Färoe. Yet in general the mental capability of the peasant of these isles is above the average:

"There is a remarkable similarity in the form of the disorder. With many we observed a quiet form of religious delusion, where the affected individuals saw frequent visions, and believed themselves to be in direct communication with the

spiritual world, with Christ and the Holy Ghost, while still more believed themselves to be possessed by an evil demon, who constantly forced them to act against their own better judgment, causing them to destroy the articles near them, and to strike the bystanders during their fits of fury, and then they subsequently sank into a melancholy irritable condition. Both of these forms seems often to pass on to fatuity."

Our author thinks that the prevalence of insanity is much favoured by the general gloomy character of the F  roe landscape, by the constant mists that hang on the mountain tops, and the perpetual struggle with the stormy ocean and inclement climate. We must pass over the curious allusions to the superstitious and exalted religious ideas of the F  roese, as being only remotely concerned in the increase of mental disorders.

The F  roe peasant is generally a temperate man; but on festive occasions, large quantities of brandy, the strength of which has not been improved by commerce, are taken almost without limit. Confirmed dram-drinkers are rare; few can afford it; but our author thinks that the greater part of the confirmed toppers end their lives insane.

We pass by other interesting details, to devote a little more space to the singular diet of these islanders. The only bread that the F  roe peasant eats is a coarse barley, which is made into long cakes or rolls without any yeast, and is baked in the hot ashes of a peat fire. Mr. Panum seems to look on these cakes, or *drujl*, as they are there called, with disgust; but in the Shetland isles a similar mode of preparing barley bread is used, and we have eaten it with relish, and found it by no means unpalatable. *Drujl* and sour milk with "*skj  rpe kj  d*" form the staple of a F  roese breakfast. The latter is mutton, dried in the wind, (in Shetland "*skeo*," dried mutton.) The sheep are driven in and slaughtered in autumn; the skin is taken off, and the carcase, after being embowelled, is hung up without further preparation, to dry in an outhouse. Throughout the winter it undergoes, according to the state of the weather, a greater or less degree of decomposition, and in the spring is regarded as fit to eat; and they accordingly consume it, cut in small pieces, and without any further preparation. In summer the remainder of the carcase becomes filled with maggots, but these only increase the "game flavour" of the meat to the natives. At dinner "*rast*," that is, half-decomposed flesh, fish, or fowl, is almost invariably eaten. This must not be confounded with the wind-dried mutton before spoken of; as the flesh of the *ca'ing* whale, *delphinus deductor*, or of any of the numerous sea fowl that inhabit the coast, or the fish that swarm in its bays, may all, when undried or unsalted, come under the denomination of "*rast*:"

"During the interval of many months that the flesh, fish, or fowl, is neither fresh, nor yet wind-dried, it is called '*rast*,' a word which I can only translate by half-rotten. This appellation it fully deserves from the horrible smell that it sends forth, from its mouldy aspect, and the numerous maggots that swarm upon it. I have seen a boat's crew of eight men, eating with great relish the raw flesh of the *ca'ing* whale, though it was so decomposed that the smell of it was disagreeable to me even in an open boat, and the bottom of the boat was almost white with the maggots that fell from the decaying mass."

Large quantities of the salted blubber of the *ca'ing* whale are likewise eaten in F  roe, and it is especially prized as a relish at supper, which is their principal meal. The peeled stalks of the *angelica archangelica* are constantly eaten, as we eat strawberries, with sugar and cream.

We may well suspect that dyspepsia and cardialgia are not unfrequent results of such diet ; but still more frequently does the rotten-flesh occasion diarrhoea, a symptom of annual occurrence on the bird islands, such as Videroe, and also invariably observed after a large take of whales, when much of the flesh of these animals necessarily becomes "rast" before it is consumed.

A diet like that above described is ill suited for the most healthy digestion, still less would it be appropriate to individuals labouring under any disease ; yet during the prevalence of measles, the poor peasant could do little more than consume his little stock of coarse barley, and that finished, he must return to his usual indigestible diet. A most exhausting diarrhoea was a frequent sequel of the attack of measles ; it often continued for months, and our author thinks it probable, from the slimy nature and ochre-yellow colour of the stools, that the well-known peculiar alteration had taken place in the glands of Peyer, from the irritating and unsuitable nature of the diet.

Hæmorrhoids, plethora, and the lithic acid diathesis, are likewise frequent in Færoe, but the teeth of the natives are often, till an extreme old age, complete, and of the most pearly whiteness. Our author often saw men of 70 years of age, who had never lost a tooth, but in these cases the crowns of the teeth were worn away quite flat, as in old British skulls.

Notwithstanding all these disadvantages of food and climate, the natives of these islands attain a great age ; the average duration of life, which in Denmark is 36 years, being in Færoe 44½ years, *including* the still-born children.

A curious account is next given of the Færoese houses, which seem scarcely to have altered in the least since the days of the early Norse adventurers. A sanitary commission is much wanted for the reform of these dwellings, as well as of the kindred habitations we have so often seen in Shetland. The constant peat smoke with which the chambers are filled, necessarily causes chronic inflammation of the conjunctiva and its consequences. In such unwholesome localities, where the walls, roof, and floor are formed of turf mixed with stones, and constantly damp, we might expect, too, to meet with Bright's disease of the kidney ; but our author assures us, that though he repeatedly saw old people with œdematous legs, yet that their urine, when tested, never once showed any signs of albumen, while, on a further examination, he almost invariably detected disease of the heart.

We pass on to the valuable statistical information supplied by Mr. Panum, regarding the ratio of mortality, its proportion to the births, and the increase or decrease of the population of Færoe. In 1845 the islands contained 7782 inhabitants. During the last 70 years the population has been steadily on the increase, for no emigration is here known, the Færoe man, unlike the Iclander, rarely even visiting Denmark or Norway. In 1834 the population of the islands was 6928, so that between that time and 1845, there has been a yearly increase of 1.05 per cent., while in Denmark the increase during this time has been only 0.83 per cent. The mortality of Færoe is likewise much less than that of Denmark.

The mortality of children under 10 years of age is in Denmark 366 per 1000, in Færoe it is 279 in 1000 ; and while the greatest number of deaths in the former country occur between the 60th and 70th year (exclusive of children under 10 years of age), we find that in Færoe the highest mortality

takes place between the 80th and 90th year. In Denmark, out of 1000 deaths, only 187 are above 70 years of age; in Färoe, among the same number of individuals, 349 have passed that usual limit of man's age. During the period of middle life, the relative mortality is greater in these islands than in Denmark, but this is no doubt chiefly the result of accidents on the rocks, or from deaths by drowning in the dangerous floods and whirlpools of the coasts. Out of 172 deaths recorded upon Norderoe, one of the Färoe group, between the years 1835 and 1845, not less than 20 men in the vigour of their age are recorded to have died from accidents.

Our author then passes carefully in review the various causes that have been assigned for this very favorable ratio of mortality, and he concludes that it is chiefly to be ascribed to "the exemption, partial or complete, enjoyed by the inhabitants of Färoe, from a number of diseases, and particularly from contagious disorders, which are the scourges of other lands."

To illustrate this assertion, he briefly enumerates the various disorders, which, frequent in other countries, are of rare occurrence in Färoe. Scrofula or tubercular disease, (our author, with many pathologists of the present day, not venturing to separate the two maladies,) is, in comparison to the number of the inhabitants, a rare disease in Färoe, in spite of the raw cold climate, and the privations endured by the inhabitants. The exemption from this malady, too, will explain, perhaps, the very favorable ratio of mortality among children under ten years of age in Färoe. The highly animalised diet we have above described may also tend to oppose the progress of scrofula; and lastly, syphilis, by so many believed to be the most fruitful parent of this malady, was, until the last few years, a disease almost unknown.

Cancer, either hard or soft, seems to be also rare in Färoe; our author neither saw any cases, nor heard of them, as having previously occurred.

Bright's disease of the kidneys, as before noticed, was never met with, though oedema of the legs was common, as a consequence of heart-disease.

Intermittent fever is unknown in these islands.

Among contagious disorders, the Färoe peasant suffers chiefly from scabies, influenza, typhus, and within the last few years, from syphilis.

Of the first, nothing need be said; but influenza "*Krujm*" is a frequent, and often here a very serious malady. In 1838 an epidemic of influenza occurred in Färoe, which increased the average mortality of these islands from 96 to 160 deaths. Here, as in the island of St. Kilda, the outbreak of these epidemics of influenza is universally ascribed to the advent of strangers, or to that of the trading vessels from Copenhagen, which arrive in the spring. Our author remarks that this opinion is not without some foundation, for the arrival of the first trading vessel is by no means at any fixed period; it may come in in March, or in April, or in May; and yet he was assured by Governor Plöyen, who, for the last seventeen years, has been a resident on the island, that within two or three days after the first ship's arrival in spring, an epidemic of this kind has invariably appeared. It first attacked the merchants, their clerks and servants, then spread through the town of Thorshavn, where the ship lay, and subsequently extended itself over the whole island.

It has been long supposed that the "*Landfarsött*" of Färoe was a malady *sui generis*. But our author had an opportunity of observing an epidemic

of this dreaded malady, shortly before his departure from the islands, and he pronounces it to be distinctly a typhoid fever, without a single unusual or anomalous symptom. Petechiæ were, however, not common, as he only saw them in three cases; but the lenticular rose-coloured spots of typhoid fever occurred on the arms, breast, and legs of one half of the patients. This, we must say, is somewhat unusual, for the "*tâches roses lenticulaires*" of the French are always most frequent on the abdomen, and are usually first seen on that region. The malady was evidently contagious, and those who observed a strict seclusion from the infected, invariably escaped.

Scarlatina has never been known in Færoe. Smallpox raged here for the last time in 1705, and then with such fury, that the inhabitants of one island were entirely swept away. Vaccination is, however, but imperfectly performed; and smallpox, therefore, may again, if introduced, cause great ravages. From the year 1781 to 1846, measles were unknown in these islands. Out of 7782 inhabitants of Færoe, nearly 6000 were in this latter year affected with this malady, within the space of six months. The first case showed itself, on the 4th or 5th of April, in Thorshavn, and the last was observed in Sandoe on the 17th of September of that year. From the beginning of the year 1846 to the middle of September, there were in all 255 deaths, of which number at least 102 were traced to this disorder. In Denmark, the annual mortality, in relation to the population, is 1 : 41·22; in Færoe it is 1 : 64·66; but in the first two thirds of the year 1846, the mortality in the latter islands was 1 : 31·107. From a carefully constructed table, given by Mr. Panum, we learn that the epidemic was very fatal to infants at the breast, but that it did not greatly augment the ratio of mortality from thence up to the twentieth year. From the thirtieth up to the sixtieth year it increased, till the mortality at the latter age was *five times* greater than ordinary. After the sixtieth year it again rapidly diminished; not because the malady was less dangerous for persons above that age, but because, as our author believes, it was only sixty-five years since the same disorder had prevailed in Færoe, and those now elderly people who then had the disorder, entirely escaped the present epidemic.

"The effect which the epidemic of measles in 1846 had upon the ratio of mortality in Færoe, may serve as a well-authenticated example of the influence which epidemics in general exercise upon the population of a country. Under ordinary circumstances, measles affect only children, while, in Færoe, they attacked individuals of all ages, almost without exception. At the same time it may be observed, that those epidemics which desolate other lands, and are wholly or partially unknown in Færoe, exhibit the same character of being nearly uninfluenced by age. I trust to have shown that the principal cause of the favorable ratio of mortality in Færoe must be sought for in the isolated condition and restricted commercial intercourse of these islands, which protects them from many diseases prevalent in other countries, as, for example, in Denmark, where their constant presence materially increases the annual deaths among the population. It is therefore evident, that prophylactic measures against the introduction and spread of foreign diseases are of vast importance in such countries, where they can be rigorously and satisfactorily adopted, as is peculiarly the case in Færoe; while the same regulations would be useless in a large commercial country or city, with constant intercourse from surrounding nations, as, for example, in Copenhagen. A strict quarantine against measles sounds somewhat absurd; but, had such been put in practice in 1846, we firmly believe that 100 valuable lives would not have been sacrificed in Færoe. Till



within a very few years, an ordinance of this kind, directed against measles, did really exist in F  roe, but it was unhappily rescinded."

The second part of our author's communication is confined solely to the examination of the nature of the contagion of measles, as he believes himself to have been placed in peculiarly favorable circumstances for conducting such observation with accuracy.

The question of the period of incubation of the disease, after a party has been exposed to the contagion, has, he thinks, never been satisfactorily settled. Some authors contend for an eight days' period of incubation, others for ten, others for fourteen, while others, again, do not admit of any distinct or settled time. By English writers, the period at which measles will appear, after an individual has been exposed to contagion, has been variously estimated; but the majority, while they differ as to the fewest number of days that may elapse before the disease shows itself, are almost unanimous in fixing the most extended period at fourteen days. In a populous country like England, and, indeed, throughout Europe, no certain results can be arrived at, on account of the great difficulty, if not the absolute impossibility, of preserving the requisite degree of isolation; but in F  roe, separated as the inhabitants are, not only by high mountains, but by dangerous and deep fiords and inlets, a degree of accuracy can be attained, not to be hoped for in other lands. The dwellings of the F  roese peasants are widely separated from each other, communication is rare, and a visit to a neighbour is an event to be long talked of before and after it has occurred. We will now follow our author briefly through his valuable investigations on this obscure point:

"The first case of measles that appeared in F  roe, in 1846, was in the person of a carpenter of Thorshavn. This man left Copenhagen on the 20th of March, and reached Thorshavn on the 28th, having been, during the voyage, in perfect health. He landed, and in the first days of April—he does not recollect on what day—was attacked with measles. About fourteen days after this, his two fellow-workmen were seized with the same disorder.

"On my arrival in F  roe, I proceeded, on the 2d of July, to Tj  rnevig, in Nordstr  moe, where 80 out of 100 inhabitants lay sick of measles. On the 14th of June, a boat with ten men from Tj  rnevig had assisted at a take of whales (Grindefangst) in Westmannhavn, and *exactly fourteen days after*, on the 18th of July, the exanthema broke out on all these ten men, without exception, after they had, for the two or three previous days, complained of cough and smarting of the eyes. These ten men had been nowhere in each other's company, except at the said take of whales, nor had any one of them been in any locality where they could have been exposed to the contagion, which they all dreaded and sedulously avoided. But in Westmannhavn, they were not only in communication with people who had recently recovered from measles, but likewise for a considerable time remained in a house where several people, the day after, took to their beds, affected with the disorder. Twelve to sixteen days after these ten men had taken measles (reckoning from the appearance of the exanthema), the disease broke out among almost all the other inhabitants of Tj  rnevig, with the exception of a very few, who were not affected till another period of from twelve to sixteen days had elapsed.

"From this we may infer, that the contagion of measles does not cause any disturbance of the general health for the first ten or twelve days after it has been received into the body; the catarrhal symptoms then first show themselves; and after that, on the fourteenth day, the eruption appears. Should this surmise prove correct, we may likewise infer, from the fact of the second or third invasion of the disease occurring with an interval of nearly fourteen days between each, that

measles are most contagious in the eruptive stage, and not, as has generally been supposed, in that of subsidence or desquamation."

We give the above as a specimen of Mr. Panum's careful and diligent investigations, and of their results. He pursued his researches throughout the whole of the islands, and has recorded not less than fifty-two localities where similar investigations were made; and among these he only met with two apparent exceptions to the rule above laid down. Both these exceptions, however, subsequently proved to have been founded on erroneous data, supplied by the patients themselves, who afterwards, of their own accord, came forward to correct their statements. Indeed, so unvarying did our author find this rule to be, that in Fuglefiord on Osteroe he narrowly escaped obtaining the evil reputation of a wizard:

"At my first arrival in this spot, the daughter of John Hansen, the churchwarden, had just had measles, but had recovered, and now, with the exception of a slight cough, felt herself perfectly well. All the other nine persons in the house were entirely without ailment, and expressed their hopes of altogether escaping the malady. I inquired upon what day the exanthema first showed itself in the above-named patient, and, asking for the almanac, placed my finger on the fourteenth day after the period above referred to, with the sole remark, that they should make a black stroke under that day, for on that day I feared that measles might break out among the rest of the household; and that, should such not be the case, there would be good hope that they might entirely escape. As it happened, ten days after, I was again sent for to Fugelfiord, and on my arrival was met by the exclamation, 'True was it indeed what ye said; the day that ye pointed out, on that day did the measles, with the red spots, appear upon all nine of us!'"

There was evidently much less danger of contagion from intercourse with the affected in the open air; and it was likewise here remarked, as it has been elsewhere, that the intensity of the epidemic greatly abated towards its close. Our author remarked, that under these circumstances the contagion seemed to spread much more slowly than at first, and though perhaps as many in each household were attacked, yet that the disease took, as it were, a longer time in affecting this extension. No instance was observed where a longer period than that of fourteen days elapsed without a new case showing itself in a household,\* where others were subsequently affected, unless the disorder, in these latter cases, was traced to a fresh source of infection. Still our author believes, that the contagion may be retained for a longer period, perhaps, than this, in woollen and other articles, such as clothes, hemp, &c. Although Mr. Panum fully admits, that the diet and constitution of the persons affected may influence the period of incubation of this disorder, still, from what he has observed, he does not believe these circumstances capable of delaying or accelerating, by twenty-four hours, the appearance of the eruption. The catarrhal antecedents may, however, vary much, he thinks, in their duration. In some instances, they prevailed six or eight days before the appearance of the exanthema; and the milder the case, the shorter in general was the preliminary stage.

Mr. Panum is also satisfied, that measles cannot be communicated during the stage of incubation, that is, while the individual yet remains

\* We use the term household, as we have no English word to express accurately the meaning of the Faroese word "Böjgd." The Scotch word "toon," in Icelandic "Tun," is, however, identical in meaning; it denotes the whole of a farm building.—outhouses, rooms for servants, for guests, &c. &c., which are often not under one roof, but separated from each other.

in perfect health. The general doctrine, too, that, this disorder is most contagious during the period of desquamation, is based, probably, upon the circumstance, that the other members of a family often do not exhibit signs of the disorder, until the first affected has arrived at the stage of desquamation. Our author is convinced that measles are most communicable during the first appearance of the exanthema, and till it reaches its maturity; this too being the period when the acid smell of the patient is most remarkable, and when probably the morbid exhalations are the strongest.

During his residence in the islands, our author vaccinated about sixty children, with a view to ascertain how far the slight febrile exanthema of variola vaccinia would serve as a prophylactic against the more dangerous measles. Our author, however, could detect no antagonism between the two; they were developed, and proceeded side by side, in the same individuals. No experiments were ventured upon of propagating measles by inoculation.

It is well known, that in certain cases measles may attack an individual a second time; but it is a remarkable fact, that of those now elderly people who, in 1781, had had measles, not one individual was affected on the present occasion. Other aged persons who had not had measles during the epidemic of 1781, were as liable to the disorder on this occasion, as were their juniors. Some young people, though perpetually exposed in the most complete manner to the contagion, never seemed capable of taking the disease.

One of the most interesting questions illustrated by Mr. Panum, is that of the relative intensity of the contagion under different circumstances, and the success or failure of means resorted to, to escape its influence.

"On the 2d of June, a boat left Funding to bring goods from the trading place at Klaksvig. The men of this boat could not however obtain the goods they sought for, unless they assisted in unloading corn from the ship which had newly come from Thorshavn. On board the ship were several men who had just passed through the measles, and the servants at Klaksvig were at that moment lying ill of that disorder. The boat's crew from Funding, who had been down in the hold of the ship and in the store-houses, but who otherwise had not been in contact with any of the sick, took the precaution, on their return home, of throwing away all the paper in which the wares they had purchased were wrapped. They likewise undressed in an outhouse, washed themselves all over, and threw the clothes they had put off into a tub of water. Not one of these men took measles until the 3d of July, when all the hamlet was already attacked by the disease. On the 3d of June another boat left Funding, and was joined by one from Nordre-Gjov, and both proceeded to the trading place at Klaksvig. One of the crew of the boat from Funding, feeling unwell, went into a house at the trading place, and even into a chamber of that house, where many people affected with measles were then lying. The other men from Funding, and all those from Nordre-Gjov were merely in the ship's hold and in the store-houses, where they were, however, in close contact with many other men, and among these with a man from Nord Ære where the measles then raged. On their return from Klaksvig, the Funding boat's crew took the same precautions as their neighbours had done on the preceding day, and not one was affected until a month after, when the malady prevailed in that locality. But the men from Nordre-Gjov, who took no precautions of this kind on their return home, were all five seized with measles about fourteen days after. On the 8th of June a third boat went from Funding to Klaksvig, where the servants were now recovering from measles, and where they met people from Leervig, who had come out for the

first time after passing through the disease. On their return home, the Funding boat's crew adopted the same precautions as had proved so unsuccessful before; but, in spite of this, they were all attacked with measles, and the eruption came out on them about fourteen days after."

Our readers may form, from these details, their own conclusion; but to us they seem rather to favour the old opinion, that the contagion of measles is strongest in the stage of desquamation.

It is not often that our profession are accused of being the carriers of disease from one patient's house to another. Though every other individual coming from the infected localities would be carefully shunned, the doctor's arrival from the house of death or sickness creates no alarm. In Färoe, however, on this particular occasion, every precaution was taken to purify "the doctor" before he was admitted. Mr. Panum thinks it quite possible that the contagion of measles may be transmitted by clothes. Our author carefully investigated the results in those houses where a strict system of quarantine or isolation was maintained throughout the epidemic. Many hamlets, and some with more than one hundred inhabitants, entirely escaped, and ascribed their immunity to having rigorously adopted this system. Were measles an epidemic, conveyed by atmospheric influence, these precautionary measures would have been utterly unavailing.

We have been thus careful in the analysis of this brochure, because we feel that the observations it contains, if fully borne out by subsequent researches, will be of no small influence in the prevention of the spread of contagious disease. It is seldom, indeed, that an opportunity like that here described is afforded to a prudent and able man of science, who, like our author, rejecting all previously conceived opinions, diligently investigates the truth for himself.

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#### ART. IX.

1. *A Treatise on Baths; including Cold, Sea, Warm, Hot, Vapour, Gas, and Sand Baths: also, on the Watery Regimen, Hydropathy, and Pulmonary Inhalation; with a Description of Bathing in Ancient and Modern Times.* By JOHN BELL, M.D., Member of the American Medical Association, and of the Pennsylvania Medical Society; Fellow of the College of Physicians of Philadelphia, &c. &c.—Philadelphia, 1850. 8vo, pp. 658.
2. *Researches into the Effects of Cold Water upon the Healthy Body, to Illustrate its Action in Disease; in a Series of Experiments, performed by the Author upon himself and others.* By HOWARD F. JOHNSON, M.D., Physician of the "Ferns" Hydropathic Establishment, &c.—London, 1850. 8vo, pp. 190.
3. *The Bath Waters: their Uses and Effects in the Cure and Relief of various Diseases.* By JAMES TUNSTALL, M.D., Physician to the Eastern Dispensary of Bath, &c.—London, 1850. 8vo, pp. 144.
4. *The Baths of Rhenish Germany; with Notices of the Adjacent Towns.* By EDWIN LEE, Corresponding and Honorary Member of &c. &c. &c.

THE hauntings of old book-stalls, and the readers of catalogues of second-

hand medical works, will often meet with a work termed, "*Psychrolousia*, or the History of Cold Bathing, both Ancient and Modern, in Two Parts," the first written by Sir John Floyer, Knt., and the second by Dr. Edward Baynard. The date of the original works is 1702, but the Sixth Edition before us was published in 1732. As compared with this volume, the modern works, whose titles head this article, are dull indeed. Sir John Floyer writes gravely enough; it is Dr. Edward Baynard's contribution which is the life of the volume, and renders it one of the most entertaining of medical works. The manners of the time permitted a plainness and coarseness of language not to be tolerated at the present day; of this license Dr. Baynard fully availed himself in defending cold bathing and carrying the war into the ranks of his antagonists, — the drug-dealing doctors of the time. A "physick-flinger, the goggling *Goliath*," is described as prescribing an "oriental pearl emulsion" "after a brace or two of unmannerly belches," and on being asked what he thought of the cold bath, is described as saying in a sputter and passion, "Z—ds! it will kill you, it will kill you." The following is a short specimen of what may be found in almost every page:

"Another of this Class, a man of no mean Magnitude once, but now despised and p—s—d upon, a Fellow of a *gelt* and *castrated* Reputation, for, having out-liv'd that Set of *Fools* that once admired him, he can beget no more; this Man, I say, being ask'd by a melancholy Patient his opinion of a *Cold Bath*? answered him in making Mouths, with strange Distortions of Chops and Nose, and after his Face had entertain'd him with turning the Somerset a while, he by Degrees broke up his Grimace, and swore it would kill him. Now I would fain know what it is that frights all these foolish People and makes their Heads run thus upon killing? How can a Man's washing himself in *cold water* kill him? 'Tis but of late years that Sprinkling came in and was us'd in *Baptism*; and what, I pray, became of all the *tender* new-born *Infants* that were made *Christians* by *Immersion* in a cold Marble Font, in a damp Church, in cold hard winters, and in the worst of weather sometimes? What, were all these children *kill'd*? I am apt to think the *Devil* has scatter'd some of his Hell-Grubs in their Sculls, and fly blown'their *Understanding* to a Degree of *Lunacy*, lest the old way of *Immersion* should come into the World again; not that I am an *Anabaptist*, for I was sprinkled myself, and a sprinkled *Christian* is better than none; for I put no great stress upon the Form, provided a Man believes well, and lives well; for he is my *Christian*, that shows me his *Christianity* by his Faith, his Faith by his good Works, &c. But this being the Parson's Province, I have done."

And so Dr. Baynard goes gossiping on, detailing cases, moralizing, punning, rhyming, *de omnibus rebus cum quibusdam aliis*. There is an interesting letter from Penn to him, detailing, as an eye-witness, the transition method adopted by the Indians in using the cold bath; the letter was sent in consequence of "dining at a nobleman's table at Bath, in September, 1702, with Mr. William Pen, and discoursing with him and some other gentlemen concerning cold baths." Baynard also refers to "that most remarkable story of Alexander Selkirk, a Scotchman, who from a leaky ship was, upon his own request, set on Shoar on an Island in the South sea, call'd Juan Fernandez, about the latitude of 33 degrees, where he liv'd four years and four months by himself alone, and eat nothing but Goat's flesh, and drank water, having neither bread nor salt, as he told me himself at the Bath, where I met him."

Both Floyer and Baynard refer to and lament the disuse of cold bathing; so also Dr. Cheyne, in his Essay on Gout. "Bathing," the latter



observes, "made a greater noise upon its first restoration, and nothing could have sunk lower in its reputation since, than cold bathing; and it will always happen so in things fitted to the vulgar capacity, when they are universally prescribed, without distinction and without choice." The wisdom of this remark must be obvious to all who have watched the progress of the popular mind on the subject of cold bathing and the watery regimen, known under the absurd term of hydropathy, or the *water-disease*. There cannot be a doubt, we think, that unless the intelligent members of the profession generalize the facts with which a mad empiricism has supplied them, and fix the medical use of water as the legitimate and appropriate remedy for a large class of diseases, the aqueous charlatans will damn it altogether in the public estimation, and so a valuable remedy be lost. The works before us have each a claim to our notice in this respect, for they all tend towards that great object. Their merit is, however, widely different. The work by Dr. Bell is the most complete monograph, we do not hesitate to say, on the subject, that has ever issued from the press, and should find a place, as a work of reference, in the library of every practitioner. It is also, we think, one of the most systematic works ever printed in the United States; and in this respect contrasts most favorably with the greater proportion of Anglo-American literature. If we were disposed to be hypercritical, we should, perhaps, demur to acknowledging the correctness of the title. The error is, however, venial; for although the title-page be headed "Dietetical and Medical Hydrology," the work contains notices of gas and sand-baths, and of pulmonary inhalation, clothing, and exercise, as well as of the various objects for which water may be taken and applied to the skin, and so gives more than it promises. Not a minor merit of the work is the history of bathing and its modes of practice in various countries; a subject of very considerable importance, now that we have at last attained to such a degree of civilization as to have public baths in the true sense of the term,—that is to say, baths established by the public authorities, and supplied at such a cost, that even the common beggar need not wander with an unclean skin. Since the use of baths will undoubtedly become more and more general, and the medical profession will be oftener requested to state their opinion on all questions of regimen and dietetics having reference to baths and bathing, we think the practitioner will not rest content with the present state of his knowledge on the subject (which is confessedly imperfect), but will gladly avail himself of any sources of information that can be rendered available.

Bathing is of high antiquity. Sir John Floyer devotes a letter "to the learned physician, Dr. William Gibbins," to set it forth. He gives extracts from the Scriptures to prove, "that the ceremony of purifying by water was ancients than the law of Moses." Thus Jacob commanded his family to purify themselves, and change their garments before they went to Bethel to sacrifice. Job speaks of a like purification with snow-water. Pharaoh's daughter went to the River Nile to purify herself; ablution, as a religious rite, appears to have been very general in ancient Egypt. Diodorus Siculus mentions that the first Egyptian monarch washed his body with water before he put on his royal robes and went to sacrifice. Porphyry affirms that the priests of Egypt washed three times a day. Dr. Bell refers to various passages and descriptions in 'Homer's Iliad,' as showing that domestic baths are of remote antiquity. The domestic customs and

religious rites of all the oriental nations, whether considered historically or ethnologically, amply prove the general prevalence of personal ablutions amongst them ; and the high esteem in which personal cleanliness was held by the leaders of the public mind. And, although the scheme of making personal cleanliness a religious duty has failed in its great object, we cannot the less admire the beneficent wisdom which dictated it, nor need we hesitate to acknowledge that the wise legislators of the day conferred a permanent social good, when they linked cleanliness to godliness by legal and ritual bonds.

The ritual observance of bathing appears to have ceased with the Romans ; but the strongly practical turn of mind of that people led them to see the advantages, and to appreciate the substantial comforts of a daily bath. The erection of public baths on a large scale quickly followed, and in the course of two centuries, there were upwards of eight hundred thermæ in imperial Rome. In these thermæ water of every kind was found ; the water of the sea, of sulphurous springs, as well as of the Tiber and mountain springs. Temples, palestræ for games of every kind, and libraries, were contained within their precincts. "Architecture, sculpture, and painting," Dr. Bell observes, "exhausted their refinements in these establishments ; which, for their extent, were compared to cities ; incrustations, metals, and marble were all employed in adorning them. Those of which the most numerous remains are still visible, are the baths of Titus, Antoninus, Caracalla, and Diocletian." Wealthy Romans set an example which wealthy Englishmen might well follow. Agrippa, the relation, friend, and counsellor of Augustus, bequeathed his gardens and the baths, which went by his name, to the Roman people ; and he appropriated particular estates to their support, in order that the baths might be free. Some idea may be formed of the magnificence of these baths, by a consideration of the fact, that the now existing Pantheon served as their vestibule. Still grander were the baths of Caracalla, of which the present church of the Carthusians—one of the largest of modern Rome—was one of the halls. They were ornamented with 200 pillars, and furnished with 1600 seats of marble, in which 3000 persons could be seated at one time. These establishments were, however, with both the Greeks and Romans, more than baths, being at the same time gymnasia ; but the fact only increases our admiration of the wisdom which placed all the best means of preserving health within the reach of the people ; and it ought to convince us that the science and art of hygiene had attained to a higher degree of development amongst ancient nations, than it has as yet reached amongst the most highly cultivated and most civilized of modern. The baths of Caracalla (of which Dr. Bell gives a ground-plan, taken, on a reduced scale, from the volume entitled 'Pompeii,' in the 'Library of Entertaining Knowledge,') give a good idea of the completeness of these establishments. The natatorium, or swimming-bath, of Diocletian was 200 feet long, and 100 feet broad. The water supplied to the *eight hundred and fifty-six public baths* of Rome was derived from a daily supply of more than thirty millions of gallons, brought to the city by its splendid aqueducts. The daily supply to London from nine water-companies, as stated by themselves, is nearly forty millions of gallons. Perhaps the method of heating the water for these baths is not less interesting than their extent and architectural arrangements. It is calculated that 18,000 persons might have been

bathing at the same time in Diocletian's baths; how was the immense body of water necessary to this purpose heated? The following appears to have been the plan:—the water from the aqueduct was first received into a cistern, and thence into a shallow reservoir, in which it was exposed to the sun; whence the water passed into a series of vaulted rooms, arranged in two rows, fourteen on each side, and all communicating with each other. Under these, were other twenty-eight rooms, situate immediately over the hypocaustum or furnace; being likewise in communication with each other, but only one of the series in connection with the upper series. The furnace was so constructed, and so connected with flues, that even the heat of the smoke was utilized. In addition to the direct application of heat to the water in the lower chambers, hot-air flues ran up through the sides and party-walls, reaching as high as the second series, and thus rendering the water in them tepid, and ready to be heated in the chamber below. The hypocaustum, or furnace, was a contrivance for heating by hot air; and remains of such an arrangement have been found in various parts of England, as well as at Pompeii; a model of a portion of a bath, thus uncovered, is contained in the Museum of the Yorkshire Philosophical Society, at York. This mode of construction is well worthy the study of architects; for the removal of the brick duty, and the impetus given to the manufacture of what may be termed *brickware*, in the form of hollow bricks, drain-pipes, flues, &c., will enable the professional man to apply his plastic material to an infinite variety of purposes, and especially to domestic comfort, in warming both by air and water, in ventilation, and in economy of construction. The floor of concrete used by the Romans to construct their floors hollow, so that the warm air might be circulated through them, has been lately patented; the material was simply lime and pounded bricks, and is now applied in combination with iron beams, to the construction of fire-proof houses. The walls were equally hollow as the floors, and communicated with the latter. Tiles were used for both the floors and walls; but for the latter very ingeniously. While the clay of the tile was moist, some circular instrument was pushed through it at the four corners, so as to make a hole, at the same time forcing out the clay and making a pipe or projection on the inside of the tile about three inches long; iron clamps were then passed through them, and the tiles fastened to the wall.

These brief and general notices of the public baths of the Romans will interest those of our readers, (a numerous class, we hope,) who are anxious for the establishment of public baths and wash-houses in our large towns and villages. And although it would be premature, perhaps, to say much of the *abuse* of an institution which has hardly come into *use*, yet we cannot altogether overlook the wisdom with which Dr. Bell touches upon the causes of the *disuse* into which the public baths in ancient Rome finally fell; since those causes may become operative in our own time. The principal of them seems to have been the perversion of the baths, from their original use as aids to individual hygiene, to the purposes of luxury and vice. Dr. Bell observes, that at first they were resorted to by the Romans for the purposes of cleanliness and refreshment, after the bodily exercises in the palæstra, or the intellectual efforts at the forum; the warm bath was, after a time, made a pretext for indulging in various frivolous amusements in the apartments and courts accessory thereto. In this way, idleness and

its attendant evils were encouraged. In addition, the frequenters of the bath lounged away their time in the bath itself; and hence a continually increasing desire to have the water at a higher temperature, so as to enjoy the same pleasurable sensations which a moderate temperature at first excites. It appears also that the baths were used hydropathically by the gluttonous to excite an artificial appetite.

"The crapulous glutton entered a small chamber, which was heated to as high a degree as the person could possibly endure, by means of lamps or flues conducted round the walls. The circulation of the blood being greatly accelerated, and the solvent power of the air much increased by this dry heat, a copious flow of perspirable matter ensued, and artificial hunger and thirst succeeded this unnatural mode of depletion, while appetite thus excited was gratified by a return to the festive board."

They also became the daily resort of the vicious and profligate. The abuses of the public baths in more modern times might also be beneficially borne in mind by those, who take an interest in the current movement for what may be very fairly termed their *re-establishment*. It is not long since that bath and *bagnio* were synonymous with brothel.

Having in view the practical question, which the establishment of public baths will raise, as to the benefits to be derived from the general use of the baths they can afford,—as to the state of health or indisposition with which bathing itself may be compatible, and as to the modes in which it may be carried out,—we shall limit our notice on the subject to an inquiry upon these points, leaving the more extensive field which Dr. Bell has occupied to the more careful study and investigation of the reader. The simplest division of baths of this kind, is into the cold, warm, and hot; terms, however, which are highly relative, inasmuch as they refer to our sensations. If water, of any given heat, be applied to the surface which bears a temperature of 90°, as the hands and feet, the sensation is different from that which is experienced when applied to the abdomen, where the temperature is 96°, or to the axillæ, where it is 98°. The same temperature excites different sensations in different individuals, and in the same individual under different circumstances. Thus, two persons were experimented on by Dr. Johnson with the wet sheet, the one being of the sanguine and the other of the "phlegmatic" (lymphatic?) temperament: the man of sanguine temperament felt, in successive experiments, as follows:

From ten to twenty minutes after packing:

1.	At a temperature of	93°, felt warm.
2.	" "	91°, " warm and comfortable.
3.	" "	93°, " quite hot.
4.	" "	93°, " quite hot.
5.	" "	90°, " quite warm.
6.	" "	95°, " warm.

Three experiments on the lymphatic person are detailed, who,

With the sheet at a temperature of	95°, felt moderately warm.
" "	93°, " neither warm nor cold.
" "	92°, " quite comfortable.

Dr. Johnson observes, that this person never, by any chance, became *hot*; and that persons have complained of feeling cold in the wet sheet, when it has been at 93°.

which will be pleasant to all, and everywhere alike,

and which Dr. Bell designates as an invariable temperature, corresponds with the animal heat, and to  $95^{\circ}$ — $97^{\circ}$  of Fahrenheit. To allow for individual variations and peculiarities,  $95^{\circ}$  may be taken as the mean temperature of the *warm* bath, and three degrees above or below ( $92^{\circ}$  and  $98^{\circ}$ ) as the extremes. The temperature of the *tepid* bath is fixed by Dr. Bell at from  $84^{\circ}$  to  $90^{\circ}$ . A *cool*, as distinguished from a *cold*, bath, may be fixed at any temperature from  $74^{\circ}$  to  $84^{\circ}$ ; under  $70^{\circ}$ , the bath may be considered as a cold bath. A *hot* bath excites the pulse, increases the heat of the surface, especially of the head and face, and induces some fulness in the head and confusion of thought. These effects result when the water is above the animal heat, or exceeds  $98^{\circ}$ . Dr. Bell very justly dwells upon the necessity of accuracy in discriminating and designating these different kinds of baths, as numerous mistakes and misconceptions arise from an incorrect use of terms. Thus, Currie calls that a *cool* bath which others would call warm. Saunders also terms Buxton at  $82^{\circ}$ , a warm bath, when it is, correctly speaking, *cool*. So, also, Dr. Tunstall vaguely states, that “a hot thermal bath is of the degree marked by Fahrenheit’s thermometer, 105 to 107. A warm bath, 98 to 102. A temperate bath, 95. A tepid bath,  $86^{\circ}$ .” Dr. Forbes seems to have been Dr. Bell’s guide, at least, partly, to his arrangements, who, in his article on bathing in the ‘Cyclopædia of Practical Medicine,’ thus arranges baths according to their temperature :

The cold bath	.	.	.	.	.	.	.	from $33^{\circ}$ to $60^{\circ}$ .
„ cool	.	.	.	.	.	.	.	„ $60^{\circ}$ to $70^{\circ}$ .
„ temperate	.	.	.	.	.	.	.	„ $75^{\circ}$ to $85^{\circ}$ .
„ tepid	.	.	.	.	.	.	.	„ $85^{\circ}$ to $92^{\circ}$ .
„ warm	.	.	.	.	.	.	.	„ $92^{\circ}$ to $98^{\circ}$ .
„ hot	.	.	.	.	.	.	.	„ $98^{\circ}$ to $112^{\circ}$ .

The German arrangement is as follows :

The cold (or sea) bath	.	.	.	.	.	.	.	from $43^{\circ}$ to $50^{\circ}$ F.
„ cool	.	.	.	.	.	.	.	„ $52^{\circ}$ to $72^{\circ}$ F.
„ tepid	.	.	.	.	.	.	.	„ $72^{\circ}$ to $86^{\circ}$ F.
„ warm	.	.	.	.	.	.	.	„ $86^{\circ}$ to $95^{\circ}$ F.
„ hot	.	.	.	.	.	.	.	„ $95^{\circ}$ and above.

Germany possesses more than 300 cold and hot mineral springs; and the Germans use, besides, air baths, sun baths (*sonnenbäder*), electrical baths, together with douche, rain, dropping, dust, and vapour baths.

*Physiological effects of the cold bath.* Having determined the definition of terms to be used, we are in a position to take up each kind of bath and examine its physiological, hygienic, and therapeutical action, whether absolutely, or in combination. And, first, as to the physiological effects of the cold bath. It has been long well known that the application of cold water to the surface excites chilliness and shivering, or a *shock*. The skin becomes pale and shrunken, from a diminished fulness of the capillaries,—both of the dermis and subcutaneous cellular tissue—so that rings not fitting tightly will drop off in the cold bath. Nor, as Dr. Bell correctly remarks, is this diminution of size observable in those parts only, which come in contact with the water, for a hip bath, or even a foot bath, will remove the colour from the cheeks. Further, (and the general fact is of great importance,) the inner or mucous surfaces are placed in an analogous condition, for the air taken into the lungs is expired without undergoing changes to the same extent as before immersion in the cold



water took place ; while in cases in which the tongue is preternaturally red previously to immersion, and the mouth dry and hot, exciting the sensation of thirst, these phenomena are so far modified by the cold bath, that the tongue loses much of its redness, and the heat and thirst disappear. In a case experimented on by Dr. Currie, the thermometer placed in the mouth under the tongue fell from  $98^{\circ}$  to  $87^{\circ}$ , after the person had been in water of  $44^{\circ}$  for two minutes ; it rose gradually, however, to  $93^{\circ}$  and  $94^{\circ}$ , after twelve minutes. Nor does it appear that the circulation through the brain and nervous system is accelerated, for the cold bath lessens mental activity ; the person becomes dull, has his range of ideas limited, his perceptions blunted, his muscular powers enfeebled. The heart's action is also impaired, for although at the first *shock* it becomes hurried and tumultuous, it afterwards beats more slowly and feebly. Dr. Currie proved that it was not possible to keep the subjects of his experiments free from some degree of previous agitation. One person upon whom he experimented, had his pulse thus raised from a natural standard of 70 to not fewer than 85, and generally more. It invariably sank, however, to 65—68 in the water, and became firm, regular, and small. After being long in the bath it could hardly be felt at the wrist, but the heart pulsated steadily and "with some force." Marcard, Athill, and Londe, corroborate Currie's results.

Dr. Johnson's experiments deserve notice here ; for although too few to satisfy the rigorous requirements of modern science, they are more numerous than those of the investigators we have quoted. We think it due, however, to science, to enter a *caveat* against the full reception of Dr. Johnson's statements, as statements of *facts* ; for, although his book pretends to have a scientific character, and is *professedly* addressed to the profession, there is ample internal evidence, independently of its being also avowedly written for popular circulation, that the writer has had less in view the progress of science than the glorification of hydropathy, and the depreciation of orthodox practice. Still, as there may be a grain or two of truth in his bushel of chaff, we sift it. His experiments were made with the wet sheet, douche, blanket, packing, shallow bath and sitz bath. Of these, the wet sheet, shallow bath, douche and sitz bath, may be considered in the nature of cold baths ; the former, however, is speedily changed into a mild vapour bath, as the body becomes bathed in a vapour. The "sitz" bath and shallow bath, consequently, deserve our more especial notice. A man, aged about 28 years, and weighing about ten stones and a half, sat six times, for fifteen minutes each time, in four gallons of water, at a temperature of about  $43^{\circ}$  Fahrenheit. During that period, he raised the water about  $8^{\circ}$  in temperature, while the pulse was modified as follows, at each experiment :

	Exp. 1st.	Exp. 2d.	Exp. 3d.	Exp. 4th.	Exp. 5th.	Exp. 6th.
On entering, the pulse was	100	86	72	66	72	68
After 5 minutes	72	64	57	64	54	52
„ 10 „	72	64	54	60	54	54
„ 15 „	72	64	56	60	51.5	51

A second series of experiments were made on the same person, but with this difference that he remained in the water half an hour. During that

period, its temperature was raised 10—12°, and the pulse and re spiration were thus influenced :

	1st Exp.		2d Exp.		3d Exp.	
	Pulse.	Respir.	Pulse.	Respir.	Pulse.	Respir.
Before the process	76	19	72	20	71	19
After 5 minutes	64	20	62	20	52	20
„ 10 „	56	18	56	20	47	16
„ 15 „	55	18	52	20	47	18
„ 30 „	52	18	46	18	42	18

The individual, thus experimented on, was of the phlegmatic temperament, and took exercise previously to the bath. From a slight calculation of the averages, it will be seen that the first five minutes' immersion lowered the pulse by nearly sixteen beats, and the second five minutes by nearly three beats. Dr. Johnson made five experiments on himself; he represents himself of a temperament intermediate between the phlegmatic and the sanguine, and took no exercise previously to bathing. Before the commencement of the bath, Dr. Johnson's pulse averaged 64·6; after five minutes' immersion, it fell to 58·8, or nearly ten beats; after ten minutes, it averaged 53·6; and, after fifteen minutes, 53·2. The wet sheet had, during the first hour, a somewhat similar influence on the pulse. The pulse of a person of sanguine temperament averaged, in six experiments, a fall from 99·33 to 77·66, or nearly twenty-two beats in *one minute* after applying the sheet; the average fall in three experiments, in a person of phlegmatic temperament, was from 68 to 54, or a reduction of fourteen strokes per minute. The average of the two series of experiments, is a reduction of nearly eighteen strokes during the first minute; but the pulse fell six or seven more during the process.

Dr. Johnson gives us data for estimating the influence of the cold bath on the respiration; and from the tables above given, it does not appear that the “*sitz*” bath has much influence on the respiratory movements. The experiments with the wet-sheet point, however, to a different result, the respiratory process being much accelerated by the shock. In the phlegmatic subject, the respiration averaged an immediate increase of twelve respirations per minute; and after the effect of the shock subsided, there was a permanent increase of one or two respirations. We cannot think with Dr. Johnson, that so inconsiderable an acceleration can have any very decisive effect on health or disease.

The *douche* bath may be considered as analogous to a cold bath, and it might have been expected *à priori* that its effects would not be dissimilar. Yet, if Dr. Johnson's experiments put us in possession of its real effects (which we doubt), this is not the case. Of the two persons experimented on, he of the sanguine temperament had his pulse accelerated; he of the lymphatic had his pulse rendered slower. The one, aged 40, of an elastic buoyant disposition, both of mind and body, received the *douche* for *one minute* in five successive days, with an average increase of 26 beats per minute; or from an average of 97·2 to 123·6. On the other hand, the pulse of the lymphatic person fell on an average of nine experiments from 98·44 to 86·44, or twelve beats per minute. In both instances, however, the respiration was accelerated.

The *shallow bath* is a shallow vessel containing 15 gallons of water, in which the individual is placed and rubbed for one minute. We gather from the context, that the water is so cold as to constitute a cold bath, and from one or two of the tabular statements, we learn that its temperature varied from  $39\frac{1}{2}^{\circ}$  to  $65^{\circ}$ . This variation in the temperature is fatal, we think, to the deductions which Dr. Johnson draws from his experiments. It is obvious, we think, that in conducting experiments of this kind, the circumstances under which they are made should be as uniform as possible. There can hardly be a difference of  $15\frac{1}{2}^{\circ}$  of temperature, without a corresponding difference in the physiological phenomena. Premising this *caution*, we state the following as the results of its action on the pulse, at which Dr. Johnson has arrived :

"The general effects of the first series of operations upon the pulse was to accelerate it by 11.37 beats in the minute. That of the second series was to diminish its frequency by 8.89 beats in the minute. In the third the pulse was quickened three times and retarded once in every four. What is the cause of this difference? It has been most satisfactorily explained on the score of variety of temperament. The pulse of the excitable temperament is excited, that of the phlegmatic one is depressed, that of the neither excitable nor phlegmatic is neither wholly excited nor wholly depressed. But it is more frequently excited (namely, three times as often) than depressed. It may, therefore, be considered, that, where temperament does not interfere either one way or the other, the more general effect of the shallow bath is to accelerate the pulse. *Its physiological action is that of a stimulant.*" (p. 163.)

Dr. Johnson adds as to the influence of the shallow bath on *respiration*, that it invariably promotes the rapidity of that function, not only actually, but also in proportion to the increased activity of the pulse. The general effect of the shallow bath is similar to that of the douche.

We have already noticed the increase of temperature which the water of the hip or "*sitz*" bath acquires during immersion of the bather. Dr. Johnson made observations on the effect which the animal heat of the bather had upon the water of the shallow bath in raising its temperature. He thought that the amount of heat thus given off would measure the constitutional power of the individual, and that the greater his bodily vigour, the more the temperature of the water would be raised,—reasoning, that "if the various animal functions be conducted in a tolerably healthy manner, as the most, or one of the most important, of them all, is the generation of heat, this product, if by any means it should be more than usually abstracted from the body, would be the more readily resupplied." On the average result of many operations he found, that a man of sanguine temperament, immersed for a minute and a half, raised the temperature of the water from an average of  $49^{\circ}\cdot 57$  to an average of  $50^{\circ}\cdot 93$ ; a man of phlegmatic temperament raised it, in like manner, from an average of  $45^{\circ}\cdot 53$  to  $47^{\circ}\cdot 46$ ; and another of neutral temperament raised it from  $44^{\circ}\cdot 57$  to  $46^{\circ}\cdot 46$ ; or  $1^{\circ}\cdot 26$ ,  $1^{\circ}\cdot 93$ , and  $1^{\circ}\cdot 19$ , respectively. The data Dr. Johnson has given, have enabled us to analyse his statistics, and we find that we come to different results. Taking the experiments made upon the man of sanguine temperament, and classifying them in two tables according as the water in the shallow bath was at a temperature above or below  $50^{\circ}$  on immersion, we find we can draw up the two following tables with eight experiments in each :

Bath at 42°·5 . . . raised to 45°.				Bath at 65° . . . raised to 66°.			
„ 41 .	„	42 .5		„ 51 .	„	52 .	
„ 39 .75	„	41 .3		„ 53 .	„	54 .	
„ 39 .5	„	41 .5		„ 53 .	„	55 .5	
„ 42 .	„	44 .		„ 55 .	„	56 .	
„ 48 .5	„	49 .25		„ 53 .	„	54 .	
„ 48 .	„	49 .33		„ 60 .75	„	61 .75	
„ 49 .25	„	50 .		„ 52 .	„	52 .75	
<hr/>				<hr/>			
Sum . . 350°·5		362°·88		Sum . . 442°·75		452°·	
Average 43 .81		45 .36		Average 55 .34		56°·5	
Being an average increase of 1°·55.				Being an average increase of 1°·16.			

If, then, these observations are to be relied on, there was a greater evolution of animal heat at temperatures below 50° than at higher temperatures, by 0°·39, or *one-fourth more*; or in other words, the man of sanguine temperament raised water from an average of 43°·81 of temperature to 45°·36, or 1°·55, while water at an average temperature of 55°·34, was raised only to 56°·5, or 1°·16. Practically, this difference is of importance.

*Stage of reaction of the cold bath.* This evolution of animal heat, is, doubtless, due to that innate faculty of the living organism, whereby an equable temperature of the whole mechanism is kept up. The first effect of contact with water below the temperature of the body, is to reduce the temperature of the latter by abstracting heat. To counteract this abstraction, an immediate effort is made to generate more. Hence the immediate acceleration of the pulse and of the respiratory movements. If, however, the abstraction of animal heat is longer than momentary, the organism then begins to experience, in the action of its mechanism, the effects of the lower temperature in which it is placed, and the vital functions proceed more slowly; consequently, the pulse and respiration become less frequent, and a sedative effect is experienced. The results will depend for their intensity on the degree of cold and the length of time during which the individual is exposed to it; or, relatively, upon the powers of the individual organism to compensate for the abstraction of heat by a further generation of it, and upon the vital force of the various organs themselves. Persons constitutionally weak and lymphatic, possessing little energy of circulation, an impoverished or *spanæmic* blood, with cold extremities and languid capillary action, suffer, for these reasons, from cold bathing in water at a temperature which throws the sanguine and robust into a glow, and which gives such an individual the feeling of healthy vigour—simply because the organism reacts with sufficient vigour to triumph over the cold. It is thus that the cold air of a frosty day—exciting the organism to action, when the latter is enabled, by breathing a more condensed atmosphere and accelerating the transformation of the tissues by active exercise, to generate such an amount of animal heat as will counteract the abstraction,—gives a freshness and sense of vigour never felt during the temperature of summer. On the other hand, this accelerated transformation calls other instincts into operation, as the alimentative, and thereby the appetite is sharpened, digestion quickened, effete materials rapidly removed, and a healthy tone given to the whole organism. To attain such results, even in part, by the cold bath, simply requires judicious management and a careful attention in each case to the physiological action of the cold water on the system. The recent progress

of physiological science promises to throw some important practical light on this subject. In Dr. Carpenter's recent paper "On the Mutual Relations of the Vital and Physical Forces," the relation of *heat* to the vital forces and functions is set forth and illustrated in a way bearing directly upon the physiological and therapeutical action of the cold bath. Dr. Carpenter has shown, that under given circumstances "the rate of life," or vital action, varies directly as the temperature of the surrounding medium; so that frogs confined in a limited quantity of water, and not allowed to come to the surface to breathe, live the longer in proportion as the water is lower in temperature. Frogs thus situated died in from 12 to 32 minutes when at a temperature of  $90^{\circ}$ , but survived from 367 to 498 minutes at freezing, not because they were torpid, as Dr. Carpenter observes, but because of a diminished activity of all their functions, and, therefore, a less demand for air.

*Hygiène of the cold bath.* The method of using the cold bath, the precautions to be taken, and the cases in which it may be beneficial or hurtful, may be readily inferred from the preceding researches. It is obvious that the period of immersion should not be prolonged, whenever it is intended to act as a tonic and stimulant; and that it should, in this respect, be assimilated with the douche and shallow bath. From one to two minutes' application of the water is sufficient to rouse the conservative powers of the organism and excite the desired reaction; a longer period will act as a powerful sedative and depress those powers. In many instances, however, a momentary plunge is all that can be safely practised, followed by immediate friction of the trunk and extremities, and brisk muscular exercise. It is obvious, that the shock, depression, and reaction caused by the cold bath render it unsuitable to be taken at all periods of the day alike. Dr. Bell well observes, that the process of digestion should be completed, whether, indeed, the bath be cold or warm. Hence the best times are before breakfast, before dinner, or late in the evening. Much mischief has resulted from the neglect of this rule, and sometimes it has had fatal consequences. Before breakfast is, perhaps, the best time of all, as the repose of night has prepared the organism for the beneficial reaction the cold bath excites. There are habits of life and of body, which render the use of the cold bath dangerous. Excessive fatigue, a sudden reduction of strength from any cause, as intemperance, excessive sexual indulgence, immoderate smoking, and the like,—all contra-indicate its use. In commencing the daily practice of cold bathing, the water should be either at the summer temperature, or if begun in winter, it should be warmed to  $70^{\circ}$ . The feelings of the bather will often indicate, when once the habit is formed, whether the bath will be safe or beneficial, or not. In our own case we are satisfied we have no better guide than this, so that whenever we feel an aversion (which may be termed instinctive) for the bath, we are content with cold sponging.

It is, doubtless, a gross error *to bathe infants* in cold water with a view to hardening them; they should have daily *tepid* ablutions only, unless in summer weather, when the water is comparatively warm, and the bath will give no shock. Persons of a spare or lymphatic habit, languid circulation, with the blood deficient in corpuscles, and of feeble powers generally, will rarely find the cold bath beneficial, except with great precaution. There can be no doubt, however, we think, that where reaction takes place with



vigour, the individual is hardened by habitual cold-bathing, if we understand by the term, that he acquires the capability of more effectually resisting the influence of cold. We apprehend this is not only established by experience (and we might mention our own case in illustration), but is in accordance with the general physiological law, that exercise of a function renders it more regular and more vigorous. The habit of resisting cold, is only in fact the habitual exercise of an instinct of the organism, whereby the capillary and calorific action of the tissues is brought into regular practice.

*River and sea bathing* are practised usually in summer; the bather can also indulge in the exercise of swimming, diving, or disporting in the water; consequently, immersion may be permitted for a longer period than in the ordinary domestic cold bath. The length of time must depend, however, upon the temperature of the water, the amount of exercise taken, and the degree of constitutional vigour which the bather possesses.

*Therapeutic effects of cold bathing.* The cold bath may be used both as a stimulant and as a sedative; the one object being attained without the other, or in sequence. Its direct sedative action on the pulse and circulation has long pointed it out as a most efficient and appropriate remedy in febrile and inflammatory diseases, and in those diseases of the nervous system which depend upon over-excitement. In this class the diminished activity of all the functions resulting from the abstraction of heat, hinders the strength of the patient, by checking the evolution and waste of vital force consequent on the febrile activity of the system. Dr. Bell, we observe, objects to the cold bath being termed a stimulant or tonic, on the ground that those terms involve us in contradictions and inextricable confusion. "The first and powerful impression of cold on the entire nervous system, expressed by the term *shock*, had better be received," he remarks, "as a separate fact, than arbitrarily connected with any series of symptoms or therapeutical phenomena, in a classification or arrangement of the *materia medica*." If by this Dr. Bell objects to placing cold bathing among the drug-stimulants, we agree with him in his objections; but if he means to object (as we gather from the context) to its classification with stimulants, we demur to his doctrine, for the arousing of the reacting powers of the organism amounts to a powerful stimulation. Undoubtedly, it is only *conditionally* a stimulant; while if immersion be sufficiently prolonged, it will act under all circumstances as a sedative; still it may be even *tonic* when a sedative; and with such a physiological and empirical knowledge of the conditions required, as we now possess, we may very safely place it under its double capacity in our *materia medica*. The conditions necessary to its use, as a tonic and stimulant, are, that the temperature and the duration of the period of immersion shall be so graduated as to correspond with the reactive powers of the patient, and never so far exceed them as to pass the bounds of stimulation, and so depress them. It is obvious, that in this point of view, what we have termed a *tepid* bath, might, in enfeebled constitutions, have all the beneficial consequences, as a stimulus, which result from the *cold* bath in the more vigorous. Hence a person with feeble powers of reaction would begin a course of cold bathing with the water at a comparatively high temperature, gradually lowering it, as the powers of the organism were invigorated.

The use of the cold bath as a *sedative* is, however, the more extensively practised, and the more generally useful; hence we find in authors a long

list of diseases in which it is the best remedy. Dr. Bell states the practice of Hippocrates, Galen, Celsus (who was the first to advise cold immersion for the cure of hydrophobia), of Rhazes (who prescribed the cold bath as a prophylactic against the smallpox), and other Arabian writers, and of a number of modern authors. Perhaps it would be advisable to include, under the term cold bath, all those means by which cold water is used for abstracting heat from the body, namely, cold affusion, the cold douche, the wet-sheet, the shallow bath, and cold immersion. We have seen that the first effect of the application of cold is to excite, during a brief period of two or three minutes, at the most, an increased activity of the circulation and respiration; this ceasing, the pulse and respiration fall below their normal rate of activity. Hence the effects of cold water in these respects are analogous to those of bloodletting, and of those remedies which act directly upon the motor powers of the heart, so as to depress the pulse. It is not matter for surprise, then, that the cold bath has been strongly recommended as an effectual remedy for febrile and inflammatory diseases; the more especially, as with the reduction in the activity of the circulatory and respiratory movements, there is a reduction of the burning heat, so annoying and irritating to the fever patient.

*The cold bath in fevers.* We shall notice, successively, the diseases in which the use of cold water will be found beneficial; and first as to fevers. In the *hot stage of intermittent fever* it is strongly indicated. Dr. Currie states that he frequently used the cold affusion in this stage, "and almost always with the immediate solution of the fit;" and that in some instances "the succeeding paroxysm has been prevented by using the cold affusion about an hour previous to the period of its expected return, and the disease has been ultimately removed by continuing this practice through four or five of the following periods." As to his own experience, Dr. Bell observes:

"It is impossible for any person, who has not actually experienced the efficacy of this remedy on himself, to realize the delightful transition from suffering to ease, from the raging heat and unquenchable thirst to the coolness and calmness of sensations which follow the use of the cold bath in the hot stage of intermittent fever. It cools, soothes, and quiets, by effectually reducing the excessive capillary excitement in all the membranes and sensitive expansions, &c." (p. 358.)

The *bilious remittent fever*, so closely allied in its nature and origin to intermittent fever, may be also most advantageously treated by the cold bath; and with reference to this point also, Dr. Bell thus records his own experience:

"I have used it by affusion, where the reaction was considerable, and the patient able to sit up; and in other cases of great local determination and heat, as in the stomach and head, I have directed ice or cloths taken out of cold water to be applied to these parts, with the most soothing effects. For an irritable stomach and craving thirst, this remedy and an allowance of ice-water for drink, or pellets of ice allowed to dissolve gradually in the mouth, are preferable to all the draughts and mixtures hitherto devised. Immersion can be practised where a tub is at hand, by assistants raising the body of a patient in a sheet and placing him gently in the water. If a prompt and decidedly sedative impression be desired, cold water may be poured at some height on the head, nucha, and along the spine. In cases in which the shock from immersion, or the douche, or spout bath cannot be borne, ablution is practised, by sponging the surface of the body, or a particular part of it, with cold water. The general indications for the use of the cold bath are applicable to its use in the disease before us, viz.:—high excitement of the

vascular, including the capillary tissue, and inordinate determination to particular organs. Exhaustion, feebleness of frame, and copious discharges, contra-indicate its use." (p. 360.)

Jackson and Currie used the cold bath largely in the treatment of *typhus fever*; they also published ample statements as to the modes and results of their practice, demonstrating its great success; yet we believe we are correct in saying that it has almost been forgotten. In the 'London Medical Journal,' for 1786, Dr. William Wright, formerly of the island of Jamaica, gave an account of the successful treatment of some cases of fever, by the ablution of the patient with cold water. Encouraged by this history, (which constitutes the first chapter of Dr. Currie's 'Medical Reports on the Effects of Water, Cold and Warm, as a remedy in Fever and Febrile Diseases,') Dr. Currie tried the method in the Liverpool Infirmary, when a contagious fever was prevalent in it in December, 1787, and with such beneficial results upon eight patients, that he continued the practice, keeping a register of the cases in which it was used: and which numbered 133. Of these, 94 occurred in the Liverpool Hospital in the four years subsequent to the period just mentioned; 27 in private practice; and 32 in the 30th Regiment of Foot, when quartered in Liverpool in 1792, besides 26 others in that regiment, in whom the fever seemed to be cut short by cold affusion. Of the 32 treated throughout a regular attack, 2 died; both these cases were those of men whose constitutions had been weakened by the climate of the West Indies; both of them had been bled in the early stages of the fever; and one of them being in the twelfth, and the other on the fourteenth day of the fever, when Dr. Currie first visited them, he did not prescribe the cold affusion in their cases. The restrictions he placed upon its use were such as naturally result from its physiological effects. He did not prescribe it during the termination of the fever, when the strength is much exhausted, and the heat of the body lower than in health; nor did he recommend it "during the feverish chill, or after the perspiration has begun to flow profusely." When the heat was a little higher than natural, in the later stages, he made the degree of cold very moderate. "Under these restrictions," he observes, (and the observation has received ample confirmation,) "the affusion of cold water may be used with perfect freedom in the low contagious fever of this country, and the facts already stated will show that it is a remedy of great power and efficacy. In the first stages of fever, it appears very generally to cut short the disease almost instantaneously; and even when it fails of this effect, as is usually the case when it is applied in the more advanced stages, it nevertheless moderates the violence of the symptoms, and shortens the duration of the fever."\*

The fevers characterised by inflammations of the skin, or the *exanthematic fevers*, are equally well treated by the cold affusion. The false pathology and therapeutics of this group current during the seventeenth and previous centuries, have not been without their influence in modern times, in restraining the free use of the remedy, although the therapeutics had been successfully exploded by Sydenham, Radcliffe, and others. Dr. Currie and Dr. Gerard, emboldened by the success of the hydriatic method in typhus, tried it, the one in smallpox, the other in *scarlatina anginosa*. From Dr. Gerard's cases, Dr. Currie infers "that the affusion of cold

\* Medical Reports on the Effects of Water, Cold and Warm, as a Remedy in Fever and Febrile Diseases, p. 41. 1797.

water extinguishes the incipient scarlatina, as well as the typhus, can scarcely be doubted." Dr. Currie "witnessed the happy effects of the affusion of cold water," in a number of cases of smallpox, and relates one or two in which he used it; he also adopted Dr. Gerard's practice in two cases of scarlatina occurring in his own family, with the same success. Dr. Bell has not found such beneficial results in the treatment of *variola*, but indorses Dr. Currie's remark as to the importance of an early use of the remedy in scarlet fever, upon which he remarks :

"My own experience of the remedial value of cold bathing in scarlet fever has been ample, and of the most satisfactory kind. In addition to numerous opportunities in private practice, my position as physician of the Philadelphia Dispensary for upwards of twelve years, gave me abundant and frequent opportunities of treating this formidable disease. Without my pretending to rely on cold bathing, to the exclusion of other remedies, I can safely say that there is no other one which unites, to anything like the same extent, efficacy with safety, and immediately pleasurable results, as the cold bath. How often have I seen the little sufferer, with burning heat and delirium, and unable to obtain sleep or repose of any kind, tranquillised immediately by the cold affusion, and fall into a sweet and refreshing sleep immediately afterwards." (p. 366.)

Having had opportunities of using the remedy in similar cases, we can fully confirm this testimony.

*Rubeola* has been treated with as great success as scarlatina, by the cold affusion. Dr. Bell quotes the experience of Thaer, who, in the autumn of 1825, treated 68 cases with only one death; and in that case pulmonary tubercles were present, and ablution had been practised contrary to medical advice. It was remarked, that the children who had been bathed were generally quite well in eight days, and the desquamation was less extensive and more rapid in its course. The conditions for using the cold affusion or sponging were, that the temperature of the patient should be above 98°,—that there should be restlessness and shortness of breath,—that the water used should be the cooler as the heat of the patient was the greater,—and that it was inadmissible when the patient was tranquil or perspiring. Frölich's table for regulating the use of the remedy with regard to these conditions is a useful document, and will, we believe, be found applicable to the treatment of every kind of fever. We, therefore, give an abstract of it from Dr. Bell's work :

Heat of the body.	Temperature of the water.	Duration of ablution.	Minutes in the bath.
98°	90½°	4 minutes	—
99½°	85°	" "	—
100°	75°	" "	1 to 1½
101°	65° — 70°	6 "	1 " 2
102°	60° — 70°	4 to 6 "	2 " 3
103°	60° — 70°	8 "	6 " 8
104°	60°	" "	3 " 4
105°	55°	" "	2 " 3
106°	40°	" "	1 " 3
107°	40°	" "	1 " 3
108° to 111°	35°	" "	3 " 4

*Inflammatory diseases* have been also very successfully treated by cold bathing, even of a kind which the received pathology would rigorously exclude from so much as an experiment on its use, on account of its supposed danger. The application of cold has long been a recognised method of treatment in cutaneous inflammations, or inflammation within the cranium,—in the latter case by the iced cap and the cold affusion. In gout and rheumatism the cold bath has been found to be eminently beneficial, although its safety has been frequently contested. At the beginning of the century Dr. Kinglake kept up a warm controversy in its favour, and Dr. Good used it in his own case. The great cause of apprehension was in the tendency to *metastasis*, in inflammations of this kind, from the surface and extremities to the viscera, which it was feared the cold bath might and did (in fact) induce. More modern pathological views, however, give little importance or prominence to this doctrine of a metastasis of the “peccant” humours, although the humoral pathology has lately received a new impulse. It appears rather to be probable that it is a special condition of the seat of the so-called metastatic action which renders it liable to inflammation, and that the old dogma *ubi irritatio ibi fluxus* contains the theory of this form of morbid action, rather than the doctrine of “revulsive” action. The wet-sheet has lately been substituted for the cold affusion and cold bath in cases of this kind; and as we propose treating of its remedial action separately, we need not dwell here upon the special indications to be noted in treating inflammatory diseases of a gouty or rheumatic character.

*Modifications of the Cold Bath.*—Certain modifications of the cold bath have been adopted, both of local and general application. The more important of these are the “wet-sheet packing,” the douche and cold affusion. It is of importance to remember, however, that in estimating the experience of hydropathic practitioners, there are important *adjuvantia*, of equal, if not greater, therapeutic value than their peculiar methods. In many cases treated by the wet-sheet, as in those in which dyspepsia and its accompanying ills supervene upon hard mental labour, the mere rest of the brain from every source of anxiety and pain, and from all conceivable forms of mental excitement, will do much towards the restoration of the health. Hence the “hydropath” enjoins “a total relaxation of the brain.” So also that large class of ills which are induced, or, at least, much aggravated by undue excitement of the sexual organs, are brought much under control by carrying out “an invariable hydropathic rule,” as Dr. Johnson informs us, namely, “that whichever is the invalid, a temporary separation be enforced.” Dr. Johnson frankly adds, “This, by the way, is a secret which alone is capable of effecting many important cures. It is most essential to insist upon this disunion in all cases of general debility, nervousness, dyspepsia, and hypochondriasis, of both the male and female sex. It is equally indispensable in all kinds of diseases connected with the womb, all hysterical affections, &c.” Add to these *adjuvantia*, a restriction from the luxuries of the table; a denial of wine, ale, and all alcoholic drinks, and of tea, coffee, and all nervine stimulants; early rising, with vigorous exercise, and we need not wonder that the “wet-sheet” with such aids has done marvels, which the practitioner in our civic populations will never equal by its use alone. Nevertheless, we can look at the “wet-sheet” as one of the most universally applicable, and therefore



one of the most valuable means of applying cold water that we possess. In the larger proportion of families a cold bath is impossible, and even the cold affusion almost impracticable, whereas in an equally large proportion the wet-sheet is ready and acceptable.

The wet-sheet, as experimented with by Dr. Johnson, is a compound bath, and consists, firstly, of a cold bath; secondly, of a tepid or mild vapour bath; and, thirdly, of a transition bath,—the shallow bath. The cold bath stage continues for about five minutes, namely, from the first envelopment of the body by the cold wet-sheet, to the time when the temperature of the latter is raised by the heat given off by the patient to about 93°. The patient's body remains in contact with the wet-sheet at this temperature for a variable period, when he is divested of the "packing" and placed in a shallow bath, containing fifteen gallons of water at 40° to 50°, in which he is rubbed for one minute or one minute and a half, and then placed in a drying sheet. It is obvious that such a process admits of very considerable modifications, and of a corresponding varied therapeutic action. If, for example, the intermediate stage or the tepid bath be omitted, it is, in all essential points, a cold bath, and as such is applicable to the various forms of febrile and inflammatory disease already enumerated. Hence we believe that Dr. Johnson's statements as to its excellent effect in soothing pain, allaying irritation, and exercising a general tranquillising power in febrile affections, are correct. His cautions as to its use, the reader will observe, perfectly coincide with these of Currie and others in recommending the cold affusion.

"But this remedy should always be used in the middle or hot stage, if possible. It must never be employed in the cold one, and rarely, if ever, in the sweating one, certainly *never* if the perspiration be at all profuse, or if it has been going on for some time..... Whenever there is a sense of chilliness present, or the skin is not hot and dry, or there is any considerable perspiration, then the cold application must be postponed to another opportunity." (pp. 69, 70.)

And again Dr. Johnson observes:

"He will content himself with merely laying down one most important practical rule in the treatment of all fever by the wet-sheet. It is this, *never apply the sheet in fever except the pulse be more rapid than normal, and the skin more heated. Otherwise it will inevitably do more harm than good; and if the skin be chilly and the pulse very feeble, even though it may be fast, it might, perhaps, be fatal.*" (p. 77.)

The great reduction in the pulse, effected by the application of cold water to the surface, indicates the wet-sheet as a powerful antiphlogistic remedy in inflammatory diseases. Looking at the remedy in this respect, Dr. Johnson boldly treated a case of "suffocative bronchitis" by the wet-sheet only. His history is so extraordinary, while the remarks of the relater are so charlatanic, so offensive to good taste, and so derogatory to the honour and dignity of the profession, that we take special notice of it. It is as follows:

"On the 25th November, 1849, I was summoned to the bedside of a patient labouring under a severe attack of acute inflammation of the lungs. She was a married woman, aged 39, of a rather corpulent habit. She had been troubled with a cough for three or four days, which was sufficiently severe to deprive her of all appetite, but not hitherto to confine her to her room. When I saw her, her cough was very distressing, and her respiration accomplished with

considerable difficulty. She complained of pain at the pit of the stomach, and displayed the usual symptoms of fever, namely, a furred tongue, great thirst, accelerated pulse, hot skin, &c. In a few days all these symptoms were aggravated. Her cough became violent and prolonged, and suffused the whole countenance with a dark red hue, making the veins stand out tinged with blood. What little expectoration was with immense difficulty and suffering achieved, was transparent, frothy, and very tenacious, indicative of the internal inflammation burning within. The breathing became noisy, and so short, that the patient was obliged to have her head and shoulders raised for fear of absolute suffocation. The pain in the chest assumed a lancinating stabbing character, as though a knife were passing through her side. In one word, the sword hung over her head suspended by a hair, and death might have claimed his own at any moment. When, lo! her symptoms were gradually ameliorated, and in the end, she totally recovered. But how did she recover, when she commenced to improve? not slowly, not gradually; on the contrary she galloped on to convalescence as rapidly as she had fallen ill. In *ten* days she was out of bed, and might have been sooner, but as there was no necessity for it, I did not wish to hazard a relapse. In *fourteen days* her cough had vanished entirely. And what was her treatment? No bleeding! no leeches! no blisters! no cupping! no mercury! no purgatives! It would be useless describing it in detail, since no two cases can be treated exactly in the same way. Suffice it then merely to say, that the case was conducted hydropathically, and that the processes administered consisted of the wet-packing, the tepid shallow bath, and the copious imbibition of cold water.

"When my patient was quite well, she told me that she had had three similar attacks, but *none so severe as the last*. For all of them she had been treated in the Infirmary of Manchester, (I mention this to show that she had the best professional advice that could be procured,) and was treated in the most generally-approved manner." (p. 82.)

Dr. Johnson then details "the most generally-approved" manner of treating bronchitis adopted by the ordinary practitioners, as in this case. In the first attack, "*bled in BOTH arms,*" "*cupped twice in the chest,*" "*fifteen leeches were applied and one blister.*" In the second attack, "*cupped to the extent of sixteen ounces of blood; the teeth of thirty leeches were allowed to lacerate her skin at one sitting; mustard poultices were brought into play; and her back was burnt in three or four places by a hot iron, as a counter-irritant.*" In the third attack, "*treated in the same hospital, by probably the same physicians, she lost sixteen ounces of blood by cupping, and as much more as would flow from the bites of from . . . sixty to seventy-five leeches.*" And so Dr. Johnson occupies above two pages with matter like this, evidently with the view of enhancing the therapeutic value of his own peculiar method in the estimation of lay readers, by depreciating, and (as we believe) most incorrectly stating that of his professional brethren. So flagrant an outrage in professional etiquette can only be atoned for by a most ample explanation and apology to those gentlemen who were physicians to the Manchester Infirmary from the autumn of 1844 to the autumn of 1846. The "blarney," (we may here observe,) with which Dr. Johnson adorns his own case, and the cant and egotism with which he describes the pretended treatment of his neighbours, indicate that his book is especially intended for the lay reader, and that the dedication of his work to the London colleges is a sham. We need hardly say, that to the medical practitioner the "case" displays internal evidence of its doubtful authenticity; while the disingenuousness which could permit so flagrant a breach of professional morals, renders the whole of Dr. Johnson's experiments *suspect*.

If, indeed, the researches of others were not, to a certain extent, corroborative, we should have thought it our duty to have passed them over without notice.

Reverting from this digression to the great point—the therapeutic value of the wet-sheet in pulmonary inflammation—what does the case, if truly related, teach us? Simply nothing. Will Dr. Johnson take an early opportunity of publishing the notes of this case, so that we may learn to what extent and when the wet-sheet was applied, and to what extent the pulse was diminished in frequency. “The wet packing, the tepid shallow bath, and the copious imbibition of cold water,” were the means used; but this statement, brief as it is, betrays a sad discrepancy on the part of Dr. Johnson; for at the close of his lucubrations on the case, he adds:

“Before proceeding farther, lest the reader should fall into an erroneous notion, it may be as well to mention that . . . . accordingly as the attack is severe must the wet-sheet wrapping be short in duration, and quickly repeated. And this must be done *without the intervention of the shallow or any other bath*. When this repetition is required, there must be two bedsteads in the patient’s room. While he is lying packed in one, the other must be prepared, so that he can turn out of one wet-sheet into another immediately. The frequency of this change must depend, as before mentioned, upon the ardency of the fever.” (p. 85.)

This marvellous “case,” be it observed, is in all respects a pattern case; the fever “ardent,” the “inflammation burning within.” Was the shallow bath used, or was it not? If it were, how could the wet-sheet be repeated, as we are again told (p. 93) it should be, “every ten, fifteen, twenty, or thirty minutes, without the intervention of any other bath. If it were not used, why has Dr. Johnson asserted that it was?

It is a general opinion that the wet-sheet is a powerful sudorific; but Dr. Johnson denies that it is so, under any circumstances, from no other considerations than those which arise out of half a dozen experiments and a few theoretical ideas; these are, however, so insignificant that we need not occupy space with an analysis.

The *warm bath*, Dr. Bell observes, has not been duly appreciated, partly from false theories regarding its operation and effects, and partly because it has been confounded with the *tepid* and *hot* bath. The temperature of the warm bath we have already fixed at a mean of 95°, or at a maximum and minimum of 92° and 98°. The temperature at which the water of a warm bath is pleasantly warm to the sanguine and plethoric with an active circulation, will be the minimum; while those who have a cool surface and extremities, and sluggish circulation, will prefer the maximum; to this class of persons, at the lower temperature of 92°, it is a *tepid* bath. The warm or tepid bath has been found serviceable in those forms of febrile diseases in which the cold bath would have a too sedative effect. Dr. Currie states, however, that in some cases the heat of the body is lowered more speedily by the tepid than the cold effusion. To attain this, it is necessary to allow the body to be exposed to the external air during the interval of time between the affusions, so as to allow evaporation to take place. Currie also justly remarks, that the tepid bath is little if at all stimulating and heating, because it does not, like the cold affusion, rouse the organism to those actions by which heat is evolved, and the effects of external cold resisted. Currie used it principally in the febrile affections of children, in which he found it to produce very generally

a considerable diminution of heat, a diminished frequency of the pulse and respiration, and a tendency to repose and sleep. He appears to have tried it in hectic fever in his own case, which is detailed in the *Zoonomia*, where we find it stated\* that "he had repeatedly seen the hectic paroxysm prevented or cut short by careful ablution of the naked body with tepid water."

When the tepid bath is thermometrically a warm bath, it is really stimulant and restorative. In ancient times it was universally used as a means of refreshing the wearied traveller, either by total immersion, or as a pediluvium. The physiological effects of the warm bath are fully stated by Dr. Bell; and he relates the results of experiments made by himself and others. Like the cold bath, it diminishes the frequency of the pulse; and the more frequent the latter is, and the more it deviates from the natural standard, the more readily is its frequency diminished in the bath. There is also a retardation of the respiration, correspondent to that of the circulation. It increases both the absorbing and exhaling functions of the skin, contrasting in this respect with the cold bath. After a prolonged stay in the bath, there is an increased secretion of limpid urine, and less usually an evacuation from the bowels. Its operation on the nervous system is of a soothing character. "A pleasing languor gradually, and almost imperceptibly, steals over the brain and senses, and produces a strong inclination to sleep, which, as I have repeatedly ascertained in my own person, may be yielded to in the bath, to one's great comfort and enjoyment. The aching and concomitant weariness caused by protracted or violent exercise or labour are entirely removed by the warm bath." (Bell.)

It is in this way that the hygienic effects of the warm bath are restorative in their character, approaching very nearly indeed to the effects of sleep. If to the warm bath, Dr. Bell observes, be added the oriental accessory of gentle friction of the skin, the person subjected to it "will more than realize all the boasted effects of animal magnetism." Dr. Bell mentions a point of resemblance not noticed by writers, between the cold and warm bath, of which he can speak from frequent personal experience, namely, that in from one to three hours after its use, the skin, especially of the head, becomes hotter, and some thirst is felt, especially if no exercise has been taken after the bath, or the immersion has been of short duration. We think it is certain that much of the benefit derived from the use of thermal mineral springs is due simply to the warmth of the water. Mr. Edwin Lee quotes "the worthy Dr. Fenner," as speaking in raptures of the effects of the thermal baths at Schlangenbad, "*vous sortez des Eaux de Schlagenbad, rajeuni comme un phénix; la jeunesse y devient plus belle, plus brillante, et l'âge y trouve une nouvelle vigueur.*" This pleasant influence, Mr. Lee attributes to the saline matter contained in the water, because baths of common warm water "tend to debilitate the system, and diminish the vital energies of the skin, rendering it more susceptible to the influence of atmospheric vicissitudes." Further inquiry will convince Mr. Lee that he has simply endorsed a popular error, and confounded a hot or tepid bath with the warm bath. Dr. Bell states on this point that he "can freely declare, that during the many winters in which I have used the warm bath, so far from my liability to catch cold having been increased

\* Vide vol. ii, p. 296.

by it, I have actually suffered less than heretofore, in this way." On the other hand, there are thermal springs with little more solid contents, if any, than common water, which are equally efficacious against rheumatism, and equally pleasant as those of Schlangenbad. At Leuk, for example, as Dr. Forbes informs us, in his charming 'Physician's Holiday,' the water, if it were not so hot, might be regarded as a good drinking water, only rather hard. Indeed, Mr. Lee need go no further than his own book for facts of this kind. At Wildbad, for instance, the amount of saline and gaseous constituents in the water is exceedingly small, "the saline residuum after an analysis of a pint of water, scarcely three grains;" yet at Wildbad, and several of the French baths where the water is very similar, bathing is "exceedingly agreeable and refreshing, and tranquillising and sedative," and beneficial to gouty, rheumatic, and paralytic cases.

*The best times* for using the warm bath are the same as stated for the cold bath, namely, when the stomach is empty. Persons advanced in life, or who are readily heated and cooled, or prone to vascular and nervous excitement, will find the warm bath more suitable and beneficial than the cold. Persons fatigued or exhausted by labour, or other depressing agencies, should give the preference to the warm bath. The duration of the period of immersion seems to be of little consequence in many cases. Dr. Forbes states, that at Leuk (the bath being taken twice a day), the daily stay in the water is from one to eight hours; namely, from one or two to five hours in the morning, and from one to three in the afternoon. Of course, occupation and amusement must be found for the bathers, and we accordingly find that they have each a floating table for their books, snuff, flowers, &c. This prolonged stay in the warm bath seems to be, indeed, the most customary and longest established method of using it. Charlemagne, it is said, held his levees while in the bath at Aix-la-Chapelle; and it is well known, as we have already stated, that the public baths at Rome became, at last, the resort of the idle and luxurious, who found a prolonged stay in them to be a pleasant mode of dissipating their time.

*The therapeutical effects* of the warm bath are numerous, and can hardly be stated within the limits we have allotted to this article. As a sedative in all maniacal, neuralgic, and hysterical affections, and in the neuroses generally, it is much extolled; and probably it would be found, that the cases of this kind are most benefited, in which the morbid condition of the nervous system is connected with an arthritic or rheumatic cachexia or diathesis. In the last-mentioned class of diseases, the warm bath is the remedy *par excellence*; and hence we find that all thermal springs, with hardly an exception, have a reputation for curing gout and rheumatism. During his period of office at the Bath Hospital, as the resident medical officer, Dr. Tunstall had ample opportunities of observing the efficacy of the thermal baths and waters at Bath, of which he availed himself by accumulating records of cases treated at the hospital, and analysing them. His experience does not differ from that of his predecessors. He found that, of all the neuroses, chorea is most benefited by the Bath waters, and then sciatica and neuralgia; next, paralytic affections, principally "dropped hands and feet," caused by the absorption of lead and other minerals. Cutaneous diseases, including lepra, psoriasis, eczema, impetigo, pityriasis, porrigo, tinea capitis and their various subdivisions and complications, together with chlorosis, anæmia, and general debility, are



all relieved or cured. But cases of gout and rheumatism far outnumber all the others. "In the first of these," Dr. Tunstall remarks, "the Bath waters have been used with the most signal and universal success . . . . . They deserve, indeed, to have accorded to them a specific action upon this disease, as far as relief is concerned." Paraplegia or hemiplegia, if from mineral poisons, are readily cured; but if from structural changes, are not influenced. Epilepsy, recent apoplexy, phthisis, and scrofula, cardiac disease, hæmorrhages, acute diseases, and chronic abscesses or ulceration, are not suitable cases. Dr. Tunstall, we ought to say, fixes the temperature of the bath at a degree which constitutes them *hot* baths. From the following sentence it will be seen, that Dr. Tunstall has yet to make himself acquainted with the physiology and therapeutics of warm bathing, whether the water be from thermal springs or be heated artificially:

"With regard to common water heated to the same temperature, no person could immerse themselves in a water bath of 105° or 110°, without severe suffering; whereas the former is the ordinary temperature employed at the Bath Hospital, the patients describing it as warm and pleasant. And I have seen a person taking a bath at 114° without any inconvenience; but this was only done as an experiment, to determine the heat that could be borne. As a contrast it may be stated, that a *hot* water bath varies from 98° to 100°, which is the temperature of a *warm* thermal bath. Moreover, the effects of the hot water bath are relaxing and sedative, those of the hot thermal bath bracing and tonic; there is no disposition whatever to sleep after it; on the contrary, the brain is more active and the spirits more buoyant. The one produces loss of appetite and depression, the other an increase of appetite and exhilaration." (p. 4.)

From this statement, taken in connection with some preceding observations, we infer, that Dr. Tunstall holds opinions like those of Mr. Lee, and means to advance for the Bath waters some peculiarity in the acquisition and communication of *heat*, and that that peculiarity is to be attributed to their solid constituents. How much all this is an assumption, is shown by the fact, that some waters, with as little, if not less, an amount of solid constituents, and others used at a *lower* temperature, have all been found equally beneficial in those forms of disease for which the Bath waters are recommended. Thus the springs of warm and hot *pure* water at Matlock, Gastein, Pfeffers, Leuk, Wildbad, Plombières, Luxeuil, of acidulous and saline water at Buxton, Bristol, Schlangenbad, of hot alkaline water at Vichy, Ems, Töplitz—to mention only a few—have all been pronounced equally beneficial in gout and rheumatism with the Bath waters. Perhaps the most perfect contrast is Wildbad, where the *pure warm* water at 98° is successfully used in identically the same diseases as the *hot saline* water of Bath at 105°;—we refer not merely to gout and rheumatism, but also to cutaneous diseases, paralytic affections, and—what is particularly significant—chlorosis and sterility. Dr. Tunstall's hypothesis, that the special beneficial effect of the Bath waters in most of these diseases is due to the iron contained in them, is certainly incompatible with the results of treatment at Wildbad.

A general review of the therapeutic effect of warm bathing has convinced us that, although much is due to the use of the bath, much, as in the case of the cold bath, is also due to the *adjuvantia*. This principle of treatment has been already pointed out, as being of essential importance to

the hydropathic method of cure by the wet-sheet, and the various other external uses of *cold* water peculiar to that method. "To live on sixpence a day and earn it," is a maxim of gouty therapeutics, which contains all the pretentious philosophy of the actively therapeutic part of the *adjuvantia*. With the quick eye which competition excites, Dr. Tunstall points out this weak point in the hydropathic empiricism in treating the disease termed cerebropathy by some, by others nervousness, and by Dr. Tunstall "brain-fag,"—a disease of literary, political, and professional men—of men who have changed night into day, either in the pursuit of science, literature, or pleasure, and robbed the brain of the repose necessary to its vigorous action. In such, a hypochondriacal condition verging upon insanity is the real state; the brain is enfeebled, the mind is in a degree imbecile, the imagination predominant. It is with this disease upon them, that men of refinement, of genius, of learning, of high station in their respective walks, fall a prey to quacks, religious and medical, and become the subjects of homœopathic, hydriatic, or mesmeric treatment; or, still worse, abandon friends, and the healthy useful employments of vigorous manhood, for the pursuit of ecclesiastical phantoms or the rigors of an ascetic "retreat." The following is the remark on this class of cases by Dr. Tunstall on the *adjuvantia* we have referred to:

"The accounts of the benefits derived from the cold-water cure have reached us by the publications of those who evidently laboured under the disease I am now treating of, in its first stage. It would not be difficult to judge, from the writings of Sir Edward B. Lytton and Mr. Lane, how much benefit they derived from the absence of mental fatigue, and the enjoyment of the pure air of the Malvern Hills, in pleasant society,—the latter, with the diet enjoined, was the adjuvant, the hilarity produced by the former, the cure. Now, at Bath, I have seen patients cured, who have in various parts of the kingdom tried the cold water cure without relief; nay, a few years since, one of these establishments was started in this city, without success, because its projectors forgot that here we had springs which *could* cure the diseases which the cold water system *professed* to do." (p. 91.)

In this class of cases, there is a more legitimate remedy than these empirical appliances; and that is, a pedestrian tour, such as Dr. Forbes enjoyed, and has described in his pleasant 'Physician's Holyday.' Let the man of refinement and imagination, who is pestered with thick-coming fancies, especially after reading "the Fathers," and feels that he has lost the healthy, noble feeling of self-reliance which characterises the true man, flee to the mountains for solace, rather than to an ascetic, enthusiastic priest. Let him defer the performance of what he thinks to be a duty, and the practice of what he yearns for, as a refuge from his gloom, until he has strengthened *the organ of thought*, and enjoys a *mens sana in corpore sano*; without this, his sacrifices and martyrdoms are but the self-imposed evils of a foolish hypochondriac, and of no religious value whatever. If, after breaking away from all his engrossing studies, and holding converse with nature in her sublimest aspects—drinking nothing more potent than water—walking twenty miles a day, and every evening taking a warm bath—if, after a three months' pedestrian tour in the Tyrol, Switzerland, or Scotland, so conducted, he returns to the world, and finds its aspect towards him unchanged, and he has no desire to do his duty—*solid* duties—actively and earnestly,—then there is nothing for him but to "retreat," and live amidst the phantoms and chimeras which are to

his taste. "Hellebore" will not cure him; Bath, the Brünnen, and Malvern will be alike useless; and even the false miracles of mesmerism will "pale their ineffectual rays" before those of another class, which, to his morbid imagination, appear real.

*Transition baths.* Our readers will, we trust, excuse the digression we have just made in regard to the importance of the subject, for it is clearly of vital interest to distinguish between those results which may be fairly attributed to the external use of water exclusively, and those in which the bath is really only the means whereby other remedial agencies are brought to bear on the invalid—almost without his knowledge. For it is the business of the practitioner to treat the multitude:—to cure the million, with whom a visit to Malvern or Bath is a sheer impossibility. To these he can administer water in various forms, and so secure for his patients all the advantages which it can afford; but the adjuvantia are beyond his power. Now, there is yet another method of bathing to be considered, which is duplicate, that is to say, includes both the cold and warm bath. This method consists in the use of a transition bath—a method practised almost universally. We quote from Dr. Bell's volume on this point:

"By transition or succession baths, I mean the quick passage from a cold to a warm, or even hot, medium, whether this be air or water, as from the frigidarium to the calidarium, or the reverse, as in the Roman thermæ, and from high heat to cold, as in the Russian and Indian baths. The Greeks must have very early resorted to this course of bathing; for Homer, in the 'Iliad,' describes Ulysses and Diomed, on their return from the Trojan camp, with the captured horses of Rhesus, to have first bathed in the sea, and afterwards to have refreshed themselves with a warm bath. The Lacedæmonians used the cold bath, by immersion in the Eurotus, and afterwards subjected themselves to a dry sudatory of air heated by a stove. From them, the chamber used by the Romans for a similar purpose was called *Laconicum*. By means like this, the whole animal economy undergoes a certain degree of commotion, extending to even the minutest organ, and portion of organ of the body. The circulation in the most important membranes, those lining the stomach and the lungs, or the cavity of a joint, is subjected to a kind of exercise; and not only is the secretion of sweat increased and arrested in alternate periods, but we have good reason to believe that the secretions from the internal membranes are also modified in a similar manner. These effects are further increased by the auxiliary manœuvres just mentioned (friction, &c.); so that when this more protracted and complicated practice of bathing . . . is gone through, more varied and more satisfactory results are obtained, than in our common fashion of passive bathing by simple immersion, and a hurried and too often imperfect drying of the skin." (pp. 171-2.)

When the shallow bath follows on blanket-packing, or the wet-sheet, it is a transition bath. We have already referred to its physiological effects, under these circumstances. Dr. Bell made experiments upon himself, with a view to ascertain the effect of transition baths. Half an hour after noon, he went into a bath at 84°, with his pulse at 78°. He felt an unpleasant shock, attended with shivering, and his pulse fell in two minutes to 60, but rose in five minutes to 66. Leaving the bath for three minutes, the pulse rose; and fell, on re-entering it, in two minutes to 60. He then raised the bath to 94°, the pulse rising to 62; then to 100°, the pulse beating 78; then to 106°, when in two minutes it was at 84; and in five minutes at 100. He then left the bath, experiencing all the physiological effects of a hot bath,—skin very hot and quite red, face flushed, arteries

of the head and neck throbbing, and giddiness, with perspiration beginning to be profuse. Reducing the temperature of the bath to  $84^{\circ}$ , he again immersed himself; in two minutes the pulse fell to 80, and in five minutes to 73, the heat and flush having disappeared, and the feelings become natural. Seventeen years subsequently to these experiments, Dr. Bell took repeatedly the Russian, or, as the proprietor termed it, the Oriental bath.

“After being immersed for a short time (eight or ten minutes) in a vapour bath of from  $100^{\circ}$  to  $105^{\circ}$ , I received a shower bath at a temperature of  $65^{\circ}$  to  $70^{\circ}$ , for a very brief period, not exceeding a minute. In this way the excitement manifested by heat and redness of the skin, flushed face, frequent pulse, fulness of the head, and thirst, caused by the vapour bath, was removed by the cold shower; and the whole system was brought to nearly its customary state.” (p. 173.)

Dr. Bell observes, that, by this transition bath, he escaped the feverish excitement which the hot or vapour bath leaves, while he escaped the general aching and discomfort which a plunge or douche bath at the same temperature would have left.

The *hot water*, *hot air*, and *hot vapour* baths, are all analogous in their effects, and may often be beneficially followed by the transition bath. The practice has the undoubted stamp of universal experience, and we are satisfied that the reintroduction of the method in our private establishments will tend to popularise them, and extend their advantages. We had intended to have dwelt more upon this portion of the subject, but our narrowing limits warn us to desist. We cannot conclude this article, however, without pointing out to the practitioner the necessity that even now is laid upon him (and which will continually increase), to become thoroughly acquainted with the physiology, hygiene, and therapeutics of bathing, and the hydriatic regimen. The coming establishment of public baths in all our large towns, will place within his reach a curative means of wide application, while the price at which the baths will be sold, will render them accessible to all classes. He will therefore be responsible for their due use, and will have to guide public opinion in this respect.

The imbibition of water as a medicine, and its topical application in disease, remain to be noticed, and may occupy another article. With regard to the merits of the books before us, we can only repeat the very favorable opinion we have already expressed of Dr. Bell's well-stored volume; it will be a valuable addition to the library of every practitioner. We regret we cannot equally recommend the other works noticed in this article; but as they are addressed to the popular reader as well as the medical workman, they will doubtless be circulated, or circulate, amongst those for whom they are designed. Mr. Lee is an experienced *litterateur*: the literary merits of the other two writers, we regret to say, are below mediocrity,—damaged, we fear, by the double attempt to please the public and appear professional.

## ART. X.

*On the Therapeutical Application of Electro-Magnetism in the Treatment of Rheumatic and Paralytic Affections.* By ROBERT FRORIEP, Doctor in Medicine and Surgery, Public Professor of General Surgery in the University of Berlin, &c. &c. Translated from the German by RICHARD MOORE LAWRENCE, M.D., Surgeon Extraordinary to H.R.H. the Duke of Saxe Coburg and Gotha.—London, 1850. 8vo, pp. 205.

IN the Third Volume of our present series (p. 373), we had occasion to take a favorable view of the efforts made by some practitioners in this country to establish the therapeutical value of electricity in certain classes of disease, especially those of a paralytic nature, endeavouring at once to steer clear of the indiscriminate recommendations of former times, and the unaccountable neglect of the present. Among these Dr. Golding Bird has been pre-eminent; and the results of his investigations, detailed in his Lectures at the College of Physicians in 1847, (and since republished from the 'Medical Gazette,') and set forth with that happy union of scientific research and practical tact, for which the productions of his pen are remarkable, are most encouraging, and cannot fail to exert great influence in promoting the extension of this interesting investigation. Although we are not aware that any substantive works upon the subject have of late appeared on the Continent, with the exception of that of M. Pallas, noticed in the article already quoted, and the one now under review, yet the journals of France, Italy, and Germany, abound in examples of the medical application of the various forms of electricity, plainly exhibiting that this agent is resorted to with a frequency, a perseverance, and a degree of success, to which we are strangers; many cases, which by us are abandoned as incurable or hopeless, being there either cured, or at all events, materially relieved.

Professor Froriep's little work embraces very many of the cases treated of by Dr. Golding Bird; but he explains the cause of the success he has met with in their treatment upon different grounds than those usually received, or than will, perhaps, even now be admitted. Of course, the main point is the fact, that electro-magnetism has been resorted to by both these practitioners with equally favorable results in certain classes of cases; but it is obvious that, in proportion as we can determine the true *modus operandi* of the curative agent, the extension of its applicability becomes more easy, and what is not less important in one of so powerful a nature, this may be restrained within limits in which it may be safely resorted to. The great error hitherto has been the indiscriminate use of electricity by incompetent hands, whence has arisen the double error of resorting to it in cases not suited for its application, and abandoning it in others in which its effects, though attainable, are not always at once perceptible.

Dr. Froriep believes, that the affections benefited by electro-magnetism are all *rheumatic* in their character, whether putting on the form of pain, convulsion, tremor, paralysis, tumefaction, or atrophy; and that its operation is, so to say, indirect, consisting in procuring the absorption of the *effusion* induced by the rheumatism. This effusion, he believes, in those cases simulating true neuralgia and paralysis, has been overlooked by for-



mer writers, no one before him having indicated its diagnostic and pathognomic value. Though most frequently occupying the subcutaneous cellular tissue, the effusion may also take place into that beneath the aponeuroses, or that of the muscles, into the periosteum, or the serous cavities. So essential in the author's view is the establishment of its existence, even in cases in which its presence might not be suspected, had not attention been thus drawn to it, that we must make rather a long extract from his description of the appearances it manifests :

" 1. *Effusion into the cutis*. Induration (thus Dr. Lawrance translates the word *schwoile*) is thus distinguished; the skin, without being much raised, seems much thicker, of a peculiar, almost cartilaginous stiffness, can only be pinched up in a thick fold, can be moved over the cellular tissue below, though not so freely as healthy skin, especially over bony surfaces. A fold of skin pinched up looks tumid, thick, and smooth, and if more compressed has a shining appearance, which in healthy skin it is almost impossible to produce. The colour of these places is mostly lighter, whitish, or fallow, which marks the effusion very distinctly; they can also be recognised by the touch. The effect of stimulants (which produce *cutis anserina* in the healthy skin) is very peculiar on these places. If by applying a powerful current to the cutaneous nerves, *cutis anserina* appears on the skin of one of the limbs, those parts which are the seat of rheumatic effusion are not affected, but remain quite smooth. This is most striking, when, owing to the treatment employed, the effusion has decreased; then the *cutis anserina* may be seen encroaching on the former smooth surface in stripes or spots, giving a spotted appearance to the skin. These spots gradually disappear, and with them the complaint, except where the effusion is not confined to the cutis, but implicates deeper-seated structures. \* \* \* \*

" The second and most frequent form of rheumatic effusion is that which takes place into the subcutaneous cellular tissue, *induration of the subcutaneous cellular tissue*. This is found either in small circumscribed spots, or spreading over a limb, half the body, or the whole body. The effusion which is confined to parts supplied by the ramifications of a nerve is remarkable; it can be recognised at once by a slight swelling, and by the skin over it having a shining appearance, and not admitting of being pinched up into a fold. The swelling is firm, sometimes hard, and does not pit on pressure, unless that has been employed for some time. When these effusions are circumscribed, their margins may be distinctly felt, and the adjoining cellular tissue is pliable and easily pinched up into folds. Where they are extensive, the surfaces over which the effusions are spread look swollen and disfigured, the skin is smooth and shining, but seldom changed in colour, and the margins are clearly defined, especially where the cellular tissue lies over a hard surface, as the forehead or front of the leg. When they are diffused over an entire limb, as the leg, they cause a tenseness of the skin, which renders it firm and unyielding. The surface is shining and full of indentations; and where the effusions are widely spread, the colour is changed to a blueish red, the skin cannot be pinched up in folds, the surface is cool, and the patient does not always complain of pain. The effect of the electro-magnetic current on these effusions is very marked; the application of it turns the surrounding skin red, whilst that over the induration remains pale, defining its extent. *Cutis anserina* is produced, provided the electrical stimulus be sufficiently powerful. These callous spots are generally extremely sensitive of the stimulus of electricity, and the passage of a current will discover their presence by rendering them very tender; but it also happens, that where the effusion is considerable the sensibility is lessened, or in places completely destroyed. This form of effusion generally succeeds an attack of acute rheumatism, affecting different parts of the body, as the shoulder and upper arm, &c., and is accompanied by chronic rheumatic pains, which continue for years, gra-

dually depriving the patient of the use of his limbs, and not remitting until absorption of the induration has been effected, during which process the power of motion returns, and the pain increases or decreases in exact proportion as the induration improves or grows worse.

"Isolated rheumatic indurations are found in spasmodic contractions, but whether or not as the cause of the same I must leave undetermined. More extensive effusions are followed by stiffness, trembling, or by paralysis; by the last especially, in those parts where the superficial cellular tissue is not separated from the muscular by aponeuroses. Rheumatic effusion into the cellular tissue of the face is generally accompanied by paralysis of those muscles supported by the facial nerve. A slighter degree of extension beneath the skin of the face gives rise, though seldom, to a diseased excitability of the muscles, which become subject to spasmodic twitchings, being no longer under the influence of the will. A peculiar variety of these indurations in the cellular tissue is a less indurated form, met with in persons wasted away from chronic rheumatism, or where a fractured limb has been kept motionless for some time. We sometimes find, at the most pendant part of a limb, for instance, the ulnar side of the forearm, or back of the upper arm, a loose pale swelling, which hangs like a bag. The surrounding cellular tissue is soft and yielding, but that of the swelling is firm, hard, and painless, and does not pit on pressure.

"These indurations are the seat and constant attendants of rheumatic pains, which spread from them over a whole limb, causing severe pain in one of the adjoining joints, undergoing exacerbation on each change of the weather and in high winds. As they become absorbed, which, under any treatment, takes place slowly, the pains also subside. These indurations are very painful on the passage of an electro-magnetic current, which produces no change of colour. They are not painful when pressed, and the skin over them maintains its sensibility unimpaired. Chilblains resemble these indurations, in which not only the cellular tissue but also the corium is indurated, and of a blueish red colour. The treatment of chilblains is well known; but it may not be so well known that similar swellings arise on the backs of the fingers and hands in rheumatism, especially in partial rheumatic paralysis, which can be cured by the stimulus of the electro-magnetic current." (pp. 5—11.)

The next form of effusion described is that which occupied the *muscles* giving them a hard, unyielding feel, and sometimes leading to true atrophy. The cellular tissue around the muscle may be soft and yielding, or may more or less participate in the induration. The flexor power of the muscles is more or less lost, and even under a powerful current they remain passive—exhibiting here a striking contrast to what takes place in paralysis from disease of the nervous system, and furnishing an important means of diagnosis. The impairment of function may vary from mere stiffness to entire loss of voluntary movement. In other cases a trembling is induced by each attempt at motion, which may go on to complete *paralysis agitans*. When only certain muscles are the seat of effusion, it is confined to such as are supplied by one branch of a nerve, and various distortions may be produced by the action of the nonparalysed muscles. When effusion is confined to a group of muscles, there is generally found over this a distinct, circumscribed induration of the cellular tissue, which may indeed be considered as diagnostic of this cause of paralysis. There are usually also, but not always, accompanying lancinating pains, much influenced by weather. Another important symptom is the decreased sensibility of the cutis over the induration, there being an actually decreased temperature here, and the surrounding parts being of a red or reddish-blue colour. The temperature, appearance, and actions of the limb are renewed,

in proportion as the absorption of the effusion is effected by the agency of electro-magnetism, shampooing, warm baths, iodine, alkaline lotions, and the promotion of local transpiration.

Another form of effusion induces *periosteal induration*, which, however, seldom exists alone without other of the tissues being implicated. Small swellings are induced, which may be termed rheumatic nodes; and where these cannot be felt, their presence can be ascertained by the pain and heat which the passage of the electro-magnetic currents causes in the exact spots. The spontaneous pain is, too, of a very severe and of a lancinating character; and these cases furnish the most obstinate examples of rheumatic affections, requiring energetic and persevering treatment. The author does not believe in the existence of the *effusion* into the *sheaths of the nerves*, assumed by many as explanatory of the symptoms of rheumatic neuralgia.

In the author's view of the nature of the case, the rheumatic or neuralgic disease must continue until the exudation is absorbed, as such effusion mechanically obstructs the influence of the *vis nervosa*, and it indicates the relaxed condition of the vessels and the diminished irritability of the nerves upon which its original production depended; our object is, therefore, to invigorate the nervous system generally and locally, and to restore it to its healthy state of excitability, while we promote the removal of the effusion which has already taken place. To these ends electro-magnetism is a powerful agent, to be employed, however, in co-operation with the various other therapeutical agents indicated by the case. Dr. Froriep employs Saxton's machine. Currents are transmitted through the medium of sponges saturated with salt and water, and applied to the cutis, by acupuncture, when action of an energetic character, or on deep-seated parts, is required, or by introducing two platinum needles in the course of a nerve. As, however, he gives no details as to the mode and duration of his manipulations, those who are about to have recourse to this means for the first time must seek such information elsewhere, and cannot do better than refer to Dr. G. Bird's work.

The demonstrative portion of the Essay consists in the succinct narration of seventy-five cases in which this means has been resorted to with success; adjuvatory measures, in some instances, having been purposely abstained from, in order to exhibit its efficiency in a still stronger light. Satisfactory and encouraging as are the results thus set forth, a more faithful picture of the exact value of the remedy would have been obtained, had the author communicated the particulars, or at all events the number, of those cases in which he had unsuccessfully employed it. The great *desideratum* in the investigation of the powers of any new medicinal agent, is the publication of the *whole* of the cases to which it is applicable, that have come under the notice of its advocate since he has been aware of its existence; stating the successful and the unsuccessful results in the cases in which it was resorted to, and the reasons for declining its employment in those in which it was not. The publication of merely successful results, by encouraging exaggerated expectations and indiscriminate experimentation, has proved highly detrimental to the true progress of medical science. It may be said, that in pointing out the distinctive sign of rheumatic induration, as he terms it, Dr. Froriep has sufficiently indicated the class of

cases in which his remedy is useful ; but is it intended to be inferred from this, that none of those presenting it have resisted the operation of electricity, and that this agent has been of no avail where such sign was absent ? Although this is not expressly stated, the general tenor of the work leads to such an inference ; and if this conclusion were a sound one, the importance of electro-magnetism could hardly be exaggerated, whether considered in a therapeutical or a diagnostic point of view. Before stating the reasons that lead us to doubt this, we will enumerate more particularly the varieties of disease, of which the author narrates examples. These are, painful affections of various parts of the body, whether preceded by distinct attacks of acute rheumatism, by this disease in its more chronic form, or mere exposure to cold ; a great number of paralytic affections, from hemiplegia simulating that arising from cerebral disease, to partial paralysis of various muscles of the extremities, and of the portio dura supplying the muscles of the face ; examples of cases usually regarded as especially neuralgic in their character, as *tic douloureux*, *lumbago*, and *sciatica* ; and various spasmodic affections, such as *chorea* of the face, and the writer's cramp. We think it unnecessary to detail any of these cases at length, as the general statement may be made, that in all of them, although some had endured for years, this rheumatic induration was detected, and that all of them yielded, when this became absorbed under the influence of electro-magnetism.

The general impression left on our minds after the perusal of the work is, that it contains strong evidence of the importance of this agent in a great variety of neuralgic and paralytic affections ; but we consider the author's appellation, *rheumatic*, applied to these, to be far too indiscriminate. The cases in which distinct attacks of rheumatism have preceded are very few in number ; and unless exposure to cold be considered as being capable of alone inducing this particular affection, we are at a loss to know why, in the great number of cases given, they are termed rheumatic at all. That exposure to cold will induce neuralgia and paralysis is well known, and the importance of distinguishing such cases from such as arise from disturbance of the nervous centres is admitted ; but it seems to us that this would be much better accomplished by the term *peripheric*, which only implies the locality without prejudging the nature of the operating cause. How vaguely the term is employed by Professor Froriep may be judged of by the fact, that he describes one of his cases as a "rheumatic gum-boil," while in very many others, in which pain is a prominent symptom, there seems to be no reason whatever for assuming rheumatism as the cause of this. It may be said that he has furnished us with a diagnostic mark in the form of the effusion which he describes as present in all these cases ; and although we are not disposed to view this in some of them as conclusive of the rheumatic nature of the disease, we cheerfully admit he has done good service in pointing out a local sign, which if confirmed by subsequent observation, may aid greatly in the effectual direction of the employment of electricity ; but if it is meant to be inferred that such cases alone are of a peripheric nature, and alone suited for the advantageous operation of this agent, we must join issue with the author, and express our conviction that affections of the extremities of the nerves may be induced, independently of centric causes, whether by the direct operation of cold or by

the reflex operation of a disordered digestion, with which rheumatism has nothing to do, in which local effusions do not exhibit themselves, and for which electro-magnetism may yet prove a highly beneficial agent. In so saying, we are yet mindful of the caution required in speaking of a new sign, for whose presence we have never expressly searched, or at all events in the manner the author recommends, and which, indeed, he believes has been hitherto overlooked from attention not having been specially directed to its recognition; but we have cases in mind, in which so scrutinising has been the examination, that it is certain no local induration of the kind could have escaped observation. Especially do we remember the care with which such local examination was instituted in a case of neuralgia of the arm, in which certain symptoms of a centric character led the professional man, in whose person the case occurred, to the groundless fear of its serious nature, and who would gladly have seized hold of any physical sign of its localization in the extremity, had any such presented itself to his repeated and anxious investigation. Although, however, we cannot agree with Dr. Froriep, that the action of electro-magnetism is always indirect upon the lesion of the nerve, and contingent upon the induction of the absorption of rheumatic effusions, we repeat that we consider his statements and experience upon the subject to be of great value, and to call for the attentive consideration of the profession.

Dr. Lawrance's translation has been executed in fluent and idiomatical English; and we feel much indebted to him for undertaking a task so usually found thankless and profitless in this country.

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#### ART. XI.

1. *On Animal Chemistry, in its Application to Stomach and Renal Diseases*. By H. BENCE JONES, M.D. A.M. F.R.S., &c.—London, 1850. 8vo, pp. 139.
2. *Elements of Urinary Analysis and Diagnosis, Chemical and Microscopical*. By ROBERT VENABLES, A.M. M.B., &c.—London, 1850. 8vo, pp. 66.

It is well known to the medical world, that Dr. Bence Jones has been assiduously engaged for some time past, in investigating the nature of the changes to which the composition of the Urine is subject in health and disease. Many of the results of his inquiries have been recorded in the 'Philosophical Transactions;' others have been published in the 'Transactions of the Medico-Chirurgical Society,' and a few have been communicated to the medical journals of the day. In the work before us, the author has brought together the general results of his labours, and has presented them to notice in the form of twelve lectures, which were given at the conclusion of the course of Chemistry at St. George's Hospital, in 1849. The lectures are arranged under the following heads: Lecture 1. On Food. 2. On Digestion. 3. On the Blood. 4. On Calculi. 5. On the Quantity and Acidity of the Urine. 6. On Uric or Lithic Acid. 7. On Oxalate of Lime and Sulphates. 8. On the Alkaline and Earthy



Phosphates. 9. On Alkalescence of the Urine from fixed and volatile Alkali. 10. On Albuminous Urine. 11. On Diabetes and Diuresis. 12. On the Relation of the Urine to the Food and Animal System. General Method for its Examination.

The first three lectures embrace so much of the subjects of food, respiration, digestion, and the blood, as serves to indicate their influence on the quantity and quality of the urine. In these portions of the work, the author dwells on the fact that all our foods are primarily derived from the vegetable kingdom, and that they always contain four classes of compounds, viz., water, nitrogenous matter, non-nitrogenous matter, and fixed saline substances; all of which compounds are absolutely necessary to the growth and nourishment of the animal body. He points out, moreover, that animals have not the power of forming these compounds, or even of transmuting them into each other, but merely of appropriating them, and ultimately of reducing them into effete products; the last change being effected by the agency of respired oxygen: concerning which, he remarks, that "the clear comprehension and investigation of this action of oxygen in the human body is of no less practical importance than the great and fundamental principle of the circulation of the blood." (p. 9.) In the last lecture Dr. Jones again enters on the discussion of this subject; but he here states that:

"If too much oxygen is taken (inspired), the body will waste; if too much non-nitrogenised food is taken, the deposit of fat may constitute disease. To preserve health, there must be some relation between the amount of muscular action—that is, exercise or labour—and the supply of nitrogenised food. By considering, then, how much air and exercise is taken, we ought to determine how much of the non-nitrogenised and nitrogenised food is required for the repair of the wasted muscles and tissues, and for the support of animal heat." (p. 9.)

Digestion is regarded by our author an act of solution and absorption; and in explaining the manner in which the solution of our food takes place, he seems to us to attach too much weight to the supposition that *free* muriatic and phosphoric acids are poured out into the stomach during the digestive process, leaving their equivalents of alkali or of base in the blood. Nothing can be more open than to doubt than this; in fact, the investigations of Hunefeld, Blondlot, Barreswill, Bernard, and Lehmann, have gone far to prove that the gastric juice never contains free muriatic acid, and it is highly probable that the same is the case with regard to free phosphoric acid. To speak, therefore, of the existence of these acids in the stomach; to speculate on the manner in which they are secreted; to attempt to decide on the influence which their removal from the circulation must have on the composition of the blood and urine; and to endeavour to explain how they act in performing the function of digestion, must be regarded, to say the least, as rather premature and profitless attempts on the part of our author. They are likely to lead the reader to suppose that the whole phenomena of digestion are accurately determined, and that they are no other than simple acts of solution; when in point of fact they are among the most complicated and incomprehensible phenomena which present themselves to our notice. In the digestion of starch, cellulose, fat, and other non-nitrogenous matters, we know that there is always something more than a mere solution of the body acted on; for

the starch, the gum, and the cellulose are each made to assume a new molecular arrangement, and, as it were, to retrograde into the condition of lower vegetable products; cane-sugar also becomes glucose; and oily matters, in undergoing the process of digestion and absorption, are never taken into the system as fats, but are decomposed and robbed of their glycerine-base before they are admitted into the circulation.

The truth is, that the whole subject of Digestion is involved in the greatest obscurity; for we have no certain knowledge of the condition in which the several acids exist in the stomach; we are totally ignorant of the nature, and *modus operandi*, of that curious complex principle termed pepsin; and we are in the greatest doubt concerning the changes which proteinaceous matters undergo, when they are submitted to the action of the gastric juice. Have we, then, at the present time, any right to assert so positively as our author does, that digestion is merely an act of solution, followed by absorption?

In discussing the phenomena of Assimilation, Dr. Bence Jones proceeds with more caution; for he states that, "the chemistry of assimilation—that is, the chemistry of the formation of fibrin and blood-globules, is at present almost entirely unknown to us, and, therefore, it is well called a vital process, and belongs as yet to physiology and not to chemistry." (p. 28.)

When referring to the morbid conditions of Blood, the lecturer states, that his own observations quite confirm the results arrived at by Dr. Garrod, respecting the presence of lithate of soda in the blood of gouty patients; and he quotes two cases in which he readily detected the existence of that compound in the blood drawn from the arm.

Concerning the production of Urinary Calculi, the author remarks, that their formation may be dependent on one of three or four causes:

"There may be too much of any substance for the water to dissolve; or too little of the water for the solution of the substance; or the solvent property of the water may be lost in consequence of some third substance being present in it, or being absent from it. By each of these methods different natural analyses, so to speak, of the urine may be made, and in each of these ways different urinary calculi may be formed." (p. 34.)

In this lecture Dr. Bence Jones has given plain directions for the analysis of urinary calculi, and he has appended a table, wherein he has set forth the reactions by which the individual deposits may be recognised.

In speaking of the quality and quantity of the Urine as voided under different circumstances, Dr. Bence Jones states, as the result of his experiments, that—

"While exercise reduces the quantity of urine secreted, and as a consequence increases the specific gravity of that fluid, diet, whether animal or mixed, has but little, if any, influence over the quantity of urine secreted; while, if the quantity of urine be the same, the specific gravity is increased by an animal, as compared with a mixed diet." (p. 43.)

This is in accordance with the results obtained by Lehmann. Dr. Bence Jones believes that the formulæ given for the determination of the amount of solid matter contained in urine, are not of any practical value; for his experiments indicate that there is not any constant relation between the specific gravity of the fluid, and the quantity of solid extract contained in it.

He finds that the acidity of the urine varies with the state of the stomach; that when the latter is quite empty, and is not engaged in doing the work of digestion, the urine is very acid; but that as soon as the stomach becomes charged with food and begins to secrete gastric juice, then the urine loses its acidity, and, in many cases, becomes alkaline: so that there appears to be an inverse relation between the acid conditions of the stomach and the bladder. Other circumstances, as the nature of the food, &c., are likewise found to affect the acidity of the renal secretion; for instance, he noticed that—

“When animal food only was taken, the diminution of the acidity after food was more marked and more lasting than when a mixed diet was taken. The increase of acidity before food was hardly so great as when mixed diet was taken.

“When vegetable food only was taken, the decrease in the acidity was not so great as when animal food was taken,—that is, though the urine became neutral after food, it did not become highly alkaline. The increase in the acidity of the urine was higher before vegetable food than it was before animal food was taken.

“The influence of animal food in lessening the acidity of the urine, and the influence of vegetable food in increasing the acidity, are in remarkable contrast. Possibly the effect of oil, starch, and sugar on the acidity of the urine may be separated, and the influence of each may be proved. That these three substances, after they are taken into the stomach, probably pass through various neutral and acid states before they become carbonic acid and water, I have already mentioned to you. The urine may from these substances receive many acids which, when animal food alone is taken, may be altogether absent. That vegetable acids partly pass off by the kidneys, the experiments with tartaric acid sufficiently prove.” (pp. 48 and 50.)

Respecting the influence of acids and alkalies on the reactions of the urine, he makes the following observations:

“Dilute sulphuric acid was taken in large doses, but it did not produce a very decided effect. Nine drachms of dilute sulphuric acid in three days slightly diminished the decrease in the acidity of the urine after food; but the acidity before food was very slightly, if at all, increased thereby.

“By comparing the acidity of the whole quantity of urine passed in twenty-four hours for three days when no sulphuric acid was taken, with the acidity when nine drachms of dilute sulphuric acid were taken during three other days, the increase in the acidity, though not very marked, was decidedly apparent. The average quantity of carbonate of soda required to neutralise the whole of the urine made in twenty-four hours, when no sulphuric acid was taken, was 15·39 grains. The average quantity required when sulphuric acid was taken, was 20·38 grains. There was but little difference in the quantity of urine made.

“Tartaric acid, in large doses, produced a decided effect on the acidity of the urine: 354 grains of dry and pure tartaric acid, taken in three days, increased the acidity of the urine, but in that time it did not render the effect of digestion on the reaction of the urine less apparent than it was when no acid was taken.

“Liquor potassæ, in large doses, produced a decided effect in diminishing the acidity of the urine; but it by no means renders the urine constantly and permanently alkaline. Its effect seems to pass away rapidly. An ounce of strong liquor potassæ, taken in three days, did not counteract or conceal the influence of digestion on the reaction of the urine.

“Tartrate of potash produced a most decided and rapid effect on the acidity of the urine; 120 grains of pure dry tartrate of potash, dissolved in four ounces of distilled water, made the urine alkaline in thirty-five minutes. In two hours the alkalescence had disappeared; but after the next meal, the effect of the tartrate of potash was again apparent. Ten drachms of tartrate of potash, in three days, produced but

little, if any, effect on the acidity of the urine after it had been omitted for twenty-four hours." (p. 48.)

To complete these investigations, we have still to learn what are the effects of hydrochloric, phosphoric, butyric, and lactic acids, on the state of the urine; for, if the author's views are correct, concerning the nature of the acids which effect the solution of the food in the stomach, it is evident that the first two of this series should have the power of rendering the urine decidedly acid. We doubt, however, whether such would be found to be the case. On the contrary, we think that the results already obtained, warrant the conclusion that the mineral acids are not so potent in their effects on the urine as the organic.

The nature of the acid secreted by the kidneys is not yet determined; but Dr. Bence Jones believes, in common with Dr. Golding Bird, Liebig, and many others, that it is most probably an acid phosphate of soda; though, in some cases, he has noticed that the quantity of this body present has not been sufficient to produce the effects observed. (May it not be, as Lehmann supposes, an organic acid?) He positively denies that the acidity is even due to the existence of an acid lithate of ammonia; asserting, in opposition to the statements made by Drs. Bensch and Prout, who entertain such an opinion, that this compound has no real existence, but that the body mistaken for it is a mere mixture of uric acid and urate of ammonia.

He believes that uric acid commonly exists in the urine in the form of lithate of ammonia, not lithate of soda; and he is of opinion that the compound is modified in form and solubility by the presence of common salt, and of other saline substances which exist with it. He finds, moreover, from experiments made in various ways, that lithate of ammonia is less soluble in an acid solution than it is in a more neutral one; and, hence, he concludes:

"That a slight excess or increase in the acidity of the urine will cause the precipitation of urate of ammonia; and the less acid the urine is, the less likely is the urate of ammonia to be precipitated.

"I have shown you how the acidity varies, how the urine becomes more acid, and more nearly alkaline, at different hours of the day, and thus the precipitation of the urate of ammonia must be greatly influenced. If the urine tends to alkalescence, an excess of urate of ammonia will be dissolved, and will show no appearance of a superabundance of urates. If the urine is highly acid, a precipitate may occur, even when no excess of urate of ammonia exists in it.

"Direct experiment shows that the precipitation of urate of ammonia depends generally on the action of both these causes conjointly. If a very small quantity of urate of ammonia exists in the urine, it may be highly acid without a marked precipitation taking place. If a very large quantity of urate of ammonia is present in the urine, it may give no precipitate if the urine be alkaline." (p. 58.)

In proof of this statement, he has extracted a table from his essay, published in the '*Philosophical Transactions*,' for 1849, which clearly shows that the mere appearance of a precipitate of urate of ammonia from the urine is not an evidence of the quantity of uric acid that exists therein, for the urine may be turbid when it contains only from 0.029 to 0.052 per cent. of the acid, and it may remain clear when it contains as much as 0.090 in the 100 parts; the difference being entirely dependent on its acid or alkaline reaction.

It appears, from Dr. Bence Jones's experiments, that, from 6 to 10 grains of uric acid are excreted daily (Lehmann found that the proportion varied from 14 to 25 grains); and it is probable, he says, that at no two hours of the day is the amount discharged perfectly similar. In fact, two quantities of urine of the same specific gravity do not by any means contain the same amount of acid; and, although it is difficult to say what it is that causes the amounts to vary, yet, Dr. Bence Jones, with Lehmann, Simon, and others, has noticed that it is influenced by the nature of the food taken. The following table represents the results of his inquiries into this part of the subject.

	Per 1000 grs. of urine.	Sp. gr.
"After animal food, the highest amount is	1·022 grs. of uric acid,	1027·8.
After vegetable food, the highest amount is	1·010 " "	1025·6.
The lowest amount, before animal food, is	0·049 " "	1024·8.
The lowest amount, before vegetable food, is	0·049 " "	1024·0.

"As regards the precipitation of uric acid crystals, this depends only on the acidity of the urine, and is quite independent of the quantity of the urate of ammonia excreted, though the quantity of crystals that fall must depend on the quantity present; but the quality of the precipitate is determined solely by an over acid state of urine, or what comes to the same thing, a deficiency of alkali there. Whenever uric acid crystals are found in the urine, it may be taken as a proof that the urine is more acid than it ought to be. Uric acid crystals are, in fact, by far the most delicate and trustworthy tests for an over acid state of the urine." (pp. 56, 60.)

The practical conclusions that are to be deduced from the preceding inquiries may be expressed in a very few words:

"1. If uric acid crystals are present, there are two things to be done—viz., to give alkalies, and to forbid and remove all that may become acid. For the first, caustic alkalies, carbonated alkalies, and earths; saline draughts, and phosphate of soda; these must be regulated according to the peculiar symptoms of the patient. For the second, vegetable acids, sugar, and starch in the food, should, as far as possible, be prohibited. The removal of acids by the skin, and carbonic acid by free respiration and exercise, is of still greater benefit; shortly, the uric acid crystals indicate that acidity must be removed, neutralized, and prevented.

"2. When uric acid crystals are absent, and urate of ammonia is deposited alone, it is a proof that no great excess of acid is present in the urine, otherwise uric acid crystals would be formed; and though, by lessening the acidity of the urine, we can keep the urate of ammonia dissolved in it, and so hinder it from appearing, yet this is not the best mode of proceeding for effecting even this purpose. An extra glass of water, or soda-water, or some slight diuretic, as nitre, is more sure to keep the urate of ammonia in solution by increasing the quantity of urine, and this is the best palliative treatment; whilst the curative treatment consists in lessening the quantity of food taken, by smaller meals, and in lessening the acidity of the urine, by increased exercise. Thus you will best prevent deposits of urate of ammonia, and the alkalies may be kept until uric acid crystals appear, when they are as necessary, as in urate of ammonia deposits they are unnecessary, for the treatment." (p. 61.)

The inquiries of Dr. Bence Jones tend to confirm the statements made by Drs. Golding Bird, Prout, and Lehmann, that octohedral crystals of oxalate of lime are to be met with under the most varied circumstances; they are associated with all sorts of deposits, as with mucus, pus, lithates, lithic acid, triple phosphate, and bone earth, though they are found most frequently in the company of uric acid and the urates. Judging from these



circumstances, Dr. Bence Jones has been led to conclude that the presence of oxalate of lime in the urine does not indicate disease, but merely disorder of the digestive system. In searching for this salt, he advises us to let the urine stand for twenty-four hours, before we decide whether the compound is present or not; for he has known cases in which the urine, when first passed, has been quite free from crystals, though it has afterwards deposited them in great abundance. This would lead us to suppose that the oxalates are secreted by the kidneys in a soluble form, and that their precipitation is dependent on reactions which take place at a subsequent period.

In addition to the two forms usually assumed by this salt, he has seen the compound in the shape of little flattened discs, looking very much like small blood-corpuscles. Donné has also referred to this appearance; and it is proper to remember it, lest the bodies in question be mistaken for blood-discs.

“One word as to treatment. In cases of indigestion with oxalate of lime, ammonia, mineral acids, quinine, and iron are indicated. Animal diet, cold bath, and brandy and water, appear frequently to give much benefit. When there is irritable bladder, with excess of mucus and epithelium, camphor, and compound tincture of camphor have proved very useful. If blood-discs occur with the octohedral crystals, the presence of gravel or calculus in the bladder or kidneys becomes probable.

“That the use of distilled water, instead of spring water, will lessen the quantity of lime passing through the body, is also most certain. By this means the oxalate of lime may be somewhat lessened, but by air, exercise, and careful diet, the disorders which accompany it may be cured.” (p. 70.)

Dr. Bence Jones has examined the urine for the purpose of determining the influence of food, exercise, and medicine on the proportion of sulphates present in this liquid; and the general conclusions at which he has arrived, are as follows:

“1. That the sulphates in the urine are much increased by food, whether it be vegetable or animal.”

The increase in each of these cases is no doubt due to the sulphur contained in the food.

“2. Exercise does not produce so marked an increase in sulphates.”

This conclusion is opposed to the results obtained by Drs. Lehmann and Simon, both of whom have found that the sulphates are increased after exercise; and they have even laid considerable stress on the fact.

“3. Sulphuric acid, when taken in large doses, increases the sulphates in the urine; in small quantity it produces little or no effect.

“4. Sulphur when taken increases the sulphates in the urine, and sulphate of soda or magnesia produces the greatest effect on the quantity of sulphates in the urine.”

Very little can be deduced from these investigations at present, but the author expresses a hope that he may be able, at a future time, to point out the variations of sulphates in disease, and also to prove on what those variations are dependent. An inquiry of this kind will no doubt yield important results; for long since, the experiments of Simon led him to conclude that the amounts of alkaline sulphates fluctuate very considerably in disease.

In speaking of the presence of phosphates in the urine, our author directs attention to the fact, that it is customary to form an estimate of the proportion of these salts present, by merely looking at the quantity of white earthy matter deposited therein, whereas this is but a small proportion of the phosphatic salts contained in the urine. To form a proper estimate of them, it is necessary to take into consideration the quantity of alkaline phosphates which are held in solution, and which therefore elude our observation. Both together will give us the variations of the phosphoric acid; while the earthy phosphates, taken alone, merely indicate the quantity of earthy matter in the urine—the amount of lime and magnesia, and nothing more.

For the purpose of determining this point, he directs us to precipitate the earthy phosphates by means of ammonia, and then to throw down the alkaline associate by an excess of chloride of calcium. By operating in this way under various circumstances, he has been enabled to prove “that the earthy phosphates are rather less in quantity when animal food is taken, than when vegetable food is consumed; that they are in excess after either diet; and I found,” he says, “that even when animal food and distilled water only were taken, there was a decided increase afterwards, though much below the average of ordinary diet. The alkaline phosphates are in excess, when bread alone is taken; when meat alone was taken, the quantity of alkaline phosphate present in the urine was considerably below the average on mixed diet. By taking no food in the morning, and by taking strong exercise before dinner, I tried to determine the effect of exercise on the amount of phosphoric acid in the urine, and I came to the conclusion that the amount of earthy phosphate was not influenced by exercise, but that the amount of alkaline phosphate is increased by exercise, but not to the same extent as by bread.” (p. 83.)

Earthy compounds, as the salts of lime and magnesia, were found to increase the amount of earthy phosphates to a great extent.

As the urine is always alkaline when phosphates are precipitated from it, Dr. Bence Jones considers that the term alkaline urine is a better one than that of phosphatic diathesis, for the designation of this condition of the fluid; and he believes that it will give still more precision to our views, if, when the earthy phosphates are precipitated by carbonate of ammonia, such urine be called *ammoniacal urine*; and if, when the earthy phosphates are precipitated by fixed alkali, this urine be called *alkaline from fixed alkali*. The term, phosphatic diathesis, ought, he considers, to be used in the same sense as the term sulphuric diathesis is, viz., to denote an increase in the total amount of phosphates present, whether it be alkaline or earthy. Moreover, he states that “alkalescence of the urine, and an increase in the total amount of phosphates, have no relation of any sort or kind to one another. They are totally distinct; in fact, they are rather opposite states. At least, ammoniacal urine generally contains only a small quantity of alkaline and earthy phosphates; and urine, which contains a great excess of alkaline and earthy phosphates, is generally highly acid, and remains so above the average time without undergoing decomposition.” (p. 86.)

After a long inquiry into the total amount of phosphates excreted in various diseases, Dr. Bence Jones has arrived at the following conclusions:

"1. The variations of the earthy phosphates are so dependent on the earthy matter present in the urine, that no deduction from them as to the nature or state of the disease is possible.

"2. Neither the earthy phosphates, nor the alkaline phosphates, are permanently increased in spinal diseases.

"3. In fevers and acute inflammations of fibrous, muscular, or cartilaginous tissues, the total amount of earthy and alkaline phosphates is not increased.

"4. In chronic diseases in which the nervous tissue is not affected, no deduction can be drawn.

"5. Chronic cases of mania, melancholia, and general paralysis of the insane, gave no marked results.

"6. In chronic diseases of the brain, and in chronic and even acute disease of the membranes, there is no increase in the total amount of earthy and alkaline phosphates.

"7. In fractures of the skull, when any inflammation of the brain comes on, there is an increase of the total amount of phosphates. When there are no head symptoms, no increase of the phosphates is observed, even when other acute inflammations supervene.

"8. In acute inflammation of the brain there is an excessive amount of phosphates in the urine. When inflammation becomes chronic, no excess of phosphates can be shown to exist in the urine by the method of analysis that was employed.

"9. In some functional diseases of the brain, an excessive amount of phosphates is observable; this ceases with the delirium. Delirium tremens shows a remarkable deficiency in the amount of phosphates excreted, provided no food is taken. When food can be taken, the diminution is not apparent." (p. 87.)

With respect to the treatment of those cases in which phosphates occur in great abundance in the urine, he reminds his reader that the presence of these compounds is not a cause of disease, but merely a symptom, and that it by no means follows that the disease would be lessened by a remedy, though the precipitation of the phosphates was entirely stopped by it.

In the lecture on the Alkalescence of the Urine, Dr. Bence Jones refers more in detail to the causes which operate in giving it that character. When it arises from the presence of ammonia, it indicates that a morbid mucous secretion is poured out from the kidneys or bladder; this secretion, by undergoing decomposition, produces a molecular change in the urea, by means of which it is resolved into a new compound, carbonate of ammonia. This condition of the urine is distinguished by a ropiness of the fluid; by the odour of ammonia; by the presence of much mucus, and even of pus; by the existence of a crystalline deposit of ammonio-magnesian phosphate; and by the fact, that though it transiently restores the blue colour of reddened litmus paper, yet, in consequence of the volatile nature of the alkali present, it also acts on the blue, giving it a pinkish tint as the paper dries. Mr. Curling, of the London Hospital, was the first to direct attention to the fact, that this kind of urine owes its alkalinity to the agency of morbid mucus. ('Med. Gaz.,' vols. xiii and xviii.)

In the other case, the alkalescence is due to the presence of a fixed alkaline compound; namely, phosphate of soda or potash. Under these circumstances the urine is not ropy; it does not evolve ammonia; it contains mucus only; and on standing, it throws down a granular precipitate of phosphate of lime; besides which, it permanently restores the blue colour of reddened litmus, and consequently does not give a pink tint to the blue paper.

It is of the greatest importance to be able to distinguish these two classes of phenomena; for while the one is, as we have said, dependent on the presence of morbid mucus, and indicates serious and permanent mischief in the bladder, kidneys, or spinal cord, the other is produced by some derangement of the digestive organs, and is, therefore, a sign of less moment.

In the tenth lecture, directions are given for the discovery of Albumen in the urine; and Dr. Jones reminds his readers, that a little acid adhering to the inner surface of the test-tube, will often check the precipitation of albumen when the urine is heated in it. It is therefore necessary to clean the tube before each operation; and in order to insure success in the experiment, it is necessary to filter the fluid, then to boil it for about a minute, so as to remove the lithates and to coagulate albumen; and, finally, to test it with a drop or two of nitric acid, in which the coagulated albumen is not soluble, though the earthy phosphates, which often cause a turbidity, are.

The recognition of albumen in the urine is at all times a matter for serious consideration; and the author reminds us that the importance of the fact is much dependent on the nature of other elements present in the secretion. If on allowing the liquid to stand for a few hours, blood, and blood alone, is deposited at the bottom of the vessel, we may conclude that both of the morbid products are due to an abrasion of some part of the mucous surface. If a layer of pus subsides, then the existence of albumen is a sign of catarrhal disease; and lastly, if tubes of fibrin—casts, as it were, of the uriniferous ducts—are discoverable, we may be pretty sure that the secretion is thrown out by a granular kidney, or at the least, by a highly congested one:

“If there are no moulds, but blood-globules and pus-globules, then probably a calculus has caused, not only abrasion, but inflammation of the mucous membrane. When fibrinous moulds, blood-globules, pus-globules, albumen, and crystalline deposit were seen, then degeneration of the kidney, inflammation, and calculus, were found in post-mortem examination.” (p. 106)

There is another cause of albuminous urine, to which the author refers; and that is, the involuntary discharge of seminal secretion. This, however, may be detected by the presence of sperm filaments, and large mucous granules.

At the conclusion of the lecture on albuminous urine, Dr. Bence Jones directs attention to the existence of a new kind of albuminous matter, which he found in great abundance in the urine of a man who was the subject of *mollities ossium*. This morbid product was not immediately precipitated by heat or by nitric acid; but if after boiling the liquid, it was allowed to stand until it got cold, then a gelatinous precipitate fell, which was again dissolved by a further application of heat. The albuminous material that occasioned these changes was not ordinary albumen, but it appeared to be identical with that form of albumen which is met with in the buffy coat and in pus.

Another case is referred to by the author, in which—

“The patient had for some time been passing the so-called chylous urine, but I have fully satisfied myself that the presence of fat in the urine, on which the white colour of the urine depends, is an accident only, and by no means the most peculiar part of the disease.

"The fat passes off in the urine, making it milky only after food; but previous to any food, albumen, fibrin, alkaline salts, and more rarely a few blood-globules, are thrown out by the kidneys, and their appearance in the urine depends on the increased force of the circulation, consequent on waking, on getting up, and on exertion; for during sleep, the albumen, fibrin, &c., entirely disappear from the urine, and by directing the patient to lie in bed, or to get up and exert himself, totally different kinds of urine were passed. When perfectly quiet, the albumen and fibrin were prevented from appearing in the urine; and when he exerted himself before breakfast, these substances transuded in very considerable quantity. The urine sometimes, previous to any food, even formed a jelly-like clot, transparent, and perfectly free from the appearance of fat. I satisfied myself that the disease does not depend on chyle or fat, but on a slight alteration in the state of the kidney, and on the circulation of the blood through it. Under treatment the urine became perfectly healthy." (p. 109.)

As a test for sugar in the urine, Dr. Bence Jones relies on the reactions exhibited by the hydrated oxide of copper. In performing the analysis, he takes a drachm of the suspected liquid, and adds to it two or three drops of a saturated solution of sulphate of copper, and then two drachms of caustic potash; the hydrated oxide of copper, which is first precipitated, redissolves, if sugar or many other organic substances are present, becoming of an intense blue colour; and when heat is applied, if grape sugar be present, the oxide of copper is rapidly reduced, and a reddish-yellow sub-oxide of copper is precipitated.

The origin of sugar in the urine, the pathology of diabetes, is a matter for further inquiry; but Dr. Bence Jones believes, that the morbid product is derived from vegetable compounds, and that the disease is an error or arrest of assimilation. Under ordinary circumstances, starch is, he says, successively converted into dextrin, sugar, vegetable acid, and carbonic acid. In excessive acidity of the urine, he is of opinion, that the metamorphoses stop at the vegetable acid; and in diabetes, he thinks, the arrest occurs at the saccharine, or perhaps the gummy stage. In support of the latter view, he quotes from the writings of Thenard, Bouchardat, and Simon, in order to show that there is a peculiar tasteless saccharo-gummy matter excreted in that particular form of disease called diabetes insipidus. This matter has all the chemical characters of sugar, and the physical properties of gum; it seems, in fact, to be an intermediate principle between dextrin and grape sugar; indeed, the researches of Bouchardat and Simon go to prove, that diabetic urine may contain one or other, or both of these saccharine principles; from which it is evident, if we adopt the views of Dr. Bence Jones, that diabetes insipidus must be regarded as a disease in which the arrest of the metamorphosis takes place at a point intermediate between the gummy and saccharine stages; while the more common affection, diabetes mellitus, is one in which the changes have been carried on a degree further.

That these opinions are deserving of some consideration, is evidenced by the fact, that vegetable foods always tend to increase the amount of sugar contained in the urine; but still the hypothesis is not complete, for it gives us no insight into the cause of this arrest, and it is insufficient to account for the formation of sugar under other circumstances than those mentioned; for the researches of Bernard and Barreswill, Scherer, and others, have established the fact that sugar can be formed from the animal tissues. The former chemists have detected this principle in the livers of carnivorous



quadrupeds, and in the bodies of foetal animals, and the last-mentioned chemist has detected it in the juice of flesh ; besides which, sugar is a common constituent of milk, and must often have been produced under circumstances when vegetable matters could not have been its source ; and then, again, a connexion exists between cane sugar and gelatine sugar, the latter being, as it were, cane sugar, minus an element of water, and plus one of urea, or, to look at it in another light, it may be considered as a compound, in which a small portion of the hydrogen of grape sugar has been displaced by nitrogen. These considerations are sufficient to show, that sugar may in some cases be derived from animal compounds.

In speaking of the treatment of diabetes, our author says :

“Why, in diabetes, does the sugar in the blood not pass into the state of vegetable acid, and so on to carbonic acid and water ? Why, in health, does the sugar in the blood rapidly undergo these changes ? Our knowledge of the chemistry of the animal system is not sufficient to give as yet any complete answer to these questions. That the alkali in the blood is one of the agents in effecting this change is most probable. The relation of diabetes to acidity, points also to the want of alkali. The probable benefit of ammonia and alkaline phosphate of soda lead to the same conjecture. The tendency of the disease being to emaciation and phthisis, gives us at present the best indication for rational treatment.” (p. 121.)

And consequently the Author recommends cod-liver oil.

In the twelfth and last lecture, Dr. Bence Jones again refers to the relation which exists between the composition of the food, the blood, the tissues, and the urine ; and he directs attention to the fact, that the kidneys are never the fabricators of the materials thrown off in the urine, but are merely the strainers, as it were, of the system, and the purifiers of the blood, carrying away by their action the morbid and effete products which exist therein. At one time, every unhealthy state of the urine was attributed to an unhealthy state of the kidneys ; and no clear ideas were entertained of the relation of the urine to the solids, fluids, and gases of the body. This is not so now, however ; for, thanks to the labours of Prout, Liebig, Lehmann, Golding Bird, and Bence Jones, the rational physician is enabled to recognise the true nature of the signs exhibited by the urine, and to refer them to causes which are operating at a distance from the renal organs.

A single lecture does not give the writer an opportunity of developing his views on this subject ; but yet he has made the most of his time, for he has done much to point out the true relation which exists between the elements in question. We strongly recommend this chapter to the perusal of our readers ; and are especially pleased to find our author recanting the heresies of his first publication, when, fresh from the school of Giessen, he proclaimed, on the authority of Liebig, that *all* the nitrogenous matter of the urine is derived from the disintegration of the tissues. This grave error, which was pointed out in the pages of one of our predecessors (Brit. and For. Med. Review, Vol. XV, p. 503), he now candidly abjures. “Long ago,” he says, “Dr. Prout most fully recognised the fact, that the food not only nourishes the body, but, when excess of it is taken, passes off in part in the urine. That this double relation of the urine exists, I have also proved to myself, in opposition to the theories I had formed.” (p. 129.)

And now, in concluding our notice of this work, it is our pleasing duty to say, that it contains a great number of new and important facts, every one of which must be regarded as a valuable addition to the stock of knowledge already possessed: and it is proper to state that the work in question does not, in any way, supersede the more complete treatises of Drs. Prout and Golding Bird, but must be regarded in the light of an Appendix to their writings. In point of fact it is rather a fault in the book, that it is not complete enough; for the author rarely or never brings his own experience into relation with that of others; and though it is true that his inquiries do for the most part tend to confirm the results obtained by his predecessors, yet the reader who may be ignorant of those results is very likely to attach a false value to the opinions advanced by the lecturer. This, however, is but a minor fault, and we trust that Dr. Bence Jones will continue his independent researches, and will soon give us another opportunity of reviewing his labours; a task to which we shall apply ourselves with the full expectation of pleasure and profit.

Dr. VENABLES' little book has been written for the behoof of those who are altogether unacquainted with the modes of conducting an examination of Urine and Calculi. It contains an account of the leading properties of the several compounds met with in healthy and morbid urine; it gives the reader an idea of the proportions in which they exist therein; it points out the methods to be adopted for their detection; and, in fact, it may be regarded as a very tolerable "guide to the urino-chemical chest." This edition, which is the second of the work, contains a description of the microscopic characters of urinary deposits.

## ART. XII.

1. *Lectures on Inflammation, delivered in the Theatre of the Royal College of Surgeons of England.* By JAMES PAGET, Professor of Anatomy and Surgery to the College. (From the 'London Medical Gazette.')—London, 1850. 8vo, pp. 57.
2. *General Pathology, as conducive to the Establishment of Rational Principles for the Diagnosis and Treatment of Disease; a Course of Lectures, delivered at St. Thomas's Hospital during the Summer Session of 1850.* By JOHN SIMON, F.R.S., one of the Surgical Staff of that Hospital, and Officer of Health to the City of London.—London, 1850. 12mo, pp. 288.

IN our last Number, we devoted considerable space to an inquiry into the essential nature of the Inflammatory process; in which we brought together and compared the leading doctrines enunciated by three distinguished pathological inquirers, who had recently been devoting themselves to the subject. We now propose to conduct our readers more briefly through the researches of Messrs. Paget and Simon into the *results* of inflammatory action; a part of the inquiry to which Mr. Wharton Jones's Essay does not extend.

We must stop *in limine*, however, to examine Mr. Simon's arguments in favour of the doctrine that the Fibrin of the blood is *not* to be regarded as its most organisable portion, but as an element resulting from the de-

generation of the tissues, and destined to speedy elimination from the circulating current. In our former allusion to this doctrine, we confined ourselves to a summary of those arguments which appear to us most convincing, in favour of the commonly-received doctrine; we shall now cite Mr. Simon's objections to this, and the grounds of his adoption of the contrary view:

"First, I find that fibrin is undiminished by bleeding, however frequently repeated; nay, that it often, or even usually, increases under this debilitating treatment: its highest figure given in Andral's book (10·2) was at a fourth bleeding: and Scherer found it as high as 12·7 at the third venesection in a case of pneumonia. I find that under many other circumstances of exhaustion and weakness and inanition, during the progress of starvation,\* during diseases essentially anæmic, during violent fatigue, and the like, its proportion has been found at least as high, perhaps higher, than in the inflammatory process. And as in these respects I find its proceeding to be in direct contrast to that of the red-globules (which we know to be potential elements in the blood, and which are at once reduced by bleeding or starvation), so also do I find a similar contrast in another striking particular. Messrs. Andral and Gavarret, in the course of their extensive researches in the comparative physiology of the blood, ascertained that an improvement in the breed of an animal tended always (*cæteris paribus*) to increase the proportion of its coloured blood-corpuscles; they found that the same improvement tended likewise to diminish the proportion of its fibrin. And I find further indications of the same inverse ratio between the fibrinousness and the perfection of the blood, in the facts—that there is little or no fibrin in the blood of the fœtus, none in the egg, none in the chyme, and less in the blood of the carnivora (who feed on it) than in that of the herbivora.

"Some of these facts, derived from very different sources, appear quite inexplicable on the theory that fibrin is essential to the progressive development of the tissues; and the opposite inference seems unavoidable, that it must be considered an excrementitious product, derived from the waste of the tissues or the oxidation of the blood, and in progress of elimination from the system. This conclusion, carried into the domain of pathology, would lead us to suppose, that an augmented proportion of fibrin in the blood (whether occurring in active disease, or within the limits of apparent health) can be taken as an indication only of increased labour and waste in certain elements of the body, not of an increased development in the resources and nutrition of the blood. And on the same grounds it would appear that a super-fibrination of the blood, in acute inflammatory diseases, must be regarded as a consequence and effect of those diseases, not as their cause, and not as a primary affection." (pp. 50-51.)

Again, when speaking on a subsequent occasion, of the ulterior development of fibrin, Mr. Simon remarks:

"It appears, then, that fibrin may remain stationary, and be nourished; or it may degenerate and decay: thus much is certain. But, may it advance? may it be developed into any higher form? into any tissue?—Notwithstanding the prevalence of a very general opinion to the contrary, I believe I may venture to question its possession of this power, and may say, that I entertain extreme doubt whether, of itself, it ever shows the slightest disposition to cell-formation, or to any process of self-development.

"Unfortunately, our opportunities of watching its solitary behaviour are very few; for, in almost every instance that can be thought of, albumen (which is

\* "In analysing the blood of seventeen healthy horses, Andral and Gavarret found the maximum of fibrin to be 5 per 1000; the minimum to be 3; the mean to be 4. In dealing with diseased horses, many of them meagre and half-starved, Dr. Franz Simon found this proportion increased to 11 or 12 per 1000. In one case, particularly, of experimental starvation of a horse, after four days' total abstinence, this observer found that the animal's proportion of fibrin had risen from 5 to 9."

probably the real regenerator of the tissues) is likewise present; and that great developmental activity, so often and so glowingly ascribed to fibrin, may, with at least equal probability, be considered the work of this associated albumen, for which (on this latter assumption) the fibrin could merely be considered to furnish an inert mechanical support. For think, gentlemen, if fibrin were that restless element of growth and vital expansion which some have fancied it, what a world of activity there would be in an aneurismal sac! A large aneurism, filled with laminated clot, has almost as much fibrin in it as the whole body put together; and yet it shows, on microscopical examination, no evidence of activity or of growth. At its circumference its pressure may have irritated surrounding parts, and may have provoked inflammatory effusion from them, but in the interior all is stationary and quiet. Towards the cavity, where the formation is most recent, lie the blood-corpuscles in a network of fibrin—the former in such numbers, that the latter can but very imperfectly be seen; but in passing outwards, as the corpuscles seem more and more wasted, the fibrin begins to show more distinctly, always adapting its meshes to the material within them, so that innumerable blood-cells are seen, each in its separate setting of fibrin: in getting still nearer to the circumference of the sac, the arrangement becomes confused, from the closer consolidation of the fibrin; but in no part of the structure have I been able to see any trace whatever of new organisation.

“There is a similar reluctance to the initiation of organic development in those other intra-vascular clots which form in tied arteries. They undergo changes referable to their blood-corpuscles, and they become pale and contracted; but their fibrin may remain for many weeks, or perhaps permanently, unaltered, except for some increase of density. I have seen it after the lapse of six weeks, showing only a vague appearance of longitudinal striation, with no essential change of physical character, and without the slightest trace of new development in its substance.” (pp. 102-4.)

And after alluding to Dr. Zwicky's observations on the metamorphosis of arterial clots into fibrous tissue, which he considers as depending on “some new influence being imparted to the clot by the prolongation of blood-vessels into its substance, much more than on any specific faculty of organic development residing in the fibrin itself,” he continues:

“I do not wish it to be understood as, in my opinion, a proved and certain thing, that fibrin is insusceptible of ulterior development; but I find, as yet, a want of sufficient evidence to establish its possession of this power; and in the examination, both of intra-vascular clots and of inflammatory exudations, I find several facts which apparently militate against such a conclusion. In all such products, the fibrin has shown itself either stationary or retrogressive; either lying as first deposited, or contracting more and more densely; or altering, only to undergo degradation. So far as my knowledge extends of adhesive inflammation, and of the several reparative processes, I see no evidence that fibrin takes a more important part in them than that of holding the true albuminous blastema within its meshes, and thus occasionally serving as a provisional matrix and scaffolding for the development of cells, fibres, and blood-vessels; and I cannot but suspect, that those who have ascribed to fibrin so large a share in the processes of growth, have been somewhat under the influence of that prejudice to which I alluded in a former lecture, and have promoted this material to so high a rank in their pathology, merely because of its physical tendency to settle in a solid form.” (pp. 104-5.)

Now let us examine the chief of these arguments consecutively, and see what they are really worth.

1. “Fibrin is undiminished by bleeding; nay, it often, or even usually increases under this debilitating treatment.” This only proves that the process, whatever be its nature, on which the generation of fibrin depends, is not checked by loss of blood, a fact which affords no indication whatever that the fibrin is a product of disintegration. If the fibrin be, as we

believe, that element of the blood which is most directly and constantly required for the maintenance of the nutritive operations, it is easily conceivable that the All-wise Creator should have placed this process out of the reach of those accidents which affect the proportion of other less important elements of the blood; just as the action of the heart is entirely withdrawn from the control of the human will, and the respiratory movements are provided for by an instrumentality over which it can exert but a subordinate influence. Let it be remembered, that if fibrin be (as we believe) albumen undergoing vitalization, its *material* will always be ready, so long as there is any albumen in the blood; and if (as seems not improbable), the very act of circulation through the living tissues is one means whereby the vitalizing influence is exerted, this will be perpetually going on, so long as the circulation continues. It must be borne in mind, too, that a new supply of fibrin, as well as of albumen, is introduced into the blood after every act of digestion; for there is distinct evidence that fibrin is generated (at the expense of albumen) during the passage of the chyle through the lacteals; and if the researches of M. Cl. Bernard are correct, the liver exerts a similar elaborative agency upon the albuminous matter which has been received into the blood of the vena portæ (p. 252). Further, in all the cases alluded to by Mr. Simon, a very active inflammation was somewhere going on in the system; and thus, according to the current doctrine, an unusually active production of fibrin was taking place, at the expense of the albumen of the blood. If not generated from this material, but resulting (as Mr. Simon thinks) from the disintegration of the tissues, how is it that in an attack of pneumonia, or of acute rheumatism, in which there is no reason to suppose that any extraordinary disintegration of tissues takes place, the proportion of fibrin in the blood mounts up to three, four, or five times its normal average?

2. "During diseases essentially anæmic, during violent fatigue, and the like, the proportion of fibrin has been found at least as high, perhaps higher, than in inflammation." We very much question whether, in any of these cases, the increase in the proportion of fibrin took place without the presence of the inflammatory condition. Every one knows that inflammatory complaints are peculiarly liable to occur when the system is imperfectly nourished; and Andral distinctly states, that in the three dogs which he submitted to experiment, two of them being wholly deprived of food, and the third partially so, the elevation in the proportion of fibrin from 2·3, 2·2, and 1·6 parts, in the three individuals respectively, to 4·5, 4, and 3·3 parts, was *coincident with inflammatory changes in the stomach*. Further, as in these diseases "essentially anæmic," there is no reason to suppose that a peculiarly rapid disintegration of the tissues is going on, we cannot see how the increase of fibrin in the blood of patients suffering under them, is in the least degree confirmatory of Mr. Simon's doctrine. That the deficiency of nutrient material does not prevent the generation of fibrin out of what the blood contains, may be explained, as we have just shown, in a manner very different from that which Mr. Simon would have us adopt.

3. The argument founded upon the comparative amount of fibrin in different breeds of animals, as determined by the observations of Andral and Gavarret, can scarcely be admitted to have much weight, when it is recollected, that the breeds here spoken of were *herbivorous* animals, to



which it is natural to possess *more* fibrin and fewer corpuscles than the carnivora (see Vol. VI, p. 89), and that they were *domesticated* animals, in regard to which the phrase "improvement of breed" often means something very different from an increase of the general vigour. We can fancy that a stall-fed cow, yielding her eighteen quarts of milk daily, would be regarded by the farmer as a much more valuable animal than a hardy mountain cow affording not one third of that quantity; but the latter is well known to be the one possessing the greatest amount of vigour, and to be the least susceptible of disease. It is, in fact, the object of the breeder to produce a state of *artificial plethora*; and this condition, as is well-known, is characterised in man by an increase in the proportion of red corpuscles, without any corresponding increase, or even with a diminution, in that of the fibrin of the blood. If we extend our observations on comparative physiology a little further, we find that the red corpuscles, which Mr. Simon affirms to be "potential elements" of the blood, are altogether absent in the lowest known animal of the vertebrated sub-kingdom (we allude to the curious little *Amphioxus* or Lancelet), and that scarcely any approach to them is presented through the whole Invertebrated series, notwithstanding that among some of these there is an extraordinary amount of vital activity; consequently we cannot help looking upon their presence as connected with some function which is to be specially, if not exclusively, performed by the vertebrata. On the other hand, the presence of fibrin is universal; and its proportion in the blood is found to bear a close relation to the formative activity, in cases in which this is subject to periodical variations. Thus Mr. Newport found, that in the larva of the insect, at the period immediately preceding the exuviation of the skin and the formation of a new integument, the blood is extremely coagulable; whereas for some little time after this process has been performed, the blood is so deficient in coagulability, that it does not (as at other times) close the orifices of wounded vessels,—as if in consequence of the temporary exhaustion of the plastic material by the new production of the tegumentary tissue. In the pupa state, during which the formative activity is the greatest, and the disintegration of the living tissues must be reduced to its minimum by the complete inactivity of the animal, the coagulability of the blood is great. But in the imago state, in which little or no further growth or development takes place, and in which, on the other hand, the activity of movement might be expected to produce an unusual disintegration, the plastic element seems to be almost entirely withdrawn.

4. "Further indications of the same inverse ratio between the fibrinousness and perfection of the blood," are found by Mr. Simon in the facts, "that there is little or no fibrin in the blood of the foetus, none in the egg, none in the chyme, and less in the blood of the carnivora (who feed on it) than in that of the herbivora." We are at a loss to see what possible argument can be drawn from any of these facts, except the first; and of this—the absence of fibrin in the blood of the foetus—we must take leave to express a doubt. Mr. Simon does not cite his authority for the statement; and it is entirely opposed to all we know of the condition of the blood in other organisms, in which formative actions are going on energetically,—as in the larvæ of insects, which may be regarded as embryos fitted to maintain an independent existence, and to procure and ingest their own food. The albumen of the egg does not stand in the relation of

blood, but in that of food, to the chick ; this food has to be assimilated by the nutrient organs before it can be converted into solid tissue ; indeed, it would seem to be the special purpose of the germinal membrane to convert this raw material into the plastic circulating fluid. To advance the absence of fibrin in the egg, therefore, as an argument for its non-essentiality in the blood, is just as unsatisfactory an argument as it would be to say, that because fibrin is not to be found in bread or in milk, its presence in the blood has no relation to the nourishment of the tissues. Mr. Simon does, indeed, make a near approximation to this absurdity, in advancing the absence of fibrin in the *chyme* as one of his "indications." But what does he say to the presence of fibrin in the *chyle* ; to its progressive increase in proportion as the chyle advances along the lacteals, and more especially after its passage through the mesenteric glands ; and this under circumstances which almost exclude the possibility of attributing its first presence and subsequent increase to the introduction of any disintegrating material,—to anything, in short, but to that assimilating operation which prepares the chyle for the part which the blood is to perform, and gradually converts it into a liquid which is fit to circulate through the sanguiferous system ? We are equally astonished that Mr. Simon should think that the difference in the proportion of fibrin in the carnivora and herbivora, taken in connection with the nature of their food, furnishes any support to his views. Surely he must know full well, that the fibrin of muscular flesh is reduced back to the state of albumen in the digestive process, and that there is no difference between chyme formed from animal food and that produced by the digestion of vegetable substances, except that which depends upon the different proportions of its protein-compounds, its oleaginous matters, its saccharine and other constituents. So long as the food supplies albuminous matter in adequate amount, so long the proportion of fibrin in the blood is capable of being sustained ; and, as we have just now remarked, we can see no reason why it should not be sustained, until all the albumen of the blood is exhausted. What is the explanation of the larger proportion of fibrin in the blood of herbivora than in that of carnivora, we do not pretend to say ; but Mr. Simon's theory does not help him to accounting for it, since he will scarcely maintain that the disintegrating processes are more rapid in the former group than in the latter,—the very contrary, indeed, appearing to be the fact. It is to be remembered, however, that little reliance can be placed upon the numerical proportion of this or any other of the elements of the blood, in estimating the share which it takes in the formative processes. If the current doctrine be correct, there is a continual production of fibrin, and as constant a removal of it from the circulating current by the formative operations taking place throughout the body ; and the amount which circulates in the blood may be considered as a sort of "floating balance," which affords no indication of the magnitude of the two sets of transactions which it serves to accommodate.

Such are the merits, in our apprehension, of the principal arguments assigned by Mr. Simon for abandoning the orthodox doctrine, and embracing the heresy newly imported from Germany. We trust that we are as much inclined as Mr. Simon to exchange old errors for new truth, provided the latter can be brought to our understandings ; but we must be satisfied that it is truth, before we abandon for it the doctrines that have been current

among physiologists and pathologists ever since they began to reason correctly on such subjects. Of the objections which he raises to the current notions, on the basis afforded by the history of the ulterior development of fibrin, we shall not now stop to point out the fallacies; they will become apparent, we think, as we proceed in our inquiry.

Passing quickly over the *deliquescence* or *resolution* of Inflammation, as a process of which we know no more than it consists in a return of all the previous abnormal conditions to their natural state, Mr. Paget classifies the effects of the inflammatory process under two heads,—the *productive*, and the *destructive*. The description of the former will include the histories of the several effusions or exudations from the blood-vessels into the inflamed part, their developments, degenerations, and other changes; that of the latter comprehends the phenomena of degeneration, absorption, ulceration, and death, to which the proper elements of the inflamed part, and, with them, the organised products of the inflammation, are liable.

1. The *effusion of pure serum*, except as the result of the lowest degrees of inflammation, or as a diluent of other products, is probably a rare event. Most of what are called serous exudations contain a certain proportion of fibrin; although this may not show itself by coagulation, until some fibrinous coagulum or a piece of some organised tissue is introduced into the fluid. This was pointed out some time since by Professor Buchanan, of Glasgow. In many of the cases in which (so-called) serous effusions are poured out as inflammatory products, they may retain their fluidity for weeks or months within the body, during life, and yet may coagulate when they are removed from the body. The following case is interesting, as showing that there is some condition unfavorable to coagulation, when the fluid is diffused through the tissues, even though those tissues are dead:

“A man received a compound fracture of the leg, and it was followed by phlegmonous inflammation and abscesses up the limb. As soon as the inflammation had subsided enough, the limb was amputated; and, three days afterwards, in examining it, a quantity of serous-looking fluid oozed from the cut through the integument. I collected some of this, and it formed a perfect fibrinous clot; yet the fibrin in this case had remained among the tissues without coagulating, for three days after the death of the limb, and for many more days during the life of the patient.” (p. 15.)

This delay in coagulation is favorable to the removal of the effusion, which may be readily absorbed so long as it remains fluid; and thus we see large quantities of effused fluid, which almost certainly contained fibrin, disappearing from the inflamed parts, leaving only a very slight residue of adhesion or thickening. On the other hand, in what has been termed solid cedema, an ordinary cedematous state has given place to induration, either in virtue of the larger proportion of fibrin which was originally present in the effusion, or in consequence of the length of time which has elapsed since it was poured out. In some of the cases in which no fibrin can be found after death in the serous effusions of inflammation, it yet appears probable that they originally contained fibrin, which has already coagulated, or afforded material for the formation of corpuscles; but that the coagulum or the corpuscles have subsequently undergone degeneration and decomposition, either in the last periods of life, or subsequent to the death of the organism. The serous effusions

which are consequent upon merely mechanical obstructions or stagnations of the blood, differ from those of inflammation, according to Mr. Paget, in the deficiency of fibrin. "The fluids of anasarca and ascites," he says, "will not coagulate. They present neither fibrin nor corpuscles, except in cases of extremest obstruction, when, as in cases of ascites from advanced disease of the heart, one may find flakes of fibrin floating in the abdomen, or masses of it soaked and swollen up with serum." Now we are doubtful whether this is not too general a statement. It has been shown by the experiments of Mr. Robinson, that fibrinous exudations may be produced by pressure alone; and we well remember to have seen a case, some twenty years since, in which the fluid of ascites, drawn off by paracentesis, formed such a coagulum in the buckets into which it was received, as to remind the bystanders of painters' size. Now Mr. Paget may affirm that the fact of coagulation was an evidence of chronic peritoneal inflammation; we can only say that there was no evidence of this in the history of the case, the effusion having been, to all appearance, purely mechanical.

2. The effusion of *blood* in inflammation must be regarded as an accidental rather than an essential constituent of its phenomena; and though very characteristic of the occurrence of the process in particular localities, as in the substance of the lungs and brain, yet its presence elsewhere usually betokens an unusual proneness to hæmorrhage through softening of the coats of the vessels, or an abnormal tendency to disintegration in the red corpuscles.

"When inflammatory products present the tinge of redness, it is either because of hæmorrhage into them, or because they have imbibed the dissolved colouring matter of the blood: and when this imbibition happens during life, or soon after death, it is important as implying a cachectic, ill-maintained condition of the blood, in which condition the colouring matter of the corpuscles becomes unnaturally soluble. Thus blood-stained effusions are among the evil signs of the products of inflammation during typhus, and other low eruptive fevers, in syphilitis, and in scurvy." (p. 17.)

Now, if Mr. Simon's doctrine were true, we ought to find purely serous affections the most prone to pass into the organised condition; and the membranes lining the cavities filled with dropsical fluid ought, one would think, to become covered with new growths, formed at the expense of the albumen they contain. So far is this from being true, that, as every one knows, the tendency of any such effusion to become organised is in uniform relation with the amount of fibrin it may contain; and it is only when this is considerable, and betokens an inflammatory change in the blood and in the tissues from which it is poured forth, that we find the surfaces of the latter covered with false membranes, or bound together by adhesion.

3. The form of exudation which is most characteristic of inflammatory action, is that which is commonly known as lymph. We heartily agree with Mr. Paget in the desire which he expresses, that this product passed by some other designation than that which is already appropriated to the fluids of the lymphatics, and by some to the liquid portion of the blood. The following is his general definition of this most frequent, or even especial, form of inflammatory exudation:

"A liquid effused from the blood-vessels, especially from the capillaries, (or, perhaps, only from them,) which is capable of spontaneously solidifying or organ-

ising itself, even while its external circumstances remain apparently the same, and of which the parts thus solidified or organised may proceed by development to the construction of tissues." (p. 17.)

Mr. Paget then directs attention to the two chief forms of organisation seen in lymph, which, though often mixed in the same product, are yet so distinct as to warrant our attributing different properties to the fluids which respectively exhibit them. Those he accordingly designates as *fibrinous* and *corpuscular* lymph; the former being that which coagulates into a fibrous clot, resembling that of healthy blood; whilst the latter (the *croupous* exudation of Rokitansky) is characterised by the want of any proper coagulation, the fibrous clot being replaced by an aggregation of cells, which in their first appearance resemble very nearly the primordial condition of the corpuscles of chyle and of absorbed lymph, and the colourless corpuscles of the blood. This resemblance it is, which has led some observers to imagine that the colourless corpuscles pass *bodily* out of the blood-vessels into the exudation; but, as we ourselves long since remarked, when criticising Mr. Addison's views, the same form and condition is common to many organisms, at an early stage of their development, which at a later period show a wide difference in structure and endowment; so that no inference as to identity can be fairly based upon resemblance, at this period of their existence. To the accuracy of the following account we can bear testimony from personal observation:

"Of this primordial form we have, perhaps, the best instances in the white corpuscles of the blood, and the corpuscles of lymph in the lymphatic vessels and thoracic duct. The same form is characteristic of the corpuscles in the vesicles of herpes and eczema, and in some forms of mucus. In all these, the first discernible organic form, the form of what might with propriety be called the *primordial cytoblast*, is that of a minute mass of soft, colourless, or pale greyish-white substance, round or oval, pellucid, but appearing, as if through irregularities of its surface, dimly nebulous or wrinkled. It does not look granular, nor is it formed (as many suppose) by an aggregation of granules; nor, in its earliest state, can any cell-wall be clearly demonstrated, or any nucleus. But, as the development of this cell-germ or cytoblast proceeds, a pellucid membrane appears to form as a cell-wall over its whole surface; and now, when water is added, it appears to penetrate this membrane, raising up part of it like a clear vesicle, while upon the other part the mass retreats, or subsides, and appears more nebulous or grumous than before. In yet another state, which appears to be a later state of development, the action of water not only raises up a cell-wall, but breaks up and disperses the outer part of the contents of the cell, *i.e.* of the enclosed mass of the primordial cytoblast, and exposes in its interior a nucleus, which is commonly round, clearly defined, pellucid, and attached to the cell-wall.

"To such a nucleated cell, if we name the corpuscle from which it is developed the *primordial cytoblast* or *cell-germ*, we may assign the name of *primordial cell*. From its various developments are derived, in the products of inflammation, all the several forms of corpuscles that are described as plastic cells, fibro-cells, caudate or fibro-plastic cells, and some forms of filaments; while, from its various degenerations, descend those known as pus-corpuscles, granule-cells, granule-masses, inflammatory globules, and much of the molecular and debris-like matter that makes inflammatory effusions turbid." (p. 18.)

It is comparatively seldom, however, that either of the two typical forms of lymph presents itself in a state of complete isolation; they are much more commonly blended in various proportions, so that one or the other predominates; and it will be mainly upon the preponderance of fibrin



that its capacity for organisation will depend, whilst upon the preponderance of corpuscles will be its tendency to degeneration. It is, then, of great consequence to ascertain the conditions that determine the production of one or other of these conditions,—the *fibrinous*, which is the symbol of the *adhesive* inflammation,—or the *corpuscular*, which may be considered that of the *suppurative* inflammation. And the inquiry divides itself into three heads: 1, the state of the blood; 2, the seat of the inflammation; and 3, the degree and character of the inflammation.

The *condition of the blood*, as determining that of the lymph poured out from it, has been carefully studied by Rokitsansky, who has shown that the characters of inflammatory deposits in different diatheses correspond very generally and closely with those of the coagula found in the heart and pulmonary vessels after death. Mr. Paget has devised a means of prosecuting a similar inquiry on the living subject:

“Other evidence may be obtained by examining the products of similar inflammations excited in several persons, in whom the state of the blood may be considered dissimilar; and here the evidence may be more pointed than in the former case; for, if it should appear that the same tissue, inflamed by the same stimulus, will, in different persons, yield different forms of lymph, we shall have come near to certainty that the character of the blood is that which chiefly determines the character of an inflammation. To test this matter, I examined carefully the materials effused in blisters raised by cantharides-plasters applied to the skin of thirty patients in St. Bartholomew’s Hospital. Doubtless, among the results thus obtained, there might be some diversities depending on the time and severity of the stimulus applied; still, it seemed a fair test of the question in view, and the general result proved it to be so. For, although the differences in the general aspects of these materials were slight, yet there were great differences in the microscopic characters; and these differences so far corresponded with the nature of the disease, or of the patient’s general health to whom the blister was applied, that at last I could generally guess accurately, from an examination of the fluid in the blister, what was the general character of the disease with which the patient suffered. Thus, in cases of purely local disease, in patients otherwise sound, the lymph thus obtained formed an almost unmixed coagulum, in which, when the fluid was pressed out, the fibrin was firm, elastic, and apparently filamentous. In cases at the opposite end of the scale, such as those of advanced phthisis, a minimum of fibrin was concealed by the crowds of corpuscles imbedded in it. Between these were numerous intermediate conditions which it is not necessary now to particularise. It may suffice to say that, after some practice, one might form a fair opinion of the degree in which a patient was cachectic, and of the degree in which an inflammation in him would tend to the adhesive or the suppurative character, by the microscopic character of these exudations. The highest health is marked by an exudation of the most perfect and unmixed fibrin; the lowest, by the most abundant corpuscles, and their nearest approach, even in their early state, to the characters of pus-cells. The degrees of deviation from general health are marked, either by increasing abundance of the corpuscles, their gradual predominance over the fibrin, and their gradual approach to the character of pus-cells, or else by the gradual deterioration of fibrin, in which, from being tough, elastic, clear, uniform, and of filamentous appearance or filamentous structure, it becomes less and less filamentous, softer, more paste-like, turbid, nebulous, dotted, and mingled with minute oil-molecules.

“I would not make too much of these observations. They are not enough to prove more than the rough truth, that the products of similar inflammations, excited in the same tissue, and by the same stimulus, may be in different persons very different, varying especially in accordance with the several conditions of the blood. Yet, simple as the observations are, they may illustrate what often seems so mys-

terious—namely, the different issues of severe injuries inflicted on different persons. To what, more than to the previous or some acquired condition of the blood, can we ascribe in general the various consequences that follow the same operations in different patients? The local stimulus, and the conditions by which the inflammatory effusion finds itself surrounded, may be in all alike; but, as in the simpler case of the blister, the final events of the inflammation are according to the blood.” (pp. 19-20.)

Of course the evidence would be more complete, if the characters of the exudation could be compared in each case with those of the coagulum of the blood; such comparisons, however, cannot be frequently effected; but their results, in cases in which Mr. Paget has been able to make them, fully bear out the view that the quality of the exudation primarily depends upon that of the blood. It is almost superfluous to point out, that this is just what might have been anticipated on *à priori* grounds.

The seat or tissue which the inflammation occupies, though exerting a modifying influence, certainly does not possess that almost exclusive power of determining the nature of the product, which has been ascribed to it by pathologists who have too closely adopted the doctrines of Bichat. It has been, as Mr. Paget justly remarks, “too much overlooked that a morbid condition of the blood, or perhaps even of the nervous force, may determine at once the seat of a local inflammation, and the nature of the inflammatory product;” as, for instance, when the presence of the variolous poison in the blood determines at once an inflammation of the skin, and the suppurative form of inflammation; or, when the presence of the *materies morbi* of rheumatism in the blood determines a peculiar form of inflammation in the various fibrous tissues. This fact is now coming to be generally admitted in this country, where the *solidism* of the French school never, perhaps, acquired so firm a hold as it gained in its native country. Still, it cannot be questioned, that, other things being the same, the tissue in which the inflammation is developed exerts some influence, in determining both the primary form and the subsequent development of the inflammatory product.

“The true influence of the tissue in this respect is best shown in some of the cases in which inflammation, excited, apparently, by the same means, has happened coincidently in two or more very different parts in the same person. Thus we may find, *e. g.*, that in pleuro-pneumonia the lymph on the pleura is commonly more fibrinous than that within the substance of the lung; and adhesions may be forming in the one, while the other is suppurating. In cases of coincident pneumonia and pericarditis, the lymph in the lung may appear nearly all corpuscular, and all the corpuscles may show a tendency to degenerate into granule-cells, while the lymph on the pericardium may have a preponderance of fibrin, and what corpuscles it has may tend to degenerate into pus-cells. So, too, one may find, in the substance of an inflamed synovial or mucous membrane, abundant lymph-cells, while all the exudation on its surface may appear purulent.” (p. 21.)

We remember to have seen a case of pleuro-pneumonia, in which adhesions of considerable firmness had been formed between the two surfaces of the pleura, the lower part of the lung presented the characters of fibrinous infiltration, whilst towards the upper part the infiltration was manifestly tubercular in its character. Mr. Paget ingeniously proposes to account for the determining influence in question, on the idea that the primary product of inflammation is often a mixture of lymph and of the

peculiar secretion or other product of the inflamed part, more or less altered by the circumstances of the inflammation.

“When it is seen that in inflammations of bone the lymph usually ossifies,—in those of ligament is converted into a tough ligamentous tissue,—and that, in general, lymph is organised into a tissue more or less corresponding with that from whose vessels it was derived,—it is usually concluded that this happens under what is called the assimilative influence of the tissues adjacent to the organised lymph. But it seems more probable that no such assimilative force is exercised after the effusion; rather, we may explain the facts by believing that the material formed in the inflammation of each part partakes, from the first, in the properties of the natural products of that part; in properties which we know determine the mode of formation independently of any assimilative force.

“We have some evidence of this in the products of inflammation of secreting organs, the only structures of which we can well examine the natural products in their primary condition.” (pp. 21-2.)

The mode in which the intensity of the inflammation affects the character of the effused lymph, may be likewise explained, by taking advantage of the admitted relation between secretion and nutrition, and applying our knowledge of the mode in which the former process is altered by inflammation, to the explanation of the phenomena of the latter.

“We may therefore believe that, in the inflammation of any part, the product will, from the first, have a measure of the peculiar properties of the material employed in the normal nutrition of the part: that, as in the inflammation of a secreting organ, some of the secretion may be mingled with the product of the inflammation, so in that of any other part, some of the natural plasma—*i. e.* some of the material that would be effused for the healthy nutrition of the part—may be mingled with the lymph. The measure of assimilation to the natural structure will bear an inverse proportion to the severity of the inflammatory process, because, the more the conditions of nutrition deviate from what is normal, the more will the material effused from the vessels deviate from the normal type. In the severest cases of inflammation we may believe that unmixed lymph is produced, the conditions of the due nutrition of the part being wholly suspended; but when the inflammation is not altogether dominant, its product will be not wholly contrary to the natural one, and will, from the first, tend to manifest in its development some characters correspondent with those of the natural formations in the part. Thence, onwards, this correspondence will increase as the new tissue is itself nourished: as scars improve, so do false membranes and the like become more and more similar to natural tissues.” (p. 22.)

4. The last product of inflammation adverted to by Mr. Paget, is *mucus*. Now it does not seem to us that it can be right to speak of *true* mucus as an inflammatory product, since it is a normal secretion; and the morbid product of an inflamed mucous membrane should be compared to the morbid secretion of any other gland, in which the discharge, as Mr. Paget has previously remarked, “is usually a mixture of the proper secretion, in a more or less morbid state, and of the inflammatory product.” The altered mucus of a slightly-inflamed mucous membrane seems to consist of true mucus mingled with an unduly large proportion of epithelium cells, which are frequently cast off before they have arrived at maturity; in a higher degree of inflammation, the exudation may become fibrinous, but it is more frequently corpuscular; and it exhibits a tendency to degeneration rather than to development. The grey, smoke-coloured mucus expectorated in slight forms of bronchitis, contains a number of cells, whose contents are speckled with black granules; and Mr. Paget gives

adequate proof that these are not mere particles of soot, as is commonly supposed, but are a true pigmentary product, like that of melanotic cells.

Mr. Paget then proceeds to trace out the Development of the lymph-products of inflammation; and begins his account of it with some judicious observations on the sense in which the term "development" must be understood. In its highest sense it implies, not merely that a part becomes more fit for membership under the most perfect economy, for such fitness may be acquired by a process of degeneration; but, also, that it attains to greater complexity of chemical composition, or exhibits evidence of greater formative or other organic power, or manifests greater difference from the structure or composition of lower beings. Here, as on a former occasion, Mr. Paget adduces the process of ossification as one which is to be looked on as essentially one of degeneration, although, as regards the organism at large, it is one of development. We do not feel satisfied that he is right on this point; for in many respects bone seems to us to possess higher vital activity than cartilage, although its chemical composition is nearer that of inorganic substances; and, in the elaborateness of its construction, it is in remarkable contrast with cartilage. We shall not stop, however, to dwell upon this point; but shall quote Mr. Paget's own application of his definition to the case of inflammatory products:

"This distinction is important in the pathology of inflammation. In all true or complete development we may believe there is a larger expenditure of vital force than in any other organic act; for all such development, too, the external conditions need to be the most complete, and the least interfered with; such development is the highest achievement of the vital force—the highest instance of what might be understood as 'increased action' in a part. It is in accordance with this that, in general, development is arrested in every severe disease, and that the residual capacity of repair, and other organic processes, is, in each species, inversely proportionate to the amount of original development, or to the distance between the embryonic and the perfect forms of the species.

"To speak, therefore, of the development of inflammatory products, when already the normal development of the body is completed, may seem to imply the exercise of unusual vital force—the renewal, as it were, of the pristine embryonic vigour—and the existence of conditions more favorable for nutrition than even those of health are. But we may be led to judge differently, if it should appear that most or all of the so-called developments of inflammatory products are instances in which the tissues, though they are formed into the likeness of such as exist in the perfect human frame, yet acquire characters of lower organisation than those they had in their earlier state. It will appear that they are such; and that however much the inflammatory products may become, by their changes, better suited for the general purposes of the economy, they are, in relation to their own condition, rather degenerated than developed. The changes that they undergo are, therefore, not always declaratory of a large expenditure of vital force; they are not such as the term 'sthenic,' applied to the inflammatory process, would suggest; not such as to imply that it is an exaggeration of any normal method of nutrition." (p. 25.)

The general direction of the development of lymph, is,—first, to produce filamentous or fibro-cellular tissue; and, secondly, to assume, sooner or later, the characters of the tissue in or near which it is situate, or in place of which it is formed. This appears to us so obvious, that in no way, but by the most forced assumptions, can the probative force of the fact be evaded.

"The natural tendency of lymph to the construction of fibro-cellular or connective tissue, such as composes false membranes and adhesions, and most thickenings and

indurations of parts, is shown by the production of this tissue under all varieties of circumstances, and in nearly all parts; even in parts which naturally contain little or none. Thus it is found in the brain, and in glands, as in the testicle; within joints, even where adhesions only pass from one articular cartilage to another; in the adhesions and thickenings of the most diverse serous membranes; in the thickening of the most diverse mucous ones. And with all these we have corresponding facts in the healing of wounds: all granulations, springing from what surface they may, tend, at least in the first instance, to the formation of filamentous tissue, such as we see uniting all parts in a stump; and a large proportion of subcutaneous injuries are repaired by similar tissue, whatever parts may have been divided. And sometimes we may find instances of this development where the lymph is not even in continuity with any tissue, but floats free, as in ascites, or in effusions into joints." (p. 26.)

The tendency to the assumption of characters peculiar to the part in which it is effused, is shown in the fact, that about fibrous and filamentous parts the new tissue will present a peculiarly tough fibrous character; about bone, it will become osseous; that in the neighbourhood of epithelium will form for itself an epithelial covering; and so on. This is attributed by Mr. Paget to the properties which the lymph possesses at the time of its effusion, rather than to the exercise of any subsequent assimilative force; these properties being related to those of the natural products of the part, so that the tissue into which the exudation develops itself, comes to resemble the normal tissue of the part, just in proportion as the inflammatory action subsides into the ordinary nutritive process. We are not quite sure that this view, however ingenious, is not too material. We cannot help recognising a *force* as at work in the operation of development, (indeed Mr. Paget himself says as much;) and, as he elsewhere remarks, although "the lymph is *produced* in inflammation, it is *developed* in health, when all the natural conditions of nutrition are restored." Hence it appears to us, that whatever *capacity* for development the lymph may possess, the *power* must be chiefly supplied by the surrounding or subjacent parts; and we are disposed to look to the influence of that power, more than to anything else, as that which determines the form of the tissue. Moreover it appears to us, that there are cases in which the lymph is not developed into the likeness of the particular tissue which effused it, but into that of another which it is destined to regenerate; as, for instance, when the space between the two ends of a divided bone, from which the intervening piece may have been removed, is filled with lymph, not, we apprehend, from the bone, but from the soft tissues around; and yet that lymph shall gradually develop itself into bone. We hold it to be essential to recognise this *dynamical* element in physiological reasoning; the age is in danger of becoming too material; and humoral differences are presumed to exist, when no evidence can be given that they are so, except that which is afforded by subsequent events. Now, a piece of iron may be a piece of simple metal, or it may be a magnet; no *material* difference can be traced between the two; but a most important *dynamical* difference exists, just such, in fact, as exists between a portion of dead and of living organised tissue.

We do not think it necessary to follow Mr. Paget through his account of the Developments of Lymph; since this part of the subject was more fully presented in his previous Course, of which we have already given an account (Vol. IV, p. 419). We may simply remind our readers, that the



numerous varieties of tissue which may be included in the class of fibro-cellular and fibrous tissues, comprise, with epithelium and bone, all the normal structures that can be formed from the lymph of inflammation in Man. All these tissues are low in organisation and in vital endowments; and it seems quite natural that such should be the case, when it is remembered that, according to the view we have formerly expressed, and in which Mr. Paget fully concurs, it is an essential part of the Inflammatory process, that the proper formative power of the tissue affected should be in a depressed state.—We may stop, however, to notice some interesting observations of Mr. Simon's, on the nature of those morbid growths on the valves of the heart, which are commonly regarded as inflammatory products, but which he believes to be mere fibrinous concretions, deposited at once from the blood:

“From a variety of reasons, it seems almost certain that the ordinary nutrition of the lining membrane of the circulating system is derived directly from the blood with which it has contact; and that its morbid changes depend—not on any inflammatory condition due to the *vasa vasorum*, but on those humoral changes, those variations in the qualities of the blood, by which it is more immediately and more certainly affected than any tissue of the body.

“On these grounds there would be great *prima facie* difficulty in believing that the endocardial deposits could be of inflammatory origin. But this is not all; for if they were inflammatory exudations, why should they be so peculiarly limited to projecting or uneven surfaces of the lining membrane? And why should they evince so decided a preference for the *left* side of the heart? Both sides of the heart, and all points of each cavity, are (one would think) equally exposed to the causes of inflammation; the coronary arteries supply both ventricles of the heart indifferently, and we well know that acute *peri-carditis* pays no respect to the grooves and septum of the heart; it traverses all such lines of demarcation, injects the blood-vessels of right and left side alike, and covers ventricles and auricles equally with its dense inflammatory exudation. On the supposition that these vegetations are inflammatory effusions from the membrane, I should be quite unable to explain why they should almost entirely confine themselves, as they do, to the valvular apparatus; and why their predilection for the left side should be so great, that the right is very rarely affected—perhaps never, except where the left has first suffered, and where the disease has been of such extreme intensity, that even its weaker affinity for the right side has been able to manifest itself.

“The opposite doctrine is the more tenable one. I believe that the origin of these vegetations is directly humoral; that they arise as fibrinous precipitations from an overcharged solution; the valves encrusting themselves with fibrin, just as a stick in certain streams coats itself with a calcareous envelope; and that the preference shown for the left side of the heart admits of explanation by reference to the peculiarities of its contents—the new-made arterial blood.

“You will observe that this theory involves the supposition, that arterial blood is more prone than venous blood to precipitate its fibrin, either as containing more of it, or as containing it in some more separable form.

“Not wishing to leave this a matter of uncertainty, I have experimented on the subject. I have carried a single thread, by means of a very fine needle, transversely through the artery and vein of a dog, leaving it there so that it might cut the stream; and I have done this repeatedly, sometimes in the femoral vessels, sometimes with the carotid and jugular, sometimes with the aorta and cava. I have suffered the thread to remain during a period of from twelve to twenty-four hours. My experiments have given me as a uniform result, that the arterial blood with the utmost readiness deposits its fibrin on the thread; the venous blood with the utmost reluctance. And in most of my experiments, the thread, where it traversed the canal of the artery, presented a very considerable vegetation on its surface,

(exactly like those we are talking of on the valves of the heart;) a vegetation sometimes as large as a grain of wheat; always of a pyramidal shape, with its apex down-stream, and its base attached to the thread. In the artery, one might say that the thread whipped the blood, just as one whips blood in a basin to get the fibrin out of it; but with this trifling difference, that, instead of the rod beating the fluid, the fluid ran over the rod and precipitated its fibrin there. In the vein, the thread seemed to operate no way but obstructively; never coating itself with fibrin, but sometimes delaying or stopping the circulation with a voluminous black clot, chiefly collected on that side of the thread remotest from the heart. Accordingly, the general statement and rationale of the matter appears to be as follows:—the disease in which these deposits are so frequent is one of intense over-fibrination of the blood, and one in which almost certainly there are other conditions, besides *quantity*, making the fibrin easy of precipitation; the left side of the heart has preference, because it is the arterial side, and because arterial blood, as we have seen, readily parts with its fibrin; the valves, and particularly their streamward surfaces, are chosen for the deposit, because their position exposes them chiefly to the friction of the current; so that the whole curious selection of site for the deposit resolves itself into the concurrence of two conditions, which are fulfilled in that one spot of the vascular system—namely, the greatest chemical tendency to the deposition of fibrin, with the greatest mechanical facilities for its entanglement.” (pp. 55-7.)

It is to be regretted that Mr. Simon does not confirm his views, by a comparative microscopic analysis of the “vegetations” formed upon his thread, and of those which are found upon the valves. The following circumstance, however, which we remember to have seen many years ago, lends probability to his view. A patient labouring under pericarditis, and what was believed to be incipient endocarditis, having been bled, the upper edge of the coagulum presented a number of little wart-like excrescences, whose resemblance to the “vegetations” of the valves struck every one who looked at them. At that time, microscopic examination of such bodies was almost unthought of.

The history of the Degenerations of Lymph constitutes one of the most important parts of the whole Pathology of Inflammation. As Mr. Paget has justly remarked, degeneration is a part of the regular series of nutritive operations; for “to degenerate and die, is as normal as to be developed and live; the expansion of growth, and the full strength of manhood, are not more natural than the decay and feebleness of a timely old age,—not more natural, because not more in accordance with constant laws, as observed in ordinary conditions.”

The study of development has always, however, had precedence in the choice of all the best workers in physiological science, and that of degeneration has been scarcely at all pursued. What little has been done in this department, has had reference solely to the human body; and “almost the only essays at a general illustration of the subject have issued in the ridiculous notion, that as the body grows old, so it retrogrades into a lower station in the scale of the animal creation.” The study of the changes of natural degeneration in old age is important, as affording a basis for the interpretation of many phenomena, which are to be regarded as morbid rather in their prematurity than in their essential nature; and these are grouped by Mr. Paget under the following heads:—

1st. *Wasting or Withering*, as in the ordinary emaciation of old age; some parts being entirely removed by absorption, whilst others are only decreased in size, and lose the succulency of earlier age.

2d. *Fatty degeneration*, as shown in the tendency to the accumulation of fat in many tissues, especially the bones; the *arcus senilis* of the cornea has been shown by Mr. Canton to possess this character; and the fatty degeneration of the arteries of the aged is well known to be a most ordinary occurrence.

3d. *Calcareous degenerations*, as shown in the gradually increasing proportion of earthy matter in the bones, in the ossification of parts that remained cartilaginous during the period of vigour, and in the tendency to earthy deposits in the arteries and other parts.

4th. *Pigmental degenerations*, as shown in the gradual accumulation of black pigmental matter in the lungs, the mucous membrane of the alimentary canal, and even in the coats of the arteries.

5th. *Thickening of primary membranes*, as seen in the tubules of the testes, the inner membrane of the blood-vessels, and the walls of cartilage-cells.

The following are enumerated by Mr. Paget as the characters in which true degenerations are distinguished from disease:

"First, and before all others, it should be a change naturally and usually occurring, in one or more parts of the body, at the approach of the natural termination of life, or, if not then beginning, yet then regularly increasing.

"2. It should be a change in which the new material is of lower chemical composition, *i. e.*, is less remote from inorganic matter than that of which it takes the place. Thus, *e. g.*, fat is lower than any nitrogenous organic compound, and gelatine lower than albumen, and earthy matter lower than all these.

"3. In structure, the form should be less developed than that of which it takes the place: it should be either more like inorganic matter, or less advanced beyond the form of the mere granule or the simplest cell. Thus the approach to crystalline form in the earthy matter of bones, and the crystals in certain old vegetable cells, are characteristic of degeneration; and so are the granules of pigment and of many granular degenerations, and the globules of oil that may replace muscular fibres or the contents of gland-cells.

"4. In function, the part should have less power in its degenerate than in its natural state.

"5. In its nutrition, it should be the seat of less frequent and less active change, and without capacity of growth, or of development." (p. 33.)

There are many things which show, that the assumption of these characters is to be ascribed, rather to a defect, than to a perversion of the vital force or of the conditions of nutrition:

"Thus (*a*) these are all apt to occur in a part of which the functions are abrogated; a motionless limb wastes or becomes fatty as surely as an old one does. (*b*) They often occur, too, in parts that fail to attain the development for which they seemed to be intended. Thus, *e. g.*, fatty degeneration usually ensues in the cells of unfruitful Graafian vesicles.\* (*c*) They bear also a certain general analogy to the changes that ensue in some of the materials that are habitually excreted from the body; in the construction or composition of which materials one seems to have an instance of the gradual supervention of the ordinary or imitable processes of chemistry. (*d*) Again, they display living parts tolerating the presence and incorporation of inorganic or dead matter; fat being commonly infiltrated about degenerate cells and membranes, and earthy matter with it, even in the crystalline form. (*e*) And, lastly, and perhaps most clearly, the origin of degenerations from defective, rather than from perverted, conditions of nutrition, appears in the fact that one, at least, (namely, fatty degeneration,) of them may be produced artificially." (pp. 33-4.)

\* Reinhardt in Traube's Beiträge, b. i.

The degeneration of lymph may commence at any period after its formation ; and that which most obviously determines it, is the continued suspension of the conditions of nutrition. We shall first speak of its *Fibrinous* element. In the *first* place, this may waste or wither, becoming firmer and drier, passing into the state which Rokitansky has called "horny;" the fibrin in these cases shows no marks of development into tissue, but retains its ordinary structure, being only drier and more compact. *Secondly*, fibrin is subject to a degeneration, which is comparable to the fatty degeneration of ordinary tissues ; this is an extremely frequent occurrence, presenting itself in the fibrinous effusions poured out in the lower forms of inflammation, or in those of cachectic individuals. The softening and disintegration of the clots within the heart, which Mr. Gulliver has described, correspond closely with the like processes in inflammatory lymph. The whole substance is seen to be dotted with granules, which are known to be oil-particles by their peculiar shining, black-edged appearance ; and the fibrin, no longer rendered transparent by acetic acid, loses its toughness and elasticity. It appears to be usually by such a degeneration, that fibrinous matter, which has firmly coagulated, softens and becomes capable of absorption :

"I suppose it may be considered as a general truth, that the elements of a tissue cannot be absorbed so long as they retain their healthy state. There is no power of any absorbent vessels that can disintegrate or decompose a healthy portion of the body : for absorption, there must be not only an absorbing power, but also a previous or concurrent change, as it were a consent, in the part to be absorbed ; so that it may be reduced (or, rather, may reduce itself,) into minutest particles, or may be dissolved. And this change is probably one of degeneration, not death, in the part ; for dead matter is rather discharged from the body than absorbed.

"Now there are some facts which indicate the probability that the fatty degeneration is that which commonly precedes the natural absorption of many normal parts ; or, rather, that, in the change which they undergo before absorption, fatty matter is one of the products, and that the principal evident difference between the atrophy of a part which is manifested by its wasting, and the atrophy which is manifested by fatty degeneration, is, that the fatty matter, which is absorbed in the former case, is retained in the latter. However this may be, it is certain that the disintegration and fatty degeneration of the fibrin-products of inflammation bring them into a state most favorable for absorption ; indeed, one may see in fibrin thus changed many things which, in regard to the fitness for absorption, make it parallel with chyle. Of such absorption of fibrin we may find many instances. In rheumatic iritis we may believe the lymph to be fibrinous ; but we see its complete absorption taking place ; and the recent observations of Dr. Kirkes on the rarity of adhesions of the pericardium, in comparison with the frequency of pericarditis, may be in the same manner explained. In rheumatic pericarditis we may be sure fibrin is effused ; and the observed friction-sound has, in at least one case, proved its coagulation ; yet in this case, when death occurred four months afterwards, scarce a trace of fibrin was found in the pericardium : it had been absorbed, and the degeneration I have been describing was probably the preparation for its absorption." (pp. 35-6.)

*Thirdly*, examples of the calcareous degeneration of fibrin are shown in the ordinary formation of phlebolithes from clots of blood, and in the calcareous deposits which are found imbedded in the fibrinous vegetations of the valves of the heart. This form of degeneration, however, is much more frequent in purulent fluids, and in later developments of lymph. *Fourthly*, pigmental degenerations are often seen in the fibrinous lymph effused in

peritonitis, which presents shades of gray and black, that are due to the presence of pigmentary granules.

A similar series of degenerations is seen in the *Corpuscular* elements of lymph. *First*, their withering is seen in certain elements of the dull ochre-yellow-coloured, and half-dry material contained in lymphatic glands that have undergone chronic and nearly stationary scrofulous enlargements. In this substance are found abundant collapsed and shrivelled cells, which might be supposed to be dried pus-cells, or corpuscles of chronic tuberculous matter, were it not that some of them present an approach to the character of fibre-cells, into which it is certain that neither pus- nor tubercle-cells are ever developed. The corpuscles found in the pus of chronic abscesses, are believed by Mr. Paget, from their likeness to the foregoing, to be rather withered lymph-corpuscles, than true pus-cells. *Second*, the lymph-cells are changed by fatty degeneration into granule-cells, or exudation-corpuscles; which, as Reinhardt has shown, may also be derived by a similar process from the primary cells of almost all other, both normal and abnormal, structures. This form of degeneration is particularly apt to occur in the products of inflammation in the nervous centres and in the lungs, but it is by no means confined to those organs; and it may take place alike in the early forms of lymph-cells, and after they have already elongated and attenuated themselves into fibre-cells, and also after they have degenerated into pus-cells. The following are, briefly, the stages of this transition, which corresponds exactly with that so commonly observable in the cells of the liver and kidney:

“The lymph-cells, which may have at first quite normal characters, such as I have assigned to ‘primordial cells,’ present a gradual increase of shining black-edged particles, like minute oil-drops, which accumulate in the cell-cavity, and increase in number, and sometimes in size also, till they nearly fill it. The fatty nature of these particles is proved by their solubility in ether: and their accumulation is attended with a gradual enlargement of the cell, which also usually assumes a more oval form. Moreover, while the fatty matter accumulates, the rest of the contents of the cell becomes very clear, so that all the interspaces between the particles are quite transparent; and, coincidently with all these changes, the nucleus, if any had been formed, gradually fades and disappears.” (pp. 36-7.)

This kind of degeneration, as in the case of the fibrinous element, appears to be preparatory to absorption, as probably happens in the “clearing-up” of the solidified lung after an attack of pneumonia. The calcareous degeneration of the lymph-cells is not so often seen, but sometimes presents itself coincidently with the preceding; of the pigmentary degeneration we have a very common example in the colouring of the cells of bronchial mucus already referred to.

The most frequent of all the degenerations of lymph, however, is into *pus*, this change ensuing in nearly all cases in which the lymph is placed in conditions unfavorable for its development. That such a change does take place, there can now be no doubt, since every gradation can be seen, from the most characteristic form of lymph-cell to that of pus-cell; and it is very questionable whether pus is ever, what it was so long undoubtingly regarded, an original or primary product of inflammation. “We cannot,” as Mr. Paget observes, “always discern a preliminary lymph-stage; but neither can we always distinguish lymph-cells from pus-cells, nor can we



see in how very brief a time the transformation may be accomplished." Other rudimental cells, besides those of the lymph, may be so altered as to take on the appearance of pus-cells; thus, in many of the supposed cases of pus in the blood, the bodies taken for pus-cells were certainly only altered white corpuscles. When it occurs as a product of inflammation, however, the pus-cell may be pretty safely regarded as an ill-formed or degenerate lymph-cell; and the variety of form which it will present will partly depend upon the previous quality and grade of development of the lymph-cell, and partly on the further degenerations which may have taken place, after the characters of the pus-cell have been acquired. The following is Mr. Paget's account of the typical conditions of the pus-cell, and of the principal degenerations which it may undergo:

"In specimens of what might be called 'good' pus, we may find three principal forms. There are—1st, some corpuscles presenting the peculiar and well-known granulated or wrinkled appearance of pus-cells, but from which water will raise up no cell-wall; 2, from others, like these at first sight, water will raise a cell-wall, and will show that the former kind consist of only such a substance as forms the contents of these; 3d, in others, even when no water is added, a cell-wall is visible, and within this are granulated contents, with a more or less distinct nucleus imbedded in them. In all these forms, moreover, the addition of acetic acid usually displays a single or a bipartite or tripartite nucleus. Now, it may be that these represent three different stages of the pus-cell, either developing, or, more probably, degenerating; but I think it is much more likely that these forms are the results of the purulent degeneration beginning in lymph-cells at different stages of their development. There is so remarkable a correspondence between these three varieties of pus-corpuscles, and the three chief forms which I described as observable in the development of the primordial cell of lymph, that one cannot but suspect that the three forms in the pus represent corresponding and similar degenerations from the three forms in the lymph.

"When once formed, the pus-cells, if they are retained within the body, have no course but to degenerate further; it is characteristic of their being already degenerate, that they can neither increase nor develop themselves. The various corpuscles found in pus, besides those I have already mentioned, must find their interpretation in these degenerations; for the pus-cells are prone to all the degenerations that I described as occurring in the lymph-cells.

"*a.* They may wither, as in the scabbing of pustular eruptions, or in long-retained and half-dried strumous abscesses.

"*b.* Or, they may be broken up, whether before or after passing into the fatty degeneration, which is one of their most common changes, and in which they are transformed into granule-cells. It is this breaking-up into minute particles which, probably, precedes the final absorption of pus.

"*c.* Or, lastly, both the cells and the fluid part of the pus may alike yield fatty and calcareous matter, and this may either remain diffused in fluid, or may dry into a firm mortar-like substance.

"It is to such degenerations as these, in various degrees and combinations, and variously modified by circumstances, that we must ascribe the diverse appearances of the contents of chronic abscesses and of the substances left after their healing." (p. 38.)

But the same kinds of degeneration may occur in the products of inflammation after they have advanced further towards complete organisation. Thus adhesions not unfrequently degenerate by wasting; seldom or never by passing into the fatty state; but very commonly by becoming calcified, the product having sometimes an approach to the character of

true bone, but much more frequently having the earthy matter deposited in an amorphous form, as if, as Rokitansky has remarked, it was a residue of the transformation of the more organised tissue whose soluble parts have been absorbed after decomposition ; and, lastly, by developing pigmentary matter, which is sometimes seen in adhesions of the pleura, but much more commonly in those of the iris.

Such, then, according to Mr. Paget, are the principal forms of degeneration exhibited by the more or less developed products of inflammation. We consider that he has done good service in thus attempting to classify them, and to point out the direction for further inquiry. The whole subject of Degenerations may be regarded, in fact, as of even more pathological importance than that of development ; and deserves the fullest investigation. For it is the essential character of by far the greater number of morbid processes, that the nutritive material is applied to some purpose different from its regular destination ; and that the product is, in relation to the normal one, of a degenerate kind. And even where an attempt is made (so to speak) to develop a normal structure, as in the organisation of lymph thrown out for the repair of injuries (whether produced by external causes, or by the morbid actions of tissue itself), that structure, until it is fully adopted into the system and made completely a part of it, is peculiarly prone to undergo degeneration. Now it has been from dwelling upon this very frequent destiny of fibrinous exudations, and from passing-by the cases in which they do remain as constituent parts of the organism, that Mr. Simon has been led to the notion that fibrin is itself a degenerating product. It may be that the last-named instances are the exception rather than the rule ; but this is because, in all ordinary cases in which these exudations are thrown out, causes of degeneration are operating. It is in those simplest cases in which the reparation proceeds with the least amount of those disturbing influences which tend to produce degeneration,—as, for example, in the reunion of the two ends of a tendon after subcutaneous division,—that the organisation of the fibrin goes on most after the fashion of ordinary nutrition, and with the most complete result ; and we seem, therefore, clearly entitled to affirm, that of all the elements of the blood, the fibrin is that which is the most prone to become organised, and is that which affords the ordinary pabulum for the development of the tissues.

The last topic considered by Mr. Paget, in relation to the products of inflammation, is that of the diseased actions to which they are subject, after they have become as fully organised as their nature admits. On this point he makes the following apposite remarks :

“ Among the signs of the attainment of complete membership in the economy, we may enumerate this, — that the organised product of inflammation is liable to the same diseases as the parts among which it is placed ; that it reacts like them under irritation ; is like them affected by morbid materials conveyed to it in the blood ; and like them may be the seat of the growth of new and morbid organisms. No more complete proof of correspondence with the rest of the body could be afforded than this fact presents ; for it shows that a morbid material in the blood, minute as is the test which it applies, finds in the product of inflammation the same qualities as in the older tissue to which it has peculiar affinity.

“ The subject, however, of the particular diseases to which these substances, themselves the products of disease, are liable, has been little studied. As I have

already said, lymph, while it is being highly organised, is often the seat of hæmorrhage; its delicate new-formed vessels bursting under some external violence, or some increased pressure, and shedding blood. Such are most of the instances of hæmorrhagic pericarditis, and other hæmorrhages into inflamed serous sacs.

Even more frequently, the lymph, when organised, becomes itself the seat of fresh inflammation. Thus, in the serous membranes, we may find adhesions in the substance or interstices of which recent lymph or pus is deposited; or, in other cases, adhesions, or the thickenings and opacities of parts, become highly vascular and swollen. It is, indeed, very probable that, in many instances of the recurring inflammations that we watch in joints, or bones, or other parts, the seat of the disease is, after the first attack, as much in the organised product of the former disease as in the original tissue.

"I suppose, also, that to such inflammations of organised inflammatory products we may ascribe many of the occasional aggravations of chronic inflammations in organs—the renewed pains and swellings of ankylosed joints, of syphilitic nodes, and the like,—which are so apt to occur on exposure to cold, or in any other otherwise trivial disturbance of the economy. In such cases we may believe that the former seat of disease is again inflamed, and that with it are involved the organised products of its former inflammations. And in such cases there are, perhaps, none of the effects of inflammation which may not ensue in the newly organised parts: evidently, they may be softened, or thickened and indurated, and made more firmly adherent: or they may be involved in ulceration, or may slough with the older tissues among which they are placed.

"Lastly, the products of inflammation may be the seats of the morbid deposits of specific diseases. In their rudimental state they may incorporate the specific virus of inoculable diseases, such as primary syphilis, variola, and the rest; and when fully organised they may be the seat of cancer and tubercle. But on these subjects I have not time to dwell, although their pathology, especially as illustrated by the tuberculous diseases of serous membranes, is full of interest." (pp. 39-40.)

The process of Inflammation, however, does not manifest itself only in the formation of morbid exudations, and in the subsequent changes of development and degeneration which these may undergo; it yet more essentially consists, in our apprehension, in the impaired nutrition of the part itself. In fact, we question whether the fibrinous exudation ought not to be really regarded as no less truly *reparative* in the case of an inflammation, than it is in the case of a wound. For, take the case of an ulcer, or of the cavity of an abscess, in which there has already been loss of substance; there is here no closure (except by the comparatively slow, though surer, process of ordinary growth), unless an organisable exudation is formed, just as in the case of a wound; and many an inflammation, we believe, would be attended with such loss of substance, if the normal tissues, contemporaneously with their degradation, were not penetrated with a fibrinous effusion which there is yet vitality enough to organise. It seems to us to be in the *want* of power to form this plastic exudation, or in the tendency of the morbid actions going on around to involve it also in the general degradation, that the special characters of what are commonly known as "unhealthy inflammations" consist. The value of the fibrinous effusion in forming the sac of an abscess, and thus limiting the purulent infiltration, is generally admitted; as is also the importance of that "reaction" which forms the line of demarcation in a spreading gangrene. These, it is true, are comparatively extreme cases; and in pneumonia or rheumatism, it may be said, there is comparatively little tendency to suppuration or to gangrene, so that the fibrinous effusion is an unmixed evil. We are not disposed to grant this last assumption; but even if it be admitted, we

must still see, that as the ultimate tendency of inflammation is to produce the disintegration of the part, the ultimate tendency of fibrinous effusions is to keep its elements together.

Of the effects of inflammation upon the proper tissue of a part, one of the most common is *softening*. This is familiar to every one, as occurring in the ordinary textures; but Mr. Paget directs particular attention to the phenomena it presents in bone, in which the softening appears to precede the more evident signs of inflammation, and often allows the tissue to undergo a remarkable expansion.

"The characters of a bone thus expanded are easily discerned. Its substance may be irregularly cancellous or porous; but the most striking change is a more or less extensive and wide separation of the concentric laminae of the walls of the bone, so that the longitudinal section of the enlarged wall appears composed of two or more layers of compact tissue, with a widely cancellous tissue between them: and these layers may sometimes be traced into continuity with those forming the healthy portion of the wall. Usually, the separated layers are carried outwards, and the bone appears outwardly enlarged; but sometimes the inner layers of the wall are pressed inwards, and encroach upon the medullary tissue. In the first periods of the disease, the cancellous tissue between the separated layers of the wall has wide spaces, which are usually filled with a bloody-coloured medulla: but this tissue, like the often coincident external formations of new bone, appears to have a tendency to become solid and hard; and its fibrils and laminae may thicken till they coalesce into a compact ivory-like substance, harder than the healthy bone." (p. 42.)

So, again, the great yielding of ligaments, which is to be seen in almost all cases of severely-inflamed joints, can scarcely be looked upon in any other light than as a result of inflammation, that is, of a defective nutrition, producing diminished cohesion of their own particles, combined with infiltration of inflammatory products:

"The softening of the tissues of an inflamed part may be regarded as one of the instances of degeneration in the inflammatory process; and its diversity from ordinary degenerations may be ascribed to the simultaneous infiltration of the inflammatory product, and perhaps to some other circumstance we cannot at present trace or guess. But a more general and unmixed form of degeneration may, I think, be occasionally observed in the tissues of inflamed parts—namely, fatty degeneration; and this in such a manner, as to make it probable that the degeneration takes place even during the inflammation. Thus fatty degeneration of the hepatic cells appears an usual coincident of the form of inflammation which produces the so-called "brawny" liver. I think, too, that I have seen fatty degeneration of the muscular fibres in inflammation of the heart; especially in a recent case, in which the heart was punctured with a needle, and the patient died four days afterwards. The portion of the heart near the needle was more degenerate than the rest of its substance. So, also, in some instances of acute ulceration of cartilage, I have found that a fatty degeneration of the contents of the cells, together with similar degeneration or disappearance of the nuclei, constantly precedes the removal of the whole substance of the cartilage. I am inclined, therefore, to believe that this method of degeneration may occur not unfrequently in inflammation of certain parts, and may be, like the softening last described, a principal constituent of the changes preceding their complete absorption." (pp. 42-3.)

These observations of Mr. Paget are in complete harmony with those of Dr. Redfern, of which we gave an account on a former occasion. (Vol. VI, p. 168.)

We quite agree with Mr. Paget in thinking that *absorption* must always

be preceded by degeneration ; and that loss of substance is not a primary but a secondary result of inflammation. Such degenerative changes can often be clearly traced ; and there is no evidence that any healthy tissue is ever thus absorbed, but much that leads to the belief, that so long as the vital force is in active operation in a part, no such absorption is possible. Physiologists, and we trust surgeons also, have pretty generally abandoned the notion that a loss of substance can ever be owing to the undue activity of the "absorbent vessels." For, in the first place, vessels have no power of absorbing solid matters ; these must have undergone solution before their particles can be taken back into the circulating current ; and besides, it is extremely doubtful whether the absorbents normally take up even the products of such solution ; as there is much more reason to believe that all the disintegrated matters, which are destined to be cast out of the system, are taken back into the veins, than that they are received into the lymphatics. Various examples of degeneration by absorption are presented by bones ; but to Mr. Paget's account of these we must not now refer. The remarks which he makes upon absorption of the softer tissues are in full accordance with the views we have just expressed, as to the reparative and preservative character of the fibrinous exudations :

"Again, other examples of the absorption of inflamed parts, or of parts that have been inflamed, are presented in the wasting of glands after inflammation ; as in cirrhosis of the liver, in some forms of granular degenerations of the kidney, in the indurated and contracted lung after pneumonia.

"No doubt, in these cases, the reduction of the organ depends, in a measure, on the contraction of the diffused inflammatory product, as it organises ; but in many cases the quantity of new tissue is extremely small (it is so in the shrivelled granular kidney) ; and, in all the cases, we may well doubt whether the contraction of organising lymph would produce such extensive and uniform absorption of the proper substance of an organ, if there were not a previous condition favouring the absorption. The most probable explanation of these cases seems to be, that as, in the early periods of the inflammation, the softening and the degeneration of the inflamed tissues coincide with the production of the lymph ; so, *as the inflammation subsides, and subsequently, the absorption of the degenerated tissues may often coincide with the full organization and contraction of the lymph.* And it is altogether most probable that these events are independent though concurrent ; that each occurs as of itself, not as the cause or consequence of the others." (p. 44.)

We have another marked example of this absorbent process, in the spontaneous opening of a subcutaneous abscess ; which is described by Mr. Paget at length. He attributes the absorption to the softening of the tissue by inflammation ; and considers that the progress of the matter towards the surface is attributable to the greater proneness of the integuments to inflammation, and their greater activity when engaged in it, than that shown by the surrounding tissues. To what this difference is due, however, he does not attempt to show ; is it not connected, we would ask, with the difference in the ordinary nutrition of these parts respectively, and the normal rate of their interstitial changes ? The desquamation of the cuticle, which is noticed over the spot where the abscess is about to *point*, is adduced by Mr. Paget as another indication of the failure of nutrition, and consequent degradation, of the subjacent tissue, which is preparatory to its death and giving-way, so as to set the contained matter free.—Still Mr. Paget considers it an undetermined



question, whether a portion of the tissues, forming the boundary-walls of an abscess, may not be removed by the mingling of its disintegrated particles with the purulent fluid. Such has been generally supposed to be the source of the abundant molecules and shreds which are usually contained in the pus discharged from an abscess; but, as Mr. Paget justly remarks, these may be as justly considered as the *débris*, not of the tissue, but of the lymph-cells or pus-cells, and of the fibrin, which may have coagulated with them.

The same uncertainty exists with regard to the removal of the tissues in ulceration; that is, whether the disintegrated particles are taken up by absorption, or whether they are ejected in the fluid exudations. Both methods may, perhaps, be in progress at once; but, on the whole, Mr. Paget is of opinion that the enlargement of an ulcer chiefly takes place by ejection; and this for the following reasons:—

“1. Parts to be removed from a surface are generally cast-off rather than absorbed, as cuticles of all kinds are, and the materials of secretions; so that, by analogy, we might assume that the particles of the surface of a spreading ulcer would also be cast-off.

“2. The materials of the ulcerating tissue may be sometimes found in the discharge from the ulcer. In most cases, indeed, this is impossible; but perhaps it is so only because, when the tissues are degenerate, and broken-up, or decomposed and dissolved, we have no tests by which to recognise them. In the case of bone, however, some of the constituents of which are not so easily disguised, the ejected materials may be found.

“3. It strengthens this belief to observe, that, in many cases, small fragments of bone and other tissues are detached and cast-out with the fluid secreted from the ulcerating part. These, indeed, when they are not fragments of tissue detached by ulceration extending around them, are good examples of the transition that may be traced from ulceration to sloughing or gangrene of parts, between which, if ulceration be always accomplished by ejection, the only essential difference will be one of degree: the ulceration being a death and casting-off of invisible particles of a tissue, while gangrene implies the death and casting-off of visible portions.

“4. And it may be proved of many that we call ulcers, that they begin as sloughs which are cast-off, and leave the ulcerated surface beneath. We may often see this, on a large scale, in the instances of what are called sloughing ulcers; but Dr. Baly has proved it for a much wider range of cases, in his observations on dysentery, in which he has traced, how even the smallest and the most superficial ulcers of the intestine are preceded by the death and detachment of portions of the mucous membrane, with its covering of basement-membrane and epithelium.” (pp. 46-7.)

There are, no doubt, exceptions to this general rule, in which ulceration takes place under circumstances that seem to forbid our regarding the removal as accomplished in any other way than by absorption; this is especially the case in what may be termed the interstitial ulceration of bones, in which there is no external discharge of fluid. But, as just now pointed out, it is in the case of superficial ulceration of bones, that we seem to have the clearest evidence that the elements of the tissue are actually dissolved in the fluids of the ulcer. On the general question thus raised, Mr. Paget remarks:—

“To speak of the solution of tissues in the discharges of ulcers may seem like the revival of an old error long since disproved. But though the expression may be revived, it is with a new meaning. The proof has, truly, been long completed, that healthy tissues, even though they be dead, cannot be dissolved in pus, or any

such discharge; but the tissues that bound or form the walls of a spreading ulcer are not healthy; they are inflamed, or otherwise diseased and degenerate; and they may now be soluble in fluids that could not dissolve them while they were sound. Insolubility is as great an obstacle to absorption as to ejection in discharges; no tissue can be absorbed without being first so far changed as to be soluble in fluids with which it was before in contact and unharmed. Therefore, whether we hold the ordinary spreading of an ulcer to be by absorption of its boundaries, or ascribe it to their ejection, we must, in either case, admit that they are first made soluble. And if this be admitted, then it is most consistent with analogy, and most probable, that the extension of an ulcer, independently of sloughing, is accomplished by the gradual degeneration of the tissues that form its walls, and by their being either disintegrated and cast-off in minute molecular matter, or else dissolved and ejected in solution in the discharges from the ulcer.

"The solution here spoken of is such as may be effected by the fluid discharged from any spreading ulcers; and we may doubt whether all discharges from ulcers possess a *corroding* property, such as Rokitansky seems to ascribe to them, and such as he considers to be the chief cause of the extension of all ulcers. We may doubt, I say, whether all ulceration can be described as a corrosion or erosion of the tissues by ichor; but, on the other side, we cannot well doubt that the properties of the discharge from an ulcer, or a sloughing sore, may have a great influence in accelerating the degeneration and decomposition, and thereby the solution, of the tissues that form its walls or boundaries. Many ichorous discharges from ulcers inflame and excoriate the parts over which they flow; and one constituent of inflammation is the defective nutrition of the proper elements of the affected tissue. Many such discharges, also, are in an active state of decomposition; and their contact with the tissues cannot but have some tendency to excite decomposition in them; a tendency which the tissues will be the less able to resist, in the same proportion as they are already feebly maintaining themselves, or as they have been moved by inflammation from their normal conditions, and their normal tenacity of composition." (p. 47.)

Of course, the products of inflammation are liable to be removed by ejection, in the same manner as the tissues themselves.

Mr. Paget's remarks on Gangrene are very short, and are chiefly confined to a denial of the ordinary notion that mortification is to be taken unconditionally as the expression of the gravest intensity of the inflammatory action in the part; since many other influences concur to modify the result. "It may be nearer the general truth," he thinks, "to say that the probability of gangrene ensuing in inflammation, is proportionate to the sum of the intensity of the disease, *plus* the debility or defective vitality of the affected part, whether that debility have a general or a local origin." But we take this to be equally true of *all* the results of inflammation. And looking to the diminished vitality of the part as itself one of the essential phenomena of inflammation, (in which view Mr. Paget concurs with us, as was seen in our former article,) we cannot see the necessity of separating the one condition from the other;—all the antecedent and concurrent conditions which favour the mortification, being those which increase the severity of the inflammation; and *vice versa*. We are glad to find, however, that as a separate series of specimens in the Museum is devoted to the illustration of the death of parts, we shall have another opportunity of learning Mr. Paget's views upon this subject more *in extenso*.

We now take our leave of this subject for the present. We believe that in some of its aspects it has been well-nigh exhausted; but abundance

of novelty, we are confident, remains to be opened up ; and to any young observers, who, microscope in hand, are about to enter upon this domain of research, we cannot give better advice than to study Mr. Wharton Jones's Prize Essay as a lesson in the art of observation, and Mr. Paget's Lectures as a guide in that more difficult acquirement, the knowledge of *what to observe, and how to make use of the facts collected*.

### ART. XIII.

1. *The Pathology of the Kidney in Scarlatina. Illustrated by Cases.* By JAMES MILLER, M.D., Physician to the Western General Dispensary.—London, 1850. 8vo, pp. 178.
2. *On Scarlatina.* By J. W. TRIPE, M.D. (From the 'Medical Times,' 1848-9.)

As in the other sciences, so in that of disease, is it essential that the same thing should be signified by the same name ; and that when a series of phenomena are included under one term, the same series should be intended by every writer who uses that term. The importance of distinguishing by the same name, the same essential deviation from function or structure, when describing or estimating the effect of remedies, is too evident to require arguments for proof. Is simple diarrhœa to be regarded as the first symptom of cholera ? Are patients relieved from the former, to be reckoned among those cured of the latter ? Is there any difference in the essential deviation from health in the two ? Could either, supposing them contagious, generate the other ? Is it correct to include both under *one name* ? Does the same name, as used in different places by the same writer, or by different authors, signify the same thing ? Whoever attempts to weigh the value of cholera specifics will find these questions meet him on the very threshold of his researches.

The necessity for the essential identity of the things signified by the same term, is equally obvious, if we would investigate the laws which determine the origin of epidemic diseases, and govern their transit from place to place. Let the influence of the soil, or of the thermometrical, barometrical, or electrical conditions of the atmosphere be considered as determining or exciting causes of any one of these diseases ; and it is clear that what is true with reference to one of them, may not be true with respect to another ; or that if we include in our calculation only one phase of such affection, we shall be very unlikely to arrive at any absolute general conclusion. Therefore must we, when speaking of either of these diseases, say scarlet fever, both extend the term used to every variety of the disease itself, and include in it no other affection. By Morton, measles and scarlet fever were confounded as one disease ; Fothergill separated from scarlet fever that which unquestionably was a mere variety of it, and conjoined probably with the group he cut off from scarlet fever, another affection having no relation to that group. These errors vitiate their general observations or conclusions with reference to the causes and treatment of the diseases they respectively described. Further, it is yet a question, whether, under the name of measles, there are or are not commonly grouped two diseases,—the r  theln of the Germans, the rubeola of

others, and the measles, or morbilli, properly so called. This point is yet open for investigation.\*

Hildenbrand† states, that some consider Rubeola, with Hufeland, Schaeffer, Formey, and Heim, to be a variety of scarlatina; and that Kapp, Wichmann, and Reil view it rather as allied to measles. Now it is obvious, that, until this point is settled, all general observations on the spread of the diseases in question have to be received with caution. We must ever ask, what does the author, using the word, mean by measles?

With few exceptions, the whole class of acute diseases may reign as epidemics; and every epidemic disease may occur sporadically. To the Father of medicine we owe the introduction of the term epidemic; the English Hippocrates moulded to their present shape our ideas on epidemic diseases and their causes. To comprehend the actual state of knowledge with reference to the latter, it will be well briefly to survey the doctrines of Sydenham on acute diseases, and their dependence on atmospheric changes.

As some epidemics affect the inhabitants of a city or country for a series of years, independently of season, or only in a degree restrained by it, Sydenham termed the diseases possessing such character, "stationary fevers." As others arise at particular seasons, disappearing with the termination of those seasons, he termed them "intercurrent fevers;" the former being, as it were, the groundwork, the latter merely passing accidents. "Intercurrent fevers preserved their identity," he says, "in all years, were attended with the same symptoms, and required the same treatment, whatever the character of the stationary fever, or of the epidemic constitution of the period." In the class of intercurrent fevers he placed rheumatism, quinsey, and pleurisy; in the last of these bloodletting was *the* remedy, and that whether the disease occurred in 1662 or 1671, *i. e.*, in that period in which the atmospheric constitution favoured the spread of smallpox, or in that in which intermittents reigned paramount.

Stationary fevers were divided by Sydenham into:—1st, typical or proper; and, 2d, those which had their names taken from some marked changes impressed upon the blood, or else from some palpable symptom. Typical or proper stationary fevers were those which preserved their identity in all constitutions of the atmosphere, but occasionally underwent modification in form; in one state of epidemic constitution being uncomplicated, trivial in severity, and rarely fatal; in another presenting some unusual symptom, and perhaps among the most mortal of diseases; smallpox and measles were considered by Sydenham as types of this class. Thus, the smallpox of 1667-68, was rarely fatal, it presented the normal characters of the uncomplicated disease; in 1669 began an atmospheric constitution favorable to the origin and spread of dysentery, and then smallpox of an anomalous form arose; in essential characters, however, the disease was still smallpox. This smallpox of the dysenteric constitution, as Sydenham termed it, presented no symptom of dysentery; so the measles which occurred during the dysenteric constitution of the atmosphere, presented none of the symptoms of dysentery, but were "the most perfect of their genus;" while those which were epidemic in 1674 "adhered less to their proper type." It is then important to bear in

\* See British and Foreign Medico-Chirurgical Review, vol. vi, p. 163.

† Institutiones Practico-Medicæ, vol. iv, p. 417.

mind, that in all essential particulars each of these typical or proper stationary fevers maintained its characters unchanged; only the eruption and the mortality of the two epidemics of smallpox varied a little; and measles, in the one constitution generally uncomplicated, was in the other frequently conjoined with local inflammation.

The second class of Sydenham's stationary fevers contained those diseases, which, in the present day, are ordinarily grouped together, and termed continued fever; these Sydenham considered to owe all their symptoms to, or to be engendered by, "certain hidden and inexplicable changes within the bowels of the earth," and to vary in every character, and in the treatment required for their cure, with each change in the constitution of the atmosphere.

He proposed to term each of these fevers by a name framed from that typical or proper stationary fever, the spread of which had been favoured by the atmospheric constitution of the period in which the fever occurred. For he held, that every epidemic constitution, although capable only of favouring the propagation and modifying the secondary symptoms of the typical stationary fevers, might *engender* a disturbance of the blood; which disturbance or fermentation, although not the prevailing typical disease itself, was its "twin sister," and therefore, at least in part, deserving of the same name. He designated, then, as variolous, morbillous, and dysenteric fevers, those continued fevers which arose during the years when either smallpox, measles, or dysentery ruled the autumn. The variolous fever wanted, however, the essential anatomical character of smallpox, *i. e.*, the pustule; the dysenteric fever was accompanied by neither purging nor tenesmus; the morbillous fever by no exanthem nor affection of the mucous membrane of the eyes or nose. At the present time, there cannot be a doubt that these twin sisters of smallpox, of measles, and of dysentery were diseases as distinct from the latter, as those were from each other.

As knowledge has advanced, physicians have separated group after group from this class of variable diseases; and have erected each group so separated into a proper and typical fever. It is clear that, if the groups thus separated be really distinct from each other, and from those with which they were previously combined,—distinct in nature, and not mere artificial divisions,—when we shall have removed all the diseases which Sydenham's second class of fevers comprised into the first, when there shall no longer be any but typical or proper epidemic fevers, we shall be in a position to determine the influence of external agents over their origin, spread, prevention, and cure. But so long as there is a class of epidemics, composed of diseases arranged together, and called by one name, only because of certain supposed properties of the blood impressed on it by a fancied occult atmospheric influence, and not combined because of any definitely diseased state of that fluid, any constant anatomical lesion, any common and known cause, or any invariable and distinctive symptoms,—so long must our knowledge of that class of epidemic diseases be imperfect, our treatment of them unstable, and our investigations into the laws which regulate their origin and spread futile.

To those appreciable atmospheric changes, which he thought induced rheumatism and pleurisy, Sydenham refused the term epidemic constitution.

The thermometrical and hygrometrical condition of the air gave rise to



the intercurrent fevers, which varied in frequency at different periods of the year, as the air was warm or cold, moist or dry. The epidemic constitution impressed on the typical or proper stationary fever a sthenic or an adynamic character, and favoured or retarded their spread; it generated, as well as determined, the symptoms of the variable fevers.

It is constantly to be borne in mind, that Sydenham in no sense held that the epidemic constitution which favoured the propagation of one typical or proper fever, impressed any of the peculiar or distinctive features of that epidemic fever on any other typical or proper fever, which arose sporadically, or was secondarily epidemic, during the prevalence of the first; thus, as we have seen, the measles of the dysenteric constitution were the very model of uncomplicated measles, and the smallpox of the same constitution presented none of the distinctive symptoms of dysentery. The epidemic constitution of Sydenham, then, never amalgamated, nor even assimilated, two or more typical or proper fevers.

Sydenham classed scarlet fever among the intercurrent fevers; he thought it a "moderate effervescence of the blood, arising from the heat of the preceding summer." Subsequent experience has raised it to the rank of a proper or typical stationary fever.

By degrees, our ideas as to the meaning of the term, *Scarlatina*, have acquired distinctness; until, at the present moment, it appears that we are in a position not only to group together, under the name of scarlet fever, a definite series of phenomena, but to prove that although in certain cases one or more of those phenomena may be absent,—nay, that only one or two may be present, yet that *the* disease, the primary, the essential deviation from health, exists. Our power of thus determining the value of these varied phenomena, as indicative of the existence of a certain unknown, essential deviation from health, which (could we detect it) would properly be considered the disease, is derived from our knowledge of the fact, that the same cause will produce, in different individuals, (1) constitutional disturbance, attended by a scarlet eruption on the skin, redness and soreness of the throat, anasarca, and fibrino-albuminous urine; *i. e.* shall exert a specific action on the cuticular, the pharyngeal, the renal, and the areolar tissues; (2) constitutional disturbance, accompanied only by a specific affection of the pharyngeal, (3) of the cuticular, (4) of the renal and cellular, (5) of the renal tissues:—and again, that an individual manifesting either of these phenomena, as a consequence of exposure to that cause, which in another has led, or (as experience has proved) might lead, to all, may emit from himself a something capable of exciting a constitutional disturbance in another, accompanied by one, two, or all of the above-named phenomena, according to certain peculiarities in the recipient, or to other circumstances at the present undetermined.

It is true that at particular places, during short periods of time, any one of these varieties may prevail over the others, some yet unknown influence determining the character; but even under these circumstances, cases of the fully-developed disease are intermingled with those of the imperfect variety. Had we only Sydenham's account of the scarlet fever of the seventeenth century, we should scarcely believe that the scarlet fever which now, and for years past, has existed in London, was the same as that which he described. But he practised in Westminster; and, at the very time that he published an assurance that scarlet fever was a disease in name

only, Morton was visiting patients, the histories of whose cases he has left, affected with that form of scarlet fever which is now regarded as a type of the disease.

Now one of two things is evident; either that Sydenham confounded severe scarlet fever with some other affection, reserving that name to mild cases only, in which the cuticular surface was the part chiefly affected; or that scarlet fever offered very different characters in different parts of the town. The former seems, on the whole, the more probable mode of accounting for his imperfect description of the disease. For that Morton's cases were really scarlet fever, no question can be raised, although he himself maintains that, specifically, it was identical with measles.

"Hunc morbum (ut ut universali medicorum consensu titulo peculiari donetur) prorsus eundem esse cum morbillis censeo, et solo efflorescentiæ modo ab illis distare."

And it is remarkable, that although in the histories he himself details, whenever more than one of a family are said to have suffered from either of the two diseases, the individuals were in every case all affected with the same disease;—e.g. in his 7th history, a child, after sleeping in the same bed with his little sister suffering from measles, "was seized with fever, accompanied by drowsiness, stupor, harsh cough, diarrhoea, redness of the eyes, and the other symptoms of the morbillous fever from which his sister was suffering;" his next history refers to a case of measles, also coming on after the child had slept with his sisters, a little while before affected with the same disease; while the 11th history tells of five members of one family who suffered from scarlet fever:—yet he says, "Quorsum enim, ubi causa morbi est eadem, eadem ferè symptomata, atque methodus medendi prorsus eadem, nova nomina affectibus imponantur, et morbi tanquam specie differentes sub alio atque alio titulo tractentur?"—and then:—"Ab eâdem causâ evidenti, *scil.* ambientis aëris injuriis, sive frigore in poros cutis admissis, sive miasmate contagioso eosdem poros invadente, uterque affectus originem suam trahit." Thus offering an example of that want of accordance between opinions, and the facts on which those opinions are founded, so often seen in the writings of the most able, who generalise from vague impressions, without reference to the individual observations on which their general conclusions are nominally founded.

The following brief sketch of some of the most remarkable epidemics of *Scarlatina* that have visited Europe during the last 200 years, will show that it may justly be classed among the typical or proper fevers, by demonstrating that it has preserved its characteristic symptoms unchanged, since it was first described by Ingrassias, in 1556, as distinct from measles. That it is really one of the stationary fevers of Sydenham, the long continuance of the last epidemic alone would prove. Sennertus, physician to the Elector of Saxony in 1650, gave an account of an epidemic disease, attended by a redness resembling that of erysipelas, sore throat, and desquamation of the cuticle, followed by anasarca; and in a case related to him by a friend, there was found after death, anasarca, ascites, and hydrothorax. Sennertus considered this disease a species of measles. An epidemic that occurred in Poland in 1664, was described by Schultzius as accompanied by sore throat, red rash, and desquamation of the cuticle, and as followed by anasarca and ascites. It was in 1667—1673, that

Sydenham observed scarlet fever; he noticed only the rash, and the desquamation of the cuticle. Morton details the histories of cases of scarlatina which occurred in 1686—89, attended by sore throat, scarlet eruption, acrid discharge from the nares, and swelling of the parotid.

Rosen de Rosenstein, detailed the symptoms observed in the epidemics that visited Sweden from 1741 to 1764; he refers to the presence of sore throat, a scarlet rash, and anasarca occurring on the twentieth or twenty-first day.

In 1751 an epidemic prevailed in Champagne, characterised, according to Navier, by sore throat, scarlet rash, desquamation of the cuticle, brown or bloody urine, and anasarca. The epidemic described by Fothergill as having visited London in 1739, and prevailed there till 1777, when the last edition of his account of the putrid sore throat was published, was attended by severe sore throat, and scanty but generally unequivocal scarlet rash; he compares the hue of skin, as Sennertus had done in 1650, to that of erysipelas; Fothergill makes no reference to anasarca; but Withering, in describing the epidemic of scarlet fever and sore throat, as it appeared in Birmingham, in 1778, after stating his conviction that the sore throat of Fothergill which he had witnessed, and the sore throat observed at Birmingham were identical, and that the latter was merely a variety of scarlet fever, states that anasarca sometimes occurred after the ulcerated sore throat. Speaking of this same epidemic, Dr. J. Johnstone, of Worcester, said in 1790,\* "this disease agrees with the ulcerated sore throat described very accurately by the excellent Drs. Fothergill, Huxham, and others;" and again, "the scarlet eruption was a much more frequent symptom and attendant of this disease, than it used to be when I first became acquainted with it, thirty years ago;" and afterwards refers to anasarca and ascites as occasionally following the disease.

In 1751—53, Huxham tells us, an epidemic of the malignant ulcerous sore throat visited Plymouth and its vicinity. He states that it was the same distemper as that described by the "very ingenious Dr. Fothergill." In addition to the sore throat, Huxham says that in general there was a very considerable scarlet efflorescence on the surface of the body, especially in children, that desquamation of the cuticle followed; in some few cases the whole body became "œdematous, and the impression of the finger would remain fixed in a part, the skin not rising as usual." Dropsies, during the same period were, he says, in another part of the dissertation, frequent during the same constitution of air.

J. Gabriel Zimmermann witnessed an epidemic at Heidelberg in 1775, attended by sore throat, red rash, desquamation of the cuticle, and anasarca.

In the first volume of the '*Memoires de l'Institut Ligurien*,' Ozanam states, is an account of an epidemic that reigned in Genoa in 1784-5, in which were observed sore throat, rash, and anasarca. Eischel describes an epidemic of scarlet fever that occurred in Copenhagen in 1787-8, in which cases occurred of angina without rash, of rash without angina, and of the two conjoined; desquamation of the cuticle and anasarca followed. Dr. John Sims states,† that in 1786 scarlatina was epidemic in London;

\* *Memoirs of the Medical Society of London*, vol. iii.

† *Ibid.*, vol. i.

it was accompanied by sore throat, scarlet rash, and, in a few cases, by anasarca.

Much of the difference of opinion entertained as to the identity of the sore throat of Fothergill and Huxham, and scarlatina maligna, has arisen from those authors having themselves supposed and stated, that the diphtheritis or morbus strangulatorius, which had from time to time been epidemic in the south of Europe, was the same disease as that which they saw and described in England in the eighteenth century. It is to diphtheritis, apparently, that the descriptions of the two Spanish physicians, Ludovicus Mercatus (1612), and M. Heredia (1626) apply; as also that of J. A. Sgambatus of Naples, published in 1610, and of Cletus Cælius of Signia in 1636. Chomel, in 1749, gave a very excellent description of the same disease, as it occurred in Paris the preceding year. His cases are remarkably well drawn up. He confounded, however, the ash-coloured diphtheritic exudation with sloughs, and the fetor of the discharges with the odour of gangrene; he mentions that if a slough was detached, it was quickly replaced by another in the same place and of the same extent. Appended to Chomel's dissertation is a letter from Astruc, giving an account of an epidemic of the same disease, which he had witnessed some years before. In December, 1753, Dr. Fothergill presented to the Medical Society an extract from a letter he had received from Mr. C. Colden, of Coldenham, in New York, containing an account of an epidemic "throat distemper," which appeared at Kingston, New England, in 1735, and then spread over all the British Colonies in America. The disease described by Mr. Colden was evidently diphtheritis. It offered the following symptoms:—little heat of skin, white specks of half an inch in length on the tonsils, the whole pharynx, in some cases, covered by white fur; cream-coloured sloughs detached from the tonsils quickly renewed; deglutition comparatively easy, oppression and sense of straightness in the upper part of the chest; difficulty of breathing; deep, hoarse, hollow cough, ending "in a livid strangled-like countenance, and death;" the patient being often able to walk about with little impaired strength, till an hour or two before his death.

Ozanam is clearly in error in referring the English epidemics described by Fothergill and Huxham to this affection; and Bretonneau's account of diphtheritis renders it impossible that physicians should ever again confound that disease with scarlatina faucium.

Numerous epidemics are referred to by Ozanam, as having raged in different localities between 1735 and 1826; but Colden's account of the American epidemic escaped his observation.

The epidemic of scarlet fever which visited London in 1847, '48, and '49, commenced its ravages during the last quarter of 1847, when the deaths ascribed to that disease were 747; the numbers having been in the corresponding quarters of 1845-6, respectively, 260 and 322, and in the third quarter of 1847 only 136. The epidemic increased in severity and extent, until, in the quarter ending December 31, 1848, the deaths registered from scarlatina reached 1765; from that time each quarter's return showed a marked diminution—thus, in those ending March 31, June 30, September 30, and December 31, 1849, the numbers were, respectively, 776, 497, 386, and 486; till, in the quarter terminating March 31, of the year 1850, the number registered as having died from scarlatina was only 199.

Dr. Miller's book contains an account of this epidemic as it affected the patients of a dispensary, situated at the north-west part of London ; but the general symptoms of the disease are treated of only with reference to the dropsy, the work having for its especial object the consideration of that one symptom, its pathology and treatment. "In the following pages," says Dr. Miller, in his preface, "I have endeavoured briefly to delineate the prevailing, and, in a great measure, *the* fatal feature of the recent epidemic of scarlatina, as well as to elucidate its nature and relation to the eruptive malady, by cases occurring at the time." The expression, "fatal feature of the epidemic," is afterwards explained to signify "scarlatinal dropsy."

Dr. Tripe's papers have a three-fold object ; first, to describe the epidemic of 1847-8, its symptoms, course, and treatment ; secondly, to consider the pathology of scarlet fever in general ; and, thirdly, to determine the influence of certain individual peculiarities and atmospheric conditions as causes of that disease.

The following is Dr. Tripe's definition of scarlet fever :

"A contagious eruptive fever, characterised by a uniform inflammatory action of the vessels of the skin, of the mucous membranes of the alimentary canal, and of the urinary passages, which terminates in desquamation of their epithelium. It ordinarily attacks any one of these membranes but once."

This definition he regards as differing from that of previous writers :—

1. Because, instead of saying that the disease attacks any one of these membranes but once during life, it is commonly said that it never recurs.

2. Because the inflammation of the alimentary mucous membrane is, by this definition, considered to be an essential part of the disease, and not a complication.

3. Because by it, also, the action of the poison is regarded as extending to the urinary passages ; and, for the future protection of the patient, this extension of the action of the poison is considered to be as essential as is the implication of the skin, and of the alimentary mucous membrane.

Although Dr. Tripe admits that various writers have held some of the opinions respecting the disease which the above definition involves, yet he thinks, that, as a whole, he has for the first time explicitly stated them. Other writers have certainly held that the inflammatory action of the skin, and that of the alimentary and urinary mucous membranes, are equally excited by the specific action of one poison ; but Dr. Tripe's definition involves, it will be seen, the opinion which he thus expresses : "If a person has had the eruption in the skin, but not in the mucous membranes enumerated, (*i. e.* the alimentary and urinary,) he will be liable, on a subsequent exposure to the virus, to another attack, when the disease will manifest itself in the membranes previously spared." On this point alone, then, can Dr. Tripe's claim for originality in definition rest.

The late Dr. Robert Williams divided the action of the poison of scarlet fever into primary, secondary, and tertiary. To the effects of the primary action he assigned that disturbance of the system which precedes the eruption ; to the secondary actions he ascribed the specific inflammation of the skin, and mucous membrane of the fauces ; the tertiary action, he said, was manifested by the occurrence of dropsy, and its concomitant, albuminous urine.



Dr. Tripe ascribes no order of sequence to the actions of the virus.

In support of his position, that, for the future protection of the patient, it is essential that in the primary attack the skin and the alimentary and the urinary mucous membranes should have been the seat of specific inflammatory action, Dr. Tripe states, *first*, that he has met with no satisfactory evidence to show that the same membrane is ever severely attacked a second time. The following cases appear to offer what Dr. Tripe's experience has not been sufficiently extended to afford him; viz., satisfactory evidence that the same membrane *may be* severely attacked a second time.

"In the autumn of 1826," says Dr. A. T. Thompson, "I attended a gentleman who was labouring under scarlatina anginosa, and who informed me that he had suffered twice before an attack of the same disease. He gave an accurate account of the attack, including the fever, the rash, ulceration of the throat, and the desquamation of the cuticle. The second attack was three years after the first; the third, five years after the second."\*

The following cases occurred in our own practice :

When seventeen years of age, and before her marriage, Mrs. C. and her two sisters had scarlet fever; her sisters had copious vivid rash, and tolerably severe sore throat; Mrs. C. had scanty rash, but her throat was very severely affected. When six years of age, Miss E. C., a daughter of Mrs. C.'s, and her three elder brothers and sisters, had scarlet fever. In 1847, we attended a son of Mrs. C.'s, with scarlet fever, the rash and throat affection being well marked; Mrs. C., while attending on her son, became affected with severe angina, attended by much constitutional disturbance, but no rash; then Miss E. C. also had an attack of angina; the three elder brothers and sisters escaped all illness, but subsequently the two younger brothers of Miss E. C. had angina and scarlet rash.

Dr. Tripe states, *secondly*, that patients who have had either *S. simplex*, *S. faucium*, or renal scarlatina, may, on exposure, be affected with a disease in the membrane or membranes previously exempt. In proof of this assertion, Dr. Tripe narrates the case of four persons, Mrs. M. and her three children; the mother and eldest daughter had previously had scarlatina simplex: the poison of scarlatina was introduced into the house; the two younger had scarlatina maligna; the two elder, severe cynanche without rash.

Now, cases so loosely detailed, even if numerous, would not bear out Dr. Tripe's assertion. What ages were Mrs. M. and her elder daughter when they suffered from scarlatina simplex? and if young, on what evidence does the assertion rest that they then had no sore throat? Moreover, in adults the mucous membrane of the fauces, in a very large proportion of cases, suffers before the general constitutional disturbance is manifested, showing that in them it is particularly susceptible to the influence of the poison; while in children the skin is occasionally affected before the system at large has shown any disorder, proving that in them the cuticular tissue is the more readily affected. Consequently, if in an adult one membrane only suffers, we should expect that to be the faucial; if in a child, one only, the cuticular. Again, to prove his point, Dr. Tripe should have

\* A Practical Treatise on Diseases affecting the Skin, p. 5. 1850.

adduced cases in which *scarlatina faucium* was followed by *scarlatina simplex*. With reference to this subject, Dr. Robert Williams says, that those who have had *cynanche*, in consequence of exposure to the virus of scarlet fever, are insusceptible to the action of the poison on subsequent exposure; consequently, according to that philosophical physician, *scarlatina faucium* protects from *scarlatina cutis*.

The following case is given by Dr. Tripe as strongly confirmatory of the assertion, that those who have suffered from *scarlatina* without affection of the kidney, are liable to disease of that organ on exposure to *scarlatina virus*. A child, *æt.* 5, had scarlet fever, with severe *cynanche* and rash; he was not attended by Dr. Tripe, and no evidence could be obtained as to the condition of the urine during that attack: when seven years of age, he was exposed to the poison of *scarlatina*, he then had slight *cynanche* only, followed by *anasarca* and albuminous urine. This case proves nothing; there is as much reason to believe the urine to have been, as not to have been, albuminous in the first attack; and, again, in the second attack the child had *cynanche*.

Dr. Tripe affirms, *thirdly*, that a protected individual who has *cynanche* only, from exposure to *scarlatina virus*, may, while so affected, emit a poison capable of exciting the complete disease in an unprotected person.

Abundant evidence exists to prove, that a person unprotected by a previous attack of scarlet fever, may, on exposure to its virus, have *scarlatina faucium*, and then himself generate a poison capable of exciting the fully developed disease in another. But Dr. Tripe's statement is, that if an individual who has *previously* had scarlet fever contract *cynanche* from exposure to *scarlatina virus*, he may generate a poison capable of exciting scarlet fever with rash in another. A case is adduced as proof by Dr. Tripe:

“Master M. died of *scarlatina maligna*, June 14th.  
 Mrs. M. was attacked with *cynanche*, without rash, June 16th.  
 Miss M. do. do. July 2d.  
 Miss E. M. was attacked with *S. maligna*, July 11th.”

Dr. Tripe argues that Miss E. M. must necessarily have caught the disease from exposure to a poison generated by her mother or sister. The period of incubation for scarlet fever is unfixed; from twenty-four hours to ten days is the time generally given. The first symptoms of smallpox occur fourteen days after exposure to the poison. Now supposing, therefore, Miss E. M. to have been infected on the 1st of July, and her brother to have been buried on the 20th of June, ten days only would have elapsed between the removal of the body of her brother and the commencement of the incubative period of the disease in Miss E. M.; and fomites might unquestionably have retained the power of communicating the disease much longer than the period here specified. Thus, scarlet fever attacked several boys in a school; the others were sent home, and the house subsequently cleansed; on the return of the boys, a month after, the disease again broke out. And we have recently known a case, in which the poison appears to have lain dormant in a similar manner for three months.

In order to account for the immunity of those who have had scarlet fever, from a second attack, the blood is generally supposed to experience some change during the first attack, whereby it is rendered insusceptible of that morbid action or state which is the essential disease, and to which

all the phenomena or symptoms, as rash, redness of the fauces, &c., are referable. Various opinions have been entertained as to the particular change experienced by the blood in the first attack.

Dr. Mitchell, of Philadelphia, holds that the symptoms of scarlet fever, in common with the other diseases of the class to which it belongs, are produced by the presence of a species of fungus in the blood; that this fungus leaves in the blood a residue, which renders that fluid henceforth incapable of affording support to the same species of fungus; just as some plants emit from their roots a substance, making the soil in which they grow incapable of supporting the same species of plant for various periods of time.

Liebig supposes that a substance susceptible of a change analogous to fermentation, exists in the blood before the occurrence of scarlatina; that the virus acts as a ferment; and that the insusceptibility of protected persons to a second attack is caused by the destruction, during the course of the disease, of all the fermentable matter. It is evident that neither of these, nor any other theory, such as that of Mr. Paget, by which a change in the condition of the blood is considered to be the only condition essential to prevent recurrence of the disease, could account for what Dr. Tripe regards as an established fact, *i. e.* that scarlatina may, and does under certain circumstances, occur three times,—once as scarlatina simplex, once as scarlatina faucium, and once as renal scarlet fever; for were the morbid state of the blood the only essential condition of the disease, and the skin-eruption, the pharyngeal and renal inflammations mere accidents, dependent for their development on individual or atmospheric peculiarities, then, when that blood had experienced its change, would the individual be protected from a second attack, as effectually by scarlatina simplex, as by scarlatina in which the three membranes suffered. The blood would have been freed from the fermentable matter of Liebig, or would have had left in it the fungoid excretion of Dr. Mitchell, as well by the one form of the disease as by the other. Dr. Tripe accordingly says:—

“Liebig considers this protection to arise from some change which occurs in the blood; whilst I think that it is from some change effected in the tissues of the membranes which are susceptible of the virus. The virus, having been taken into the blood, is *evidently* thrown off by the skin and mucous membranes of the alimentary canal, and of the kidneys—in the same way as that of typhus appears to be excreted by the glands of the intestines—and *thus* produces the symptoms which we class under the denomination scarlatina.”

The italics are our own. The *style* of reasoning forcibly recalled to our mind the following passage in Huxham's Essay on Fevers:—

“A very strong elastic set of vessels act with great force on the contained fluids, and produce much friction, and *of course* great heat, and withal a constant and large dissipation of the more subtile and aqueous parts, which render the blood globules, in proportion, more numerous, more dense, and compact, and the humours in general more viscid; as *is evident* by the state of the blood always observable in strong, laborious people, which is ever of this kind.”

By a sufficient use of such evidently's and of course's, the moon could easily be proved to be made of green cheese.

In the concluding paper of the series Dr. Tripe modifies his theory, and attempts to amalgamate it with that of Liebig:

“We may then consider that the blood is, as far as it will at the time admit of it, altered entirely by the attack, and that all the material capable of taking on the

fermentation, or other process, when the virus is absorbed into the system, is changed in the first attack, whether that be one of scarlatina simplex, mitior, or gravior."

But then he thinks that, in addition to this change in the blood, it is essential that the tissues sensible to the specific action of the virus should have had that specific action exerted on them; or else, when the individual is again exposed to the virus of scarlatina, it will be absorbed, will fail to produce any action on the blood, because by the previous attack that fluid has been rendered insusceptible, but will select the tissue not protected by a previous attack, and will thus produce skin eruption without sore throat, cynanche without skin eruption, or desquamative inflammation of the kidneys, according as either of these parts, by reason of having escaped in the first attack, is susceptible to its action. This local action of the poison, Dr. Tripe thinks, may give rise to a multiplication of the specific virus.

"I am of opinion, and think, that the action between the poison in the blood and the membrane diseased, gives rise to the formation of an increased quantity of the virus; and that an increase of the poison might thus occur, totally independent of any augmentation in the blood."

Such is the theory propounded by Dr. Tripe, to account for what he avers to be a fact, viz. that in order to protect an individual from a second attack of scarlet fever, he must, during the first, have experienced specific inflammation of the skin, and of the alimentary and renal mucous membranes. We have shown the insufficiency of the evidence he has adduced in support of his assertion, and have pointed out *facts* in direct contradiction to it; it is unnecessary, consequently, to examine the hypothesis further.

The following are the symptoms of scarlet fever, in the order of their sequence, as observed by Dr. Tripe anteriorly to the outbreak of 1847:—fever; headache; languor; stiffness and soreness of the neck; vomiting; increase of the febrile symptoms; eruption on the second or third day; cynanche; derangement of the urinary organs.

"In the present epidemic," he says, "this order did not ordinarily obtain, the earliest symptoms usually consisting rather in vomiting or diarrhoea, or in both; these did not happen in all, but were observed in every severe case; indeed so constantly did this happen, that I was enabled, with one exception, to prognose the course of the disease by the premonitory symptoms. All the cases in which diarrhoea and vomiting appeared amongst the premonitory symptoms, were attended with severe inflammation and ulceration of the tonsils."

In order to show the exact relation which existed between the vomiting and diarrhoea, and the severity of the throat-affection, Dr. Tripe enters into the following analysis of 36 cases of which he kept notes.

Of these 36 cases, 20, or five-ninths, commenced with vomiting and purging; 8, or two-ninths, commenced with vomiting only; and 8, or two-ninths, did not suffer from the one or the other.

Of the five-ninths, or 55·5 per cent., which began with vomiting and purging, 80 per cent., being 44·4 per cent. of the whole number of cases, had severe cynanche; and the other 20 per cent., being 11·1 per cent. of the whole number of cases, had slight cynanche.

Of the two-ninths, or 22·2 per cent., which commenced with vomiting without purging, the whole had sloughing of the fauces and palate, and inflammation of the parotid, without any swelling of the tonsils.

Of the remaining two-ninths, or 22·2 per cent., which were not affected with diarrhoea or vomiting, none had severe, and only about half had slight cynanche; the remainder were cases of scarlatina simplex.

Thus, then, vomiting was the first symptom in 28 of 36 cases; and in 24 of those 28, serious disease of the throat set in at a subsequent period of the disease, and in 4 of the 28 there was extensive sloughing of the mucous membrane of the fauces. While of the remaining 8 of the 36 cases, not one was affected with vomiting, and not one suffered from more than trifling cynanche; while four escaped without any sore throat.

The relation between vomiting at the outset of the disease, and the subsequent condition of the throat, is a subject worthy of a more extensive investigation. That in many cases the relation is intimate, the analysis of Dr. Tripe clearly proves. Whether the coincidence in the vomiting at the outset of the disease, and the severe throat affection subsequently, was peculiar to the epidemic of 1847-48, remains yet to be shown. Should it prove a general truth, the importance of the knowledge of the fact, as influencing prognosis, will be evident. Nothing short of a numerical analysis of an extensive series of cases, can settle the question here raised.

The dates at which the eruption respectively appeared, in the 36 cases of which Dr. Tripe took notes, were as follows:

In 20 out of 36, or 55·5 per cent. of the cases, the eruption appeared on the second day of the febrile symptoms; in 7 out of 36, or 19·4 per cent., on the third day; in 3 out of 36, or 8·3 per cent., on the fourth day; while in the remaining 6 out of 36, or 16·6 per cent., the time could not be decidedly ascertained, either from the febrile symptoms being very slight, or because the patients were unwell previously. So that of the 30 patients in whom the date of eruption was clearly ascertained, in 20, or two-thirds, it appeared on the day which has by general consent been fixed, as that on which it ordinarily shows itself.

"It is," says Dr. Tweedie, "however, probable that in the majority of instances, the rash comes out on the second day of the fever, and that in cases in which it appears earlier or later, there is some peculiarity in the individual, or in the disease, to account for the variation."

The colour of the eruption was noted by Dr. Tripe in these 36 cases. Speaking generally, he says, "that it partook more of a purple than of a bright red tint. In 26 out of 36, or 72·2 per cent., it resembled that of measles so closely, that if we had judged by that alone, the disease would have been mistaken for it."

The prognosis in those cases in which the hue was even more purple than that of rubeola, was not more grave than it was in those in which the tint of the skin was bright-red. Of the two cases which proved fatal, the eruption in one was purplish; in the other bright-red.

"The cynanche," Dr. Tripe states, "commenced on the second day of the eruption; having been preceded by a congested condition of the fauces in 44·4 per cent. of the whole number of cases. Most of the patients presented numerous small aphthous-like ulcers on the tongue, fauces, and tonsils. The application of a solution of nitrate of silver not only caused these ulcers to heal rapidly, but if applied more extensively, appeared to exert a beneficial influence on the congested and inflamed mucous membrane of the fauces."

Enlargement of the parotid occurred in several cases, in two it was so



great as to threaten the life of the patient. The enlargement of the gland usually commenced on the fifth or sixth day of the eruption.

Coma and delirium, Dr. Tripe thinks, have a very different signification, according to the date of the disease at which they supervene; and he thus divides the cases in which these symptoms are present.

1st. Those in which coma and delirium appear previously to the eruption. In such circumstances he regards the symptoms to be invariably due to the direct action of the poison on the nervous system.

2d. Those in which coma and delirium supervene after the eruption has come out; and these he subdivides into the inflammatory and the non-inflammatory. In the inflammatory cases, turbid serosity or lymph is found in the cavity of the arachnoid. In the non-inflammatory, there is either excess of serosity in the ventricles or arachnoid cavity, or no lesion within the cranium is discoverable after death to account for the fatal termination. In some of these cases, compression of the jugular by the swollen parotid appears to have some influence in hastening death; but in other cases it is highly probable that the patients died in consequence of the retention of urea in the blood, and the direct action of that poison on the brain.

With regard to the symptoms referable to the specific action of the scarlet fever virus on the renal substance, Dr. Tripe's observations were not (he informs us) numerous; and instead of stating in how many cases he examined the urine, and how often, and at what stages, in each particular case, he speaks only in general terms. This diminishes very materially the value of this part of his papers.

In both *scarlatina simplex* and *anginosa*, Dr. Tripe states, that as the disease advances, the urine not unfrequently becomes albuminous; and "if an increased quantity of epithelial scales should be present, they will generally be met with in the greatest quantity at the termination of the eruptive period, just previously to the desquamation." The fact that albumen may be found in the urine of a majority of persons suffering from *scarlatina simplex*, has since been confirmed by Dr. J. W. Begbie.\* That gentleman examined the urine daily of twenty-one persons suffering from *scarlatina simplex*, not followed by dropsy, and he found that "in all these cases albumen manifested itself in the urine, as recognised by the application of heat, or the addition of nitric acid." With two exceptions, Dr. Begbie states, "the period of the disease at which the albumen appeared was a few days after the commencement of desquamation. In a few the process of desquamation had been going on for several days, and in one case was almost complete before the albumen appeared."

In the two exceptional cases the albumen made its appearance while the eruption was yet brilliant. In one of these two, blood-discs, and in the other, pus-corpuscles, were detected in the urine. The duration of the albumen in the urine varied in Dr. Begbie's cases from forty-eight hours to ten days. The usual period was four or five days. Having once disappeared, in none of the twenty-one instances did it recur.

The long continuance of the albumen in the urine is regarded by Dr. Tripe as indicating either that the convoluted tubes of the kidney are impacted by the desquamation of their epithelium, or that a state of subacute

\* *Edinburgh Monthly Journal*, Jan., 1849, p. 443.

inflammation of the organ exists. In the former case he regards the exudation of albumen as merely mechanical. Subacute inflammation of the kidney is indicated by the presence, in the urine, of the fibrinous "casts of tubes" of Simon.

With reference to the dropsy which sometimes occurs after scarlet fever, Dr. Tripe considers that it may in some cases arise from suppression of the perspiration during extensive desquamation of the cuticle, and from simple debility of the areolar tissue, the result of previous congestion and distension. In these cases the urine is, he says, non-albuminous, at least in a large majority of instances. The existence of a form of anasarca occurring subsequently to scarlatina, of which the above is the pathology, appears to us, to say the least, very doubtful. Dr. Tripe does not adduce a case to illustrate its symptoms, nor does he detail the symptoms by which it may be distinguished from the anasarca dependent on renal disease.

Dr. Miller's attention appears to have been especially directed to the renal dropsy which occurs in connection with scarlatina. In his very large collection of cases, there is no example of anasarca from debility after scarlet fever. The following exhibits at once the character of the epidemic, and the extent of the field for observation enjoyed by Dr. Miller. As many as 219 cases of scarlet fever occurred during 1848, between the months of May and October inclusive, in the practice of the Dispensary with which he is connected. Of these, 119 were cases of scarlatina simplex, one of which proved fatal from an attack of pneumonia. None of the cases of scarlatina simplex exhibited any "manifest renal symptoms." As these cases were attended at the homes of the poor, we suppose that no daily chemical examination of the urine was made in the large majority of them; in two cases (73 and 75), albumen was found in the urine on the fifth and sixth days respectively; the rash at the time was well out, and there was no marked œdema. So far, then, as these cases go, they offer support to the general statement of Dr. Tripe, that albumen is not unfrequently present in the urine of patients suffering from scarlatina simplex; and to the more definite, and therefore more valuable observation of Dr. Begbie, that in 21 cases of that form of scarlet fever, he found albumen in the urine in every instance. Of the 219 cases, 21 were cases of scarlatina anginosa, of which 11 died,—the latter being chiefly infants:—10 were cases of true scarlatina maligna, of which number 9 died:—69 were dropsical cases, and of these 8 died. Dr. Miller does not state, however, from what form of scarlatina these 69 cases had suffered.

As to the date of the occurrence of the renal and dropsical symptoms, he tells us:

6 of the cases became anasarcaous between the termination of the first month, dating from the decline of the eruption, and the sixth week.

12 on the twenty-first day.

4 between the fourteenth and twenty-first day.

12 on the fourteenth day.

4 on about the twelfth day.

11 between the seventh and twelfth day.

5 on the seventh day.

3 while the eruption was yet vivid.

In 13 cases, symptoms of renal dropsy set in, without the specific action of the scarlatina virus having been previously manifested on the skin, or

mucous membrane of the fauces. These cases, especially, are adduced as evidence in favour of the opinion held by Dr. Miller, in common with several of the best writers on the subject, and especially the late Dr. Robert Williams, viz., that the renal affection is due to a specific action of the scarlatina virus; and that it is an essential concomitant, and not (as is maintained by some) a sequela of the disease.

"The general symptoms of the cases observed in this epidemic," Dr. Miller remarks, "strongly contend, irrespectively of exposure, a cold season, or desquamation, for a primary influence of the poison on the kidney. The ordinary period of the occurrence of renal symptoms,—viz., from the fourteenth to the twenty-first day, very frequently on those particular days, is probably in accordance with the normal action of the scarlatinal poison, still active, on the kidney."

Dr. Miller remarks, that his cases appear to point to the twenty-first, fourteenth, and seventh days after the decline of the eruption, as those on which the anasarca is particularly likely to occur. Rosen de Rosenstein, as we have already remarked, states that in the Swedish epidemic, dropsy began on the twenty-first or twenty-second day. Dr. Miller adds, that true Bright's disease is sometimes the result of a permanent morbid action, originating in the action of the poison of scarlatina; and seven or eight cases of this description, he states, have fallen under his notice; the patients suffering, that is to say, from dropsy, years after an attack of scarlet fever. To this chronic disease alone, Dr. Miller thinks that the term sequela should be applied. The consideration of the morbid changes in the kidneys, we shall reserve for a future occasion.

By far the most valuable portion of Dr. Tripe's series of papers, however, is that in which he examines the influence exercised on the diffusion of scarlatina by different states of the atmosphere. The analysis we had prepared of this portion of his inquiry we must postpone to our next Number, our disposable space being now exhausted.

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#### ART. XIV.

*On a New and Successful Treatment of Febrile and other Diseases, through the Medium of the Cutaneous Surface. Illustrated with Cases.* By WILLIAM TAYLOR, Surgeon to the Clerkenwell Infirmary, and to the Police.—London, 1850. 12mo, pp. 170.

WHATEVER may be the scientific merits or demerits of Mr. Taylor's production,—and of these we shall presently try to help our readers to form an opinion,—it has the great merit, in contradistinction to many books of its class, of being purely professional in its character. It is dedicated to Mr. Stanley; is published, the author assures us, in compliance with the wishes of his professional brethren; and bears every evidence of being honestly intended, not for the glorification of Mr. William Taylor, but for the benefit of mankind. Nothing can be better in its spirit than the following passage from the preface:

"When a practitioner has found a remedy invariably efficacious, it is his bounden duty to acquaint his fellow-labourers with the success he has met with; his field of usefulness is thus spread immeasurably wide, and he has the pleasing consciousness of having contributed his mite to the relief of the sufferings of humanity. Yet, I conceive, that such a communication ought not to be made hastily; for I think

that years of careful observation, trials of various kinds, and unbiassed and unenthusiastic deductions, are the basis on which any new method ought to be supported."

The author seems honestly to believe that he has fulfilled these requisites; and we have no reason to doubt his good faith. We must take leave, however, to question his logic; and must confess that we were prepossessed against his system, before we knew in what it consisted, by the *invariable efficacy* claimed for it in the very first line of the book. On going further into it, we became more and more satisfied of the excellence of Mr. Taylor's intentions; but our faith in the merits of his supposed discovery has not received a proportional accession.

Nothing can be simpler than the plan of treatment here advocated. It consists in the *inunction* of the skin (which is continually referred to as *induction*, whether intentionally, or by a clerical error, we cannot make out,) with a "hard ointment," composed of equal parts of suet and lard, rubbed in "with vigour and energy," not by old and weakly nurses. We are told little, however, in regard to the mode in which this application is to be employed; over what proportion of the surface it is to be made at one time; or how frequently it is to be repeated. With regard to the former point, we are only informed that the ointment is to be rubbed in over "every part of the skin which is hot and dry." Now, in most of the febrile diseases in which Mr. Taylor considers the remedy especially efficacious, the *whole* surface is hot and dry;—does he mean that the whole is to be anointed and rubbed at once? With regard to the frequency of the application, Mr. Taylor only says that it should be made "night and morning, more frequently in urgent cases;" he does not tell us *how* frequently he has ever used it with apparent benefit, nor how often *per diem* the patient's entire surface has been thus enveloped in the hard ointment. The quantity which it is requisite to use, we are informed, will vary according to the state of the skin; the friction must be continued "freely, but gently, during from half an hour to an hour at a time, indeed, until the skin is saturated; when, from being harsh, hot, and dry, like washed leather, it becomes soft, and, for the most part, after a few applications, yields the feeling of velvet." We should much like to have been told how much of the hard ointment is ever needed at any one time, to produce the requisite "saturation."

Now, we certainly should have been among those who would expect, *à priori*, "that a greasy ointment is calculated to obstruct and clog the cutaneous surface, and prevent the skin from acting as an emunctory." Mr. Taylor assures us, however, that such is not the case; but that, on the contrary, "the natural functions of the skin, far from being impeded, are rendered considerably more active." And, on further reflection, we have found reason to believe that such may be the case. For the experience of all nations inhabiting the warmer regions of the globe, whose bodies, with little protection of clothing, are much exposed to the solar heat, has led them to the conclusion that the functions of the skin are best preserved, and its activity as an exhaling surface maintained, by continual inunction with oleaginous substances. Nature, indeed, has herself provided for this in many of the darker races of mankind; the action of their sebaceous glandulæ being so energetic, that the skin, without any artificial applications, always presents a soft unctuous surface. And we know it to be the uni-

versal assurance of hair-dressers among ourselves, that oleaginous applications to the scalp are much more efficient, not only in promoting the growth of hair, but in keeping the skin cool and moist, than spirituous or other lotions, which produce a greater temporary refreshment, when applied to the head whose surface has been heated by the determination of blood consequent upon cerebral labour.

We are quite disposed, then, to believe that oleaginous frictions may do much to restore the natural actions of the skin, when these are disturbed in febrile diseases; but we should not hence expect, notwithstanding our sense of the great importance of the healthy activity of that organ, to find them exerting such a marvellous influence over the course of severe maladies, as that which is imputed to them by Mr. Taylor; and our confidence in the validity of his observations is not strengthened, but, on the contrary, greatly weakened, by the mode in which they are recorded. Thus, for example, we are assured (p. 10) that the use of the hard ointment in typhus and the exanthemata speedily dispels the offensive fœtor emanating from the patient, and removes the fear of contagion. But we find, in all the cases in which this circumstance is noticed, that *ventilation* was employed at the same time; and we take leave to think that this had a more potent influence than the inunction. So in the treatment of fever, generally, we find that other medicaments were used to such an extent, as to preclude the possibility of assigning the actual value of the inunction; and the author, moreover, seems to us to have entirely lost sight of the fact, that cases of simple fever will very commonly run their course, and arrive at a favorable termination, without any other treatment whatever than rest, fresh air, and a duly regulated diet. Such was Case I, in which, in addition to the friction, Mr. Taylor ordered "grain doses of calomel and saline medicine every four hours;" and yet naïvely says,—"In this case it was perfectly palpable that the friction reduced the pulse from 120 to 80, in two days. What other remedy can produce such an effect in so short a time?" Now, considering that the man had been previously ill for seven days, and that nine days is a very common period for the type of fever to which (in the absence of more particular description) we may presume his malady to have belonged, we feel fully justified in the assertion, that there is an absence even of probability, that the lowering of the pulse was in the least degree consequent upon the oleaginous inunction.

Nor will other cases better tolerate the scrutiny, to which every one who is accustomed *logically* to consider the facts of medicine would feel it essential to subject them. Take, for instance, the following example of Mr. Taylor's treatment of a case of dropsy, which he considers one of the most extraordinary upon record. "I ordered him," says Mr. Taylor, "blue pill, with James's powder, every night, to act upon his liver, and a sulphate and carbonate of magnesia draught, with half a drachm of colchicum wine, every morning, to act on the bowels; a saline diuretic mixture every four hours; and the hard ointment to be rubbed in." That a man whose illness was brought on by drinking two quarts of gin a day, besides malt liquor, and occasionally brandy, and who had not previously been subjected to any medical treatment, except his own remedy of rust of iron mingled with cider, should be speedily relieved by this vigorous attack upon his liver, bowels, and kidneys, is not at all surprising; and the only wonder to us is, that any man of ordinary intelligence should



narrate such a case as an illustration of the special virtues of his pet remedy. We have carefully searched for a single instance that should be even tolerably free from the fallacies we have pointed out; and we can honestly say that we have not found one.

Yet, as we have before remarked, we are by no means indisposed to believe that this plan of treatment may promote the healthful action of the skin; and we are, moreover, decidedly of opinion, that too little is done by *direct* applications, to excite this important organ to increased activity, and that too much faith is placed in the internal exhibition of diaphoretic medicines, which are frequently of very doubtful efficacy. Even nauseating doses of antimonials, ipecacuanha, &c., often fail to induce a copious perspiration; or, if they produce the effect at all, it is but temporary, and the skin soon returns to its original dryness. If it *can* be kept soft, moist, and cool, by Mr. Taylor's method, we do not in the least doubt that the severity of many diseases will be thereby mitigated; although we should not anticipate the wonderful results which Mr. Taylor imputes to it. The only statement that affords the least basis for the estimation of the success of his method, is the following. He assures us, that, since his treatment has been adopted in the Clerkenwell Infirmary, during the twelve years between 1837 and 1849, between 200 and 300 cases of fever occurred in the Clerkenwell Infirmary, *without a single death* in idiopathic cases; whilst, previously, the deaths from fever in that establishment were from ten to twenty annually; and, in 1836, twelve patients died of typhus in one month. But even this statement is far from impressing us, as the author doubtless expects that it should do; for, in the first place, the looseness of his statement as to the entire number of patients treated,—*between 200 and 300*,—does not inspire us with any great confidence in his statistical accuracy; and, in the second place, the mode in which his fever cases are reported leaves us entirely in the dark as to what he would consider “idiopathic” cases, and what he would class as “complications.” For anything that appears to the contrary, all the deaths previous to 1837, like all those that have subsequently occurred, may have been attributable to “complications.”

Let Mr. Taylor publish two sets of carefully-recorded cases, in one of which his system of treatment shall have been employed; whilst in another, of as nearly parallel nature as possible, the ordinary appliances shall have been used; and the profession will then have some means of forming a judgment of the value of inunction with the “hard ointment.”

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## PART SECOND.

## Bibliographical Notices.

ART. I.—*On Excision of the Enlarged Tonsil, and its Consequences in Cases of Deafness. With Remarks on Diseases of the Throat.* By WILLIAM HARVEY, Surgeon to the Royal Dispensary for Diseases of the Ear.—London, 1850. 8vo, pp. 121.

THIS little Treatise is highly creditable to Mr. Harvey; who has carefully set himself to ascertain the real value of the operation of the Excision of enlarged tonsils, which has been vaunted as a cure for a large number of cases of deafness. Some of the results of his inquiries were communicated to the Medical Society of London in 1848; but he has, since that time, obtained a large amount of additional information upon the subject; and it does not surprise us to learn that his conclusions are altogether unsatisfactory. "He has endeavoured to show," he says, (Preface, p. vi,) "that the operation exercises no beneficial influence whatever over such imperfections of hearing, and pointed out the misery oftentimes resulting from it. He has also shown how such misery may be avoided, by adopting the more comprehensive line of treatment here set forth. That occasions may arise when operations may be called for, in order to afford relief for the distress occasioned by enlarged tonsils, he does not mean to deny; but that deafness could be one of those occasions,—at least, as long as the anatomical relations of the tonsil and the aperture continue the same as they now are,—he can by no means admit." The cases which he has collected seem amply to justify these remarks.

Mr. Harvey's observations have led him to the conclusion, that chronic enlargement of the tonsils is generally connected with some morbid condition of the system generally,—strumous, gouty, rheumatic, or the like; and he urges the importance of *constitutional* treatment, as the only means by which the disease is likely to be subdued. In this, we have no doubt that he is correct. Follicular disease may or may not coexist; when it does, he advocates the local treatment by nitrate of silver, which is now so generally and successfully employed. We find nothing in the plan of treatment that is peculiar to Mr. Harvey, save the dressing of blisters with colchicum, which he states to have been very efficacious. His treatise, however, is a very creditable one; and it is satisfactory to find an aural surgeon setting himself against that tendency to operative procedures, which unfortunately seems to cling to this branch of the profession, in a degree that will render every calm-judging practitioner suspicious of the results so ostentatiously paraded.

ART. II.—*The Surgical Anatomy of the Principal Regions of the Body.*

By THOMAS MORTON, late F.R.C.S., and Assistant-Surgeon to University College Hospital; and WILLIAM CADGE, F.R.C.S., and Assistant-Surgeon to University College Hospital.—*London*, 1850. Royal 8vo, pp. 395. With Twenty-three Plates and numerous Wood-engravings.

It is well known that Mr. Morton had produced, at successive intervals, a series of Treatises on the Surgical Anatomy of several of the most important regions of the body, which had acquired for him a high reputation for anatomical knowledge, and for skill in imparting it. The first of these was devoted to the Perinæum; the second to the Groin and the Femoral and Popliteal Regions; and the third to the Testis and its Coverings, including Inguinal Herniæ. There remained only, therefore, the Head and Neck and Upper Extremity; and for these he had prepared the necessary drawings, some time previously to his lamented death. The Commentary upon these has been prepared by Mr. Cadge, who has succeeded Mr. Morton in his appointment; and we have much satisfaction in stating, that it is executed in a style which does no discredit to the original. Having held the appointment of Demonstrator at University College, Mr. Cadge could scarcely be otherwise than familiar with the anatomy of the parts he describes; and it is obvious that he has seen much surgical practice, and has made judicious use of what he has seen. Moreover, the style is good, being at the same time concise and free from obscurity.

Altogether we can strongly recommend the work as one of standard value. It is greatly inferior in comprehensiveness to Mr. Maclise's; and as regards the execution of the plates, will scarcely bear comparison with his. But as far as it goes, it is very complete; and may be most advantageously employed as a companion in the dissecting-room, whilst Mr. Maclise's more bulky folio will serve to refresh the memory in the study.

ART. III.—1. *Physician and Patient; or a Practical View of the Mutual Duties, Relations, and Interests of the Medical Profession and the Community.* By WORTHINGTON HOOKER, M.D.—*New York*, 1849. 12mo, pp. 453.

2. *Lessons from the History of Medical Delusions; being the Fiske Fund Prize Dissertation of the Rhode Island Medical Society.* By WORTHINGTON HOOKER, M.D.—*New York*, 1850. 12mo, pp. 105.

OUR readers will doubtless recollect that in our number for October last, we reviewed Dr. Bentley's English edition of Dr. Hooker's 'Physician and Patient;' and, whilst speaking very favorably of the work, took occasion to express regret that Dr. Bentley should have left it so uncertain "how much belongs to the original author, and how much to his editor." We have since received from Dr. Hooker a copy of the original work; with a heavy complaint against his English editor, whose production, he affirms, is not merely a "garbled" edition, but is "badly, unskilfully, not to say unfairly, garbled." Some allowance is doubtless to be made for a little over-sensitiveness on the author's part; and we can see reasons for many changes which Dr. Bentley has made, which are very probably not apparent to Dr. Hooker. The original, as Dr. Hooker justly remarks, will give a much clearer idea of the state of the medical profession in the United

States, and of the difficulties with which it has to contend, and of the various forms which empiricism has assumed in that country, than the English edition does. But, in reproducing it here, Dr. Bentley did not intend to enlighten English readers on the "Thompsonism" and "Natural bone-setting" of America; having rather in view to give the book that adaptation to the wants of the profession and public in this country, which the original possessed in its own. Still, in doing this, he has unquestionably taken undue liberties with his author; and it is a little singular that a deficiency on which we commented,—namely, a notice of the *homœopathic* practice of administering *allopathic* doses,—should have been the result of an omission made by Dr. Bentley, with what motive we are at a loss to divine. So far as we have observed, the American editors of English works do not cut and mangle their authors after this fashion, but content themselves with *adding* passages, which are clearly distinguished from the original text. It is not often that we pay an American medical author the compliment of reprinting his work; but, when we do, we think he has a right to expect that if omissions be made, the purport of these shall be specified, and, if additions, that they be so incorporated as not to leave the relative responsibility of author and editor in any doubt.

With the original of the preceding treatise, Dr. Hooker has forwarded a smaller work, which is admirable alike in its conception and execution, and which we should most gladly see reprinted and widely diffused in this country, both among the profession and the public. That it received a prize adjudged by the Medical Society of the State of Rhode Island, will be a sufficient guarantee for its *orthodoxy*; but, like a candid friend when consulted in the case of a matrimonial disagreement, he does not hesitate to say that the mutual distrust which is taking the place of the confidence that ought to exist, is the consequence of "faults on both sides." The following passage from the Preface will give an idea of the spirit and method in which he treats the subject:—

"A history of the medical delusions of the *popular* mind alone would be both an incomplete and a superficial history. It would leave much of the truth out of view. It would fail to lay open the secret springs of error. To present a true and full picture of medical delusions, the agency which the *professional* mind has had in the production and diffusion of them among the people must be unsparingly portrayed. This I have attempted to do in this essay; and I think the candid and intelligent reader will allow that the errors and delusions of the medical profession are delineated with quite as much faithfulness as are those which are strictly of a popular origin, and are clearly traced with them to their common source. The exposition which I make is not a partial one. It is not a one-sided argument—a plea for the doctors against the people; but it is an attempt to show how both doctors and people have ever been liable to error, and how they have been alike in the common elements, if not in the forms, and modes, and fashions of their delusions. And while I ask the intelligent and rational among non-medical men to examine the sources of the delusions by which they may have been entrapped, I also earnestly request my medical brethren to look candidly at the errors of our profession; and then, while they aim to achieve a full deliverance from these, they will be less disposed to rail at the errors of the community, and will pursue in charity and patience the proper measures for their removal."

We trust that this hint will not be lost upon some spirited publisher, and that, in a *verbatim* reprint of Dr. Hooker's Essay, he may receive that justice which is his due.

ART. IV.—*Deafness practically Illustrated: being an Exposition of Original Views as to the Nature, Causes, and Treatment of Diseases of the Ear.* By JAMES YEARSLEY, Surgeon to the Metropolitan Eye and Ear Infirmary.—London, 1850. Crown 8vo, pp. 244.

THE first edition of this work was noticed by us in our number for April, 1848; and the edition now before us differs from that one so little, except as containing the author's papers on the treatment of deafness by the application of moistened cotton-wool, and on the artificial perforation of the membrana tympani, that we should not have thought it requisite to notice the republication; were it not that we are struck with the curious coincidence, that whereas the number of enlarged tonsils removed by Mr. Yearsley up to 1847 was 2000, the number of patients having been 1400, the number of tonsils and of patients remain *at precisely the same figure* in 1850. What may be the meaning of this, we do not profess to comprehend. Has tonsil-cutting been superseded by cotton-wool? Is the trephining of the membrana tympani a neater and prettier operation than the excision of the gland from the throat? Or has Mr. Yearsley seen the error of his ways, and learned from the *after* experience of his patients, that the loss of their tonsils has rather done them harm than good? We ask these questions in all sincerity; simply desiring to know Mr. Yearsley's *present* convictions.

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ART. V.—*Thirty-seventh Annual Report of the Dispensary for the Cure of Complaints of the Eyes, Frogmore Street, Bristol.*

WE depart from our usual rule of not noticing reports of individual institutions, in order to bring under the cognizance of our readers some peculiar features in the above-named charity. Instituted by Mr. Estlin, in the year 1812, when there were but few Ophthalmic institutions in the country, it was for some time supported at his own expense; as the number of patients applying for relief increased, he applied for assistance to a few personal friends; and the progressive extension of its sphere of operations has been met by a corresponding liberality, so that no want of pecuniary means has ever been experienced. In October, 1849, the total number of patients admitted from the commencement of the institution amounted to *fifty thousand*; and the annual average is now above *two thousand*. Yet the average annual expenditure is not above *seventy-five pounds*. The mode in which this very narrow income has been made sufficient for the required purposes, may be gathered from the following extract from Mr. Estlin's Report:

"In consequence of the funds raised having been committed to my sole control, all unnecessary expenses have been avoided; and few medical charities can present an example where professional aid to the poor, in an important class of diseases, has been extended so widely at so small an annual expenditure.

"The plan of the Dispensary has been to give advice and surgical aid, together with the required medicines, to all poor people applying for assistance, irrespective of any recommendation. When operations were required, they were performed, either at my own house, or at the residences of the patients, or, on occasions when confinement was needed, (especially in the case of persons who resided at a distance from Bristol), at a house near the Dispensary, where the subjects of them were



lodged and nursed. In some instances the patients or their friends defrayed this extra expense; in others it was entirely or in part paid from the funds of the Dispensary."

During the last ten years, twenty-one patients have thus had the whole or a portion of their expenses defrayed from the funds of the charity.—We happen to know that the benefits of the Institution are by no means confined to Bristol and its immediate vicinity, but that patients come to it from the remotest parts of South Wales, and even from North Devon and Cornwall, attracted by Mr. Estlin's well-earned fame as an Ophthalmic surgeon. In general, where it has been requisite that such patients should remain in Bristol under treatment, it has been found possible, where the patient or his friends could not afford the expense, to obtain assistance from his parish; and this we conceive to be the most legitimate method of affording medical assistance; since it is hard that the inhabitants of any local metropolis should be called upon to provide, not merely the medical skill which a country practitioner cannot be expected to possess, but the material supplies for which the patient would be otherwise dependent on parochial aid, and on which he must remain dependent, if reduced to helplessness by the loss of his sight. We hold up this Dispensary, then, as an example to all those who are concerned in the organisation of medical charities. We believe it to be altogether unparalleled in regard to the amount of good that it does in proportion to the amount of money expended; and in its freedom from the liability to those abuses, which impair the efficiency and increase the expenditure of more pretentious institutions.

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ART. VI.—*Additional Observations on the Nitrate of Silver; with full Directions for its Use as a Therapeutic Agent.* By JOHN HIGGINBOTTOM, F.R.C.S.—London, 1850. 8vo, pp. 40.

MORE than a quarter of a century has elapsed, since Mr. Higginbottom published the first edition of his essay on the Nitrate of Silver; and during this interval, its application has been almost daily made, not only by Mr. Higginbottom himself, but by multitudes of other surgeons, who have borne testimony to its value. Indeed, we do not hesitate to rank its extensive introduction into practice, as one of the greatest improvements of modern surgery. We speak feelingly on the subject, having ourselves had an attack of traumatic erysipelas at once circumscribed by the magic circle drawn by a pencil of nitrate of silver.

We believe Mr. Higginbottom to be right in the suspicion, that the great obstacle to the general and free use of the nitrate of silver, even at the present day, arises from the impression on the minds of many surgeons, that it is a caustic, a destructive agent. Of this impression he shows the fallacy, by comparing nitrate of silver with caustic potass, the latter being legitimately entitled to that designation, whilst the former tends to induce cicatrization instead of producing ulceration.

The pamphlet consists of directions for using the nitrate of silver in the following cases: 1. In recent bruised wounds of the skin. 2. In small ulcers. 3. In large ulcers. 4. In old ulcers. 5. In ulcers attended with inflammation. 6. In punctured wounds, bites, and stings. 7. In wounds received in dissection. 8. In lacerated wounds. 9. In hæmor-

rhage from leech-bites, &c. 10. In incised wounds. 11. In erysipelas. 12. In phlegmonous erysipelas. 13. In inflammation of the absorbents. 14. In phlegmonous inflammation. 15. In small irritable ulcers with varicose veins. 16. In burns and scalds. 17. In variola. 18. In gangrena senilis. To these he might have added ringworm, which we have seen more effectually treated by this application than by any other.—The latter part of the pamphlet consists of cases illustrating the use of the nitrate of silver. We can do no more than recommend to any of our readers, who may not yet be convinced of its value, to read Mr. Higginbottom's pamphlet, and then make trial of the remedy for themselves.

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ART. VII.—*The Scale of Medicines with which Merchant Vessels are to be furnished, by Command of the Privy Council for Trade; with Observations on the Means of Preserving the Health and Increasing the Comforts of Merchant Seamen; and Directions for the Use of the Medicines, and for the Treatment of various Accidents and Diseases.* By T. SPENCER WELLS, F.R.C.S., Surgeon, R.N.—London, 1851. Fcap. 8vo, pp. 190.

THE duty of regulating the Scale of Medicines for Merchant-ships not carrying a surgeon, having been transferred, by the late "Mercantile Marine Act," from the Admiralty to the Board of Trade, a new scale has been issued by the latter; and a most excellent guide to the use of it has been prepared by Mr. T. Spencer Wells, whose large experience in the diseases and casualties to which seamen are more particularly liable, renders him a most competent and trustworthy director; whilst his peculiar facility in imparting information has enabled him to give forth his directions in the form best adapted to the occasion. The following extract from the Preface will show the objects at which he has aimed:

"Extreme simplicity has been studied, and a familiar mode of address adopted, in order to render the advice and directions of the author easily comprehended and executed. Woodcuts have been added for the same purpose. The principal objects have been:—

"I. To avoid all technical phrases, and to inform men of average intelligence how they may preserve health and avoid disease, by following certain simple rules.

"II. When disease arises, how they may place the patient under the most favorable circumstances for recovery, and DO NO HARM by the improper use of medicines; and—

"III. If accidents occur, either on board, where the medicine-chest is at hand, or on shore, far from all assistance, what is to be done to avoid immediate danger, and prevent, as far as possible, future ill consequences.

"A medical man would do many things, when treating some of the accidents and diseases mentioned in the following pages, which it would be most dangerous for any one, not medically educated, to attempt. Such things will not be alluded to, but simple means alone described and recommended, which any man of common sense can easily understand and safely adopt." (Preface, pp. vi-vii.)

The work has been most skilfully executed on the foregoing plan, and will prove, we feel assured, of the greatest value for the purpose to which it is more especially applicable. But its utility need by no means be confined to the seaman. As the author justly observes:

"As many of the accidents and diseases to which seamen are liable are also common on shore, in our sea-ports, colonies, and in many rural districts where medical aid cannot be obtained readily,—and as the principles to be observed for the preservation of health, as well as for the prevention and cure of disease, and the treatment of accidents, are the same, whether ships or houses be inhabited,—it is hoped that this little work will prove useful to persons living in the country, far from a medical man; to clergymen, missionaries, and colonists; and to the numerous class of yachtsmen and travellers." (Preface, p. vii.)

Mr. Wells lays great stress upon the importance of ventilation, cleanliness, and proper diet, as means of avoiding disease; and points out the most efficient means for securing them. We are glad to find him also bearing testimony to the disadvantages of the spirit-ration; asserting, from his own experience, that fermented liquors are not necessary for the preservation of health, and that they are fruitful sources of disease, as well as of moral evil. He speaks highly of the chloride of zinc, the introduction of which is due to Sir W. Burnett, as preventing, when properly used, the formation of offensive bilge-water, and thereby removing a most powerful predisposing cause of disease; and the Appendix contains a large amount of evidence, in regard to the preservative and disinfectant powers of this agent.—Altogether, we consider this the most valuable little work on any branch of *popular* medicine, that has ever fallen under our notice; and we anticipate for it a wide range and long period of usefulness.

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ART. VIII.—*Surgical Anatomy*. By JOSEPH MACLISE, F.R.C.S.—*London*, 1851. Fasciculus IX.

THIS Fasciculus of Mr. MacLise's work takes a much wider range than is included in most works of its kind; but the subjects which he has introduced are of peculiar practical interest and value to the Surgeon. Having displayed, in the last Fasciculus, the surgical relations of the bladder and adjacent structures, in reference to the lateral operation of lithotomy, Mr. MacLise now proceeds to the anatomical comparison of the lateral with the bilateral operation, and to point out the results of the chief variations from the normal disposition of the parts, upon the surgical procedures which each involves. The First Plate contains eight figures illustrative of this subject. The Second is devoted to "Congenital and Pathological Deformities of the Prepuce and Urethra," including stricture and mechanical obstructions of the urethra; and contains no fewer than twenty-six figures. The Third Plate contains twenty-five figures illustrating the "various forms and positions of strictures and other obstructions of the urethra; false passages; and enlargements and deformities of the prostate." And the Fourth Plate includes twenty figures, illustrating "deformities of the prostate; distortions and obstructions of the prostatic urethra." Thus the whole Fasciculus contains no fewer than seventy-nine figures, almost all of them illustrating points of great practical importance to the surgeon. We are left more in ignorance, however, than we could have wished, how many of these figures are purely ideal or diagrammatic, and how many are taken from actual dissections or preparations.

In his Commentary on the last Plate, Mr. MacLise diverges again into his favorite speculations, and revives the old doctrine of the homology

of the prostate gland and the uterus. We much regret to see this brought forwards, being satisfied that it is erroneous. The exact evidence supplied by cases of monstrosity has shown that the prostate, *as a whole*, has no relation to the uterus; but that the true representative of the latter in the male is the *utricleus prostaticus*, or *vesica prostatica*, (see our First Volume, p. 271, and the 'Cyclopædia of Anatomy and Physiology,' Art. "*Prostate*," p. 151.)

We learn from an announcement issued with this Fasciculus, that the work will be completed in another Part; it will certainly be without a rival in this country as to its value and comprehensiveness, and not less so in its marvellous cheapness.

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ART. IX.—*Corpulence, or Excess of Fat in the Human Body; its Relations to Chemistry and Physiology, its Bearings on other Diseases and the Value of Human Life, and its Indications of Treatment. With an Appendix on Emaciation.* By THOMAS KING CHAMBERS, D.M., Fellow of the Royal College of Physicians.—London, 1850. Fcap. 8vo, pp. 166.

THIS neat little book contains, in a revised and improved form, the Gulstonian Lectures which were delivered before the College of Physicians last year, and which were published at the time in the columns of the 'Lancet.' We think that the author has done well to issue them separately; since they contain much information that has not been elsewhere brought together in so compact a form; the only special treatise on the subject,—the well-known 'Comments on Corpulency' of the late Mr. Wadd,—being one which has no pretension to the character of a scientific essay. We cannot perceive that Dr. Chambers has added much to the knowledge of the subject possessed by those who had previously made it their study; and some parts of the inquiry might, we think, have been a little further prosecuted with advantage. For instance, we do not find that he anywhere notices the importance of fat in the process of primary assimilation, as shown in the fact that all cell-nuclei contain fatty particles; nor does he attempt to apply this fact to the instinctive desire for fat, which leads all nations to employ it, under one form or other, in their ordinary diet. We would recommend to his especial consideration the remarkable immunity from scrofulous diseases and phthisis enjoyed by the Icelanders, under circumstances which, according to all ordinary experience, strongly tend to the induction of that disease; the only assignable cause of exemption, so far as can be ascertained, being the highly-oleaginous diet of the people of that most uncomfortable island. (See our Number for April, 1850, p. 461.) We might indulge in a much greater length of criticism, were we to point out all the parts of this treatise which we consider improveable; but this would be unfair to an author who has shown much research in the collection of his materials, and much intelligence in the arrangement of them; and we shall therefore only commend his little book to the notice of those who are curious in the subject, either as a matter of personal or of professional interest.

ART. X.—*Annual Report of the Progress of Chemistry, and the Allied Sciences, Physics, Mineralogy, and Geology; including the Application of Chemistry to Pharmacy, Medicine, Agriculture, the Arts and Manufactures*. By JUSTUS LIEBIG, M.D., Professor of Chemistry in the University of Giessen, and H. KOPP, Professor of Physics in the University of Giessen. Edited by A. W. HOFFMANN, PH.D. F.C.S., Professor in the Royal College of Chemistry, London; and H. BENCE JONES, M.D. F.R.S., Physician to St. George's Hospital. Part I for 1849.—London, 1851. 8vo, pp. 272.

WE regret that we have not taken an earlier opportunity of formally bringing this most valuable work under the notice of our readers, although it was frequently named in our Periscope. The vast number of the labourers in the various departments of Chemistry and the allied sciences, who are now at work in different countries, altogether precludes any single individual, except under peculiarly favorable circumstances, from keeping up his knowledge of what is being done by others, even in his own department, without the assistance of such a work as this; in which, thanks to the effective arrangements of Professors Liebig and Kopp, with their *collaborateurs* (who are all Professors in the University of Giessen), the results obtained by these numerous inquirers are collected and systematised, some notice being given of their methods where this is most needed, and frequently some critical observations being made on their value. The Report for 1847-8, the first, we believe, of its kind, was translated by Professor Hoffmann and Mr. Warren De la Rue; and it formed two goodly volumes, together containing 1100 pages. Mr. De la Rue's other engagements having prevented him from continuing to take the large share in the work which he originally performed, his place has been supplied by Dr. Bence Jones, except as regards the application of Chemistry to the Arts and Manufactures, the department for which, as is well known, Mr. De la Rue is most especially qualified. In the hands of such accomplished editors, we have no doubt that the English edition of the forthcoming volume will be an accurate representation of the German original; more especially as Dr. Hoffmann may be expected to have acquired increased facility in English translation from his former effort, so as to be better able to keep clear of the German idioms which were rather too frequent in the former volumes.—We rejoice to find that the success of the work, as a commercial speculation, has been such as to induce the publishers to continue the English reproduction of these Reports; and we trust that there may be no abatement in the zeal either of its authors or translators; but that year by year we shall be supplied with this most valuable epitome of what has been done for Chemical Science in the previous twelvemonth. The only regret which mingles with our satisfaction, arises from the feeling that Professor Liebig's time might be more worthily occupied, than in such a labour of mere compilation. We regret that his mind should be drawn off from original research, when Animal Chemistry offers so many subjects peculiarly fitted for his methods of investigation. He will speedily find himself thrown into the back-ground by the younger chemists who are rising up around him, and will be reduced to live upon his past reputation, if he does not produce something more worthy of himself, than his treatises on the "Juice of Flesh," and on the "Motion of the Juices."



ART. XI.—*The Moral and Sanitary Aspects of the New Central Cattle Market, as proposed by the Corporation of the City of London.* By J. STEVENSON BUSHNAN, M.D. London, 1851. 8vo, pp. 44. With Plans.

WE are sorry to see that so able a pen as Dr. Bushnan's has been enlisted in favour of the Corporation scheme for the construction of a new Cattle-Market in the heart of the metropolis, in opposition to the Government plan for its removal to the suburbs; and that the merits of the former should be set forth in such glowing colours, as a great measure of sanitary improvement, without a word being said of its disadvantages.

It would really seem, from his account of it, as if the Corporation, *proprio motu*, had come forward to remove this monster evil, with nothing but the most pure and disinterested regard to the welfare of the inhabitants of the metropolis in general, and of the neighbourhood of Smithfield in particular. We venture to predict, on the other hand, that of the squalid population displaced from the site of the proposed new Smithfield, not one tenth part will have recourse to the accommodation provided for them on the old; and that, notwithstanding the boon which the establishment of Model Lodging-houses, Baths, and Wash-houses, &c. will be to the decent artisan, the great mass of the degraded population of the locality, "loving darkness rather than light, because their deeds are evil," will only make the purlieus of Saffron-hill, Field-lane, &c., twice as bad (if that be possible) as they are at present.

Besides, the continuance of the system of slaughtering cattle in the heart of the city, involves the perpetuation of a number of highly objectionable trades, which ought to be prosecuted at a distance from the atmosphere breathed by two millions and a half of human beings. And let Dr. Bushnan say what he will, these nuisances *will* spring up around the new Smithfield, as they have around the old. Really his expectations of the watchful care to be exercised by the Corporation of London, on this and other points, seem like a piece of delicate irony, when their past negligence and apathy on such subjects are borne in mind. We do not question that the City scheme would be, in many respects, a great improvement on the existing order of things, than which, in fact, nothing can well be worse; but we do deny that it is the best possible, or that it ought to receive the support of the medical profession, on the ground of the sanitary improvements which it will effect. If the Corporation is really anxious on this score, why does it not obtain an Act enabling it to purchase the locality about whose welfare it has all at once grown so paternally solicitous; and *gradually*, not suddenly, metamorphose it into a region fit for the dwellings of human beings, allowing the Government Commission to deal with Smithfield and its contingent abominations as it proposes to do?

Dr. Bushnan seems entirely ignorant of the enormous amount of *dead* meat even now brought into London by railroads and steam-vessels; and speaks of the peculiar advantages of the present locality in reference to cattle brought to London from the Docks, (the access being by some three miles of crowded streets,) without any reference to the fact, that such cattle may now be transported direct by railway from the Docks to the immediate neighbourhood of two of the proposed Suburban sites.

## PART THIRD.

## Periscope.

## ANATOMY, PHYSIOLOGY, AND ORGANIC CHEMISTRY.

*On the Absorption of Sugar and Albumen by the Vena Portæ.* By M. BERNARD.

M. BERNARD, in a recent memoir to the French Academy, explains, according to his observations, the precise part taken by the chyliferous vessels in the process of absorption. The author, by a set of well-conducted experiments, has laboured to show the nature of the nutritive principles taken up exclusively by this part of the system; with the object of fixing more definitively the precise meaning and import of the term *chyle*; and with a view also to determine if there exist certain alimentary compounds, which escape the process of absorption through the veins, and consequently arrive at the lungs without traversing the vascular system of the liver.

Nutritive matters taken into the stomach ultimately reduce themselves into three special principles:—Sugar, albuminous matter, and dissolved fatty substances. M. Bernard has experimented on each in detail.

The absorption of Sugar by the system of the vena portæ is proved by the several facts he has related;—having fed several animals with large quantities of sugar, he always found the traces of this sugar very distinct in the blood of the vena portæ; while, on the other hand, he has been quite unsuccessful in his researches in detecting sugar in the contents of the thoracic duct. Hence he concludes, that sugar is exclusively absorbed from the digestive canal by the former set of vessels, and consequently that all sugar entering the system, previous to reaching the lungs, must traverse the liver. M. Bernard states, that if we inject a solution of sugar into the general venous system of a dog, in place of becoming assimilated, it is soon expelled by the urinary excretion. If, however, we make this same injection into a branch of the vena portæ, so that the sugar is obliged to go through the liver before it arrives at the general venous current, the saccharine matters will not be eliminated by the kidneys, but assimilated with the blood, as in the normal process of digestion.

The absorption of Albumen has been tested by M. Bernard in a manner not very dissimilar. Having injected into the jugular vein of a dog a certain amount of white of egg, mixed in water, it was found that not long after, distinct traces of albumen presented themselves in the urine. This experiment seems to show, that the albumen of the egg is not identical with that of the blood, and that it requires, for assimilation, certain previous modifications; these are effected, according to the author, by the passage of the albumen through the tissue of the liver, for on throwing the albumen into the portal system, as in the instance of sugar, it is at once assimilated.

The phenomena observed in the absorption of Fat, are quite different. M. Bernard has already shown, that fatty matters require a previous mixture with the secretion of the pancreas in the small intestine, before they can be absorbed, while albumen and sugar may be taken up from the stomach; this emulsion of fat and pancreatic juice, the author looks upon as the essential ingredient in chyle; and fatty matters,

as might be expected, do not require to pass through the liver, for injecting them into the jugular vein produces no sensible effect, nor does the urine become chylous or fatty; this proposition, however, he offers with some reservation, as the microscope exhibits fat in the blood of the portal system of birds.—*Gaz. Médicale*, 1850.

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*Affection of the Olfactory Nerve, with total Loss of Smell and Taste: with Remarks upon the probable Identity of these two Senses.* By Dr. KNEELAND.

A MARRIED woman, aged 47, had entirely lost the sense of smell and taste for two years and a half prior to coming under the reporter's observation. He found that the smell of strong ammonia was as imperceptible as water, and was only distinguishable from it by its irritating effect on the fifth nerve, the functions of which nerve were entire throughout its distribution. Common sensation of the tongue was perfect, but the organ could not distinguish the tastes of different bodies.

This case, Dr. Kneeland believes, corroborates the view he entertains, that the olfactory nerve is both the nerve of smell and taste, the lingual and glosso-pharyngeal being, as far as taste is concerned, mere names of common sensation, whose division would impair it only by destroying the general sensation of the mucous membrane. However powerful they may be, sapid bodies cannot be tasted as long as the nose is held. The common sensation of the tongue enables us to perceive many qualities of bodies which may be mistaken for the *flavour* dependent on *true taste*, e. g. heat and cold, consistency, form, size, chemical and mechanical action of bodies, &c. He regards those cases in which taste is said to exist, though smell is congenitally absent or lost, as examples of common sensibility, dependent upon the lingual branch of the fifth pair, and not examples of *true taste*. Analogically with what we see in cases of the privation of other senses, the common sensation of the tongue may undergo an amount of development in persons deprived of smell and taste not attainable in others not so situated, and may then be readily mistaken for true taste. On the supposition that taste and smell are identical, and dependent on the olfactory nerve, the lingual nerve, ministering to a peculiar modification of the common sensibility, rejects substances improper as regards temperature, consistency, chemical or irritating qualities, though their odorous properties might not have procured their rejection; while the glosso-pharyngeal, guarding the orifice of the œsophagus, by a reflex action causes the rejection, through vomiting, of any improper substance the olfactory or lingual may have allowed to pass.—*Amer. Journ. of Med. Sc.*, vol. xxi, p. 41.

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*On the Functions of the Chorda Tympani Nerve.* By M. DUCHENNE.

M. DUCHENNE has communicated to the French Academy the result of his electric researches as to the function of the chorda tympani nerve. Having limited the galvanic action on this nerve to a low degree, so as to represent, as nearly as possible, the normal action, he found a very slight sensation in the anterior two thirds of the tongue on the side galvanised; at a higher degree, there was super-added a distinct gustatory sensation, to that of ordinary sensibility. He could not detect any muscular contraction either in the tongue or the velum, nor any appreciable change in the state of the papillæ of the tongue or salivary glands.

In several paralytic patients, in whom the seventh pair of nerves was affected, he found a diminution of the sense of taste and of general sensibility of the tongue of the side paralysed; the chorda tympani also had lost its excitability by galvanism; on the sound side, however, all the usual phenomena took place on the application of this agent. Hence he concludes, that the chorda tympani nerve conduces to the general sensibility and gustative function of the anterior two thirds of the tongue, and that the integrity of this nerve is necessary to the complete efficiency of these two functions.—*Gazette Médicale*, 1850.

*On so-called Chylous Urine.* By H. BENGE JONES, M.D. A.M. F.R.S., &c.

THE definition given of chylous urine is, that it is urine which is white from the suspension of fatty matter in it. An opportunity of observing a case of this disease having occurred to the author, he was led to make the experiments described in this paper. A harness-maker, æt. 32, half-caste, who had lived in London for twelve years, had been passing such water for nine months. On examination of the water made at 2 p.m. it solidified, looking in ten minutes like blanc-mange. It was very feebly acid, contained fibrin, albumen, blood-globules, and fat; specific gravity = 1015. 1000 grs. of this urine gave—

44.42 grs. total solid residue.  
8.01 grs. total ash.  
14.03 grs. albumen.  
8.37 grs. fat.  
13.26 grs. urea and extractive matter.  
.75 grs. loss.  
955.58 grs. water.

In order to watch the variations produced by food and exercise in the appearance of the urine, every time the urine was made, for five days and nights, it was passed into bottles marked with the hour. From these observations, and more particularly from the third, fourth, and sixth days, it was evident that the fibrin and albumen appear in the urine when no fat is there, and that the albuminous urine occurs before food has been taken, and disappears during the night with perfect rest. Thus the fourth day, at 7h. 15m. A.M., on first getting up, the urine contained the slightest trace of albumen. The specific gravity = 1027; the precipitate by alcohol = 0.8 gr. per 1000 grs. urine.

At 9h. 50m. A.M., just *before* breakfast, the urine formed a solid coagulum, free from fatty matter, but contained a visible deposit of blood. Specific gravity = 1015.6; the precipitate by alcohol = 14.1 grs. per 1000 grs. of urine.

At 11 A.M., the urine was chylous or white from fatty matter.

Further experiments on the influence of rest and motion in lessening or increasing the albumen in the urine previous to food, are then given.

On five different mornings, by rising early or late, and by collecting the precipitate from the urine by alcohol, the influence of rest and motion was determined. The author states that he could fix beforehand whether the urine should be albuminous or not, by directing the patient to get up, or to lie still.

The patient was bled, and the serum was opalescent, but did not clear with æther: the blood contained no excess of fat. 1000 parts of blood gave—

2.63 grs. fibrin.  
159.3 grs. blood-globules.  
78.1 grs. solids of serum.  
240.03 grs. total residue.  
759.97 grs. water.

The urine made the same day was examined at different hours; that made immediately before the bleeding was quite white, and that made an hour and a half afterwards was very milky also. Specific gravity = 1018. 1000 grs. of urine gave—

56.87 grs. total residue.  
10.80 grs. total ash.  
13.95 grs. albumen.  
7.46 grs. fat.  
24.06 grs. urea, &c.  
.60 gr. loss.  
943.13 grs. water.

The conclusions from these experiments are,—

1. That so-called chylous urine, besides fat, may contain albumen, fibrin, and healthy blood-globules.

2. That, although the fat passes off in the urine after food is taken, yet the albumen, fibrin, and blood-globules are thrown out before any food has been taken. During perfect rest the albumen ceases to be excreted; and it does not appear in quantity in the urine even after food is taken, provided there is perfect rest. A short time after rising early the urine may coagulate spontaneously, although no fat is present; and this may happen previous to food, when the urine is free from fat.

3. Though the urine made just before and a short time after bleeding was as milky as it usually was at that hour of the day, yet the serum of the blood was not milky: it did not contain a larger quantity of fat than healthy blood does.

The general results are,—

1. That the most important changes in the urine in this disease take place independently of the influence of digestion.

2. That the urine in one respect only resembles chyle, and that is in containing, after digestion, a large quantity of fat in a very fine state of division. The supposition that the disease consists in an accumulation of fat in the blood, which is thrown out by the kidneys, carrying with it albumen, fibrin, blood-globules and salts, is altogether disproved, both by actual analyses of the blood, and by the frequent occurrence of a jelly-like coagulum in the urine when no white fatty matter can be seen to be present.

3. The disease consists in some change in the kidney by which fibrin, albumen, blood-globules and salts are allowed to pass out, whenever the circulation through the kidney is increased; and if at the same time fat is present in the blood, it escapes also into the urine. That this change of structure is not visible to the naked eye on *post-mortem* examination, Dr. Prout long since demonstrated; and in a case of this disease which was in St. George's Hospital, and was examined at Plymouth, no disease of the kidney was observed. From the total absence of fibrinous casts of the tubes from the urine, it is not improbable that by the microscope a difference may be detected in the structure of the mammary processes, rather than in that of the cortical part of the kidneys.—*Philosophical Transactions*, 1850.

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*On the Iodine of Fresh-Water Plants and Animals.* By M. CHATIN.

IN his second memoir, M. Chatin comes to the following conclusions, the results of very multiplied investigations.

1. The detection of iodine in the aquatic plants of the various parts of the world confirms the deduction derived from the examination of those of the vicinity of Paris, viz., the presence of this substance in the mass of the globe, and in most of its fresh waters. 2. The different conditions of the soil of former epochs of vegetation, in reference to the prevalence of water, may be deduced from the amount of iodine found in the fossil remains. 3. Fresh-water animals contain even a larger proportion of iodine, than do the plants of the same waters. 4. The presence of iodine in fresh-waters is proved by the examination made of those of about 300 rivers, fountains, and wells. From this it results (*a*), the water is rich in iodine in proportion as the soil through which it flows is ferruginous. (*b*). The proportion contained increases with that of iron, so that ferruginous waters may just as truly be termed iodined waters. (*c*). Waters of igneous soils are, upon an average, more iodined, and especially more uniformly so, than waters from soils of deposit; waters of soils containing green chalk and ferruginous oolithites holding the first rank among the latter, and may even be placed before those of igneous ones. (*d*). Though rich in iodine, the waters of the coal formation come after those of the igneous soils, or ferruginous deposits. (*e*). Waters of essentially calcareous and magnesian soils hold very little iodine. (*f*). Iodine is especially rare in the saliferous marls, the habitual seat of rock salt. (*g*). The iodides are by no means



necessarily proportionate to the chlorides. (*h*). Rivers fed by glaciers contain little iodine, especially at the period of the melting of the snow. (*i*). The waters of rivers are, as a general rule, more iodined, and more uniformly so, and less charged with earthy salts, than those of their sources. (*k*). The waters of wells are most calcareo-magnesian and least iodined. 5. The relation which exists between iodine and iron in the waters, the easy decomposition of the iodide of iron, and the complete decomposition of the iodide of the waters on evaporation without the addition of potass, render it probable that the iodine exists in the form of iodide of iron. 6. Iodine exists in terrestrial plants and animals. 7. The salts of potass, and the greater part of the salts of which they form the base are iodined; but nitre, cream of tartar, tartar emetic, and the double tartrate of soda and potass do not contain it. Ammoniacal and soda salts contain it, as also the salt of saline marshes. Rock salt and the *salines d'Est* are almost completely deprived of it. 8. Fermented liquors contain it, wine, cider, and perry in larger proportions than the average of fresh-water. The quantity in wines differs according to the richness in iodine of the soils upon which they were grown. 9. Milk, and especially that of the ass, is yet richer in iodine than wine. Apart from the influence of the soil, according to which it varies, the quantity is inverse to the abundance of the secretion. 10. Eggs (but not the shells) are highly iodined. A fowl's egg of 50 *grammes* contains more iodine than a *litre* of milk, or than two *litres* of wine, or Seine water. 11. Iodine exists in arable lands abounding in sulphur, iron, manganese, and sulphuret of mercury; it is rare in gypsum, calcareous and siliceous soils. 12. A too small proportion of iodine in the potable waters of certain countries seems a probable cause of *goitre*. The change of such water, or at least the use of wine, of ferro-iodated waters, watercress, and animal food, especially eggs, is rationally indicated. Marsh salt, too, should be substituted for the rock salt found usually in goitrous countries. 13. Most of the bodies regarded by therapeutists as pectoral and antiscrofulous, are rich in iodine.—*Jour. de Phar.* No. 18, p. 241, 1850.

*Observations on the Curare (Woorara) Poison.* By MM. BERNARD and PELOUZE.

THE preparation of this poison is kept secret by the Indians. According to Humboldt it is a simple infusion of a plant of the *Strychnia* family, and M. Houdet is of the same opinion, adding, however, that before the extract becomes quite dry, some drops of the venom of the most poisonous serpents are allowed to fall into it, and, in fact, its properties agree with those of venom, inasmuch as it can be swallowed with impunity, but inserted in a puncture in any part of the body it is always fatal. A weak watery solution of the extract thrown into the jugular of a dog or rabbit, kills it instantly, without cry or convulsion, life being extinguished with the rapidity of lightning. Introduced under the skin, it acts in a variable time, according to the dose, and the size and species of the animal. Birds die soonest, then mammals, and lastly reptiles; the last living hours, the others minutes. But in all, death is preceded by the same symptoms, which are remarkable. At first, after the prick, the animal manifests no symptom, the bird flying about, or the dog walking about just as usual, yet shortly, according to the activity of the poison, they expire without having uttered a cry, or given the least signs of suffering. If the body be examined immediately after death, we find phenomena indicative of the entire extinction of all the properties of the nervous system. In general, after sudden death, the nerves react for a while in the presence of mechanical or chemical stimuli. If we stimulate a motor nerve, we induce convulsion of the muscles which it supplies, and if we irritate the skin, we produce reflex movements. In an animal killed by *curare* but a minute since, the nerves are as inert as in an animal that has long been dead and cold. Lastly, in an animal so poisoned, the blood is found always black, does not redden on exposure to air, and coagulates with difficulty.

If these results are compared with the phenomena observed by Fontana in the blood and nervous system of animals killed by the venom of the viper, a close analogy is observed; and like this, the curare can be introduced into the stomach

with impunity. This does not arise from any modification its properties undergo by the agency of the gastric juice, for poison previously digested in this for twenty-four or forty-eight hours, proved just as rapidly virulent as ever; and gastric juice to which *curare* has been added, retains its digestive powers as actively as ever. *Curare* allowed to remain in a dog's stomach for some time, proved as active as ever on withdrawal. In like manner, the various secretions which make up the intestinal juices have been experimented upon with the same results.

The cause of this innocuity is simply that the gastro-intestinal mucous membrane will not absorb the poisonous principle of this substance, soluble as it is; and an experiment proves this to be the case. If the gastric mucous membrane of a recently killed animal be adapted to an endosmometer, so that the mucous surface looks outwardly, and the endosmometer containing sugared water is then placed in a watery solution of *curare*, endosmosis will have been found to have been effected in three or four hours, the level will have been attained in the tube, and yet the liquid it contains will exhibit no trace of *curare*, as may be ascertained by inoculating with it. If the experiment were allowed to go on for a much longer time, the endosmosis of the poison might occur, but we should then find that the mucous membrane had undergone modification, the mucus and epithelium covering it being altered, so that imbibition and endosmosis of the *curare* are possible; and if in place of taking a quite fresh mucous membrane, we take one that has undergone some change, the endosmosis of the toxical fluid occurs instantly.

As it was interesting to ascertain whether other mucous membranes possessed this resisting power, those of the bladder, nasal fossæ, and eyes were tried, and constantly with the same results. An injection was kept in the bladder without inconvenience, for from six to eight hours, by a dog; but the urine it passed after this time had all the toxical properties of *curare*. One mucous membrane is alone excepted from this immunity, the pulmonary, applied to which the poison comports itself in exactly the same manner as in the subcutaneous cellular tissue.—*L'Union Médicale*, 1850, No. 125.

#### *Absorption of Inorganic Poisons by Plants.*

CHEVALLIER has communicated several observations respecting the absorption of mineral substances by plants. Pepper-wort (*Lepidium sativum*) was planted in earth, and watered with an aqueous solution of tartar emetic, blue vitriol, or sugar of lead; antimony, copper, and lead were found respectively in the stalks of the plants, but only copper or lead in the seeds. He found lead in plants grown in a white-lead manufactory, and he likewise confirmed the observation that chloride of sodium is absorbed by plants.

The action of arsenious acid upon plants has been investigated by Chatin: he states that this acid is, to a certain extent, absorbed by plants, and that if they are not destroyed by the influence of the poison, it is at a later period again ejected by the roots. He has examined the conditions which favour either the action of the poison, or its secretion; we mention here only that the action of the poison on the various kind of plants exhibited a remarkable difference, phanerogamia dying earlier than the cryptogamia, and the dicotyledons sooner than the monocotyledons. Filhol has confirmed the statements of Chatin; and he has, moreover, minutely investigated the unequal distribution of the arsenic absorbed throughout the various parts of plants; he finds that arsenic acid, employed in the same proportion and in an equal state of dilution, has a more poisonous action on plants than is exhibited by arsenious acid.—*Liebig's Report*, vol. ii, p. 119.

#### *Remarks on the Cooking and Preserving of Meat.* By PROF. LIEBIG.

THE view, that broth derives its nourishing properties essentially from the dissolved gelatin—an opinion which has frequently been discountenanced in practice—is shown by this investigation to be completely untenable. The gelatine imparts no

taste to broth, and forms by far too insignificant a portion to allow of its nutritious properties being dependent upon it. Chopped beef, or veal, previously exhausted in the cold, when boiled for five hours, yielded to the broth, the former 0·5 per cent., and the latter 1·5 per cent. of soluble constituents, of which gelatine formed, at most, but one half. On the contrary, this investigation confirms the view of Prout, that the peculiar constituents of broth exist ready formed in the flesh, and are by no means merely products of the process of ebullition. The residue of the chopped muscular flesh of different animals—as of the fox and ox—after having been exhausted in the cold, cannot be distinguished the one from the other; all the peculiarities of the flesh, especially its flavour, depending entirely upon the soluble constituents which are found in the broth.

The researches of Liebig offer a simple and convenient method of preparing, in a few minutes, a broth of the highest nutritive properties. Finely-chopped lean beef is mixed with an equal weight of cold water, and left, if possible, to macerate for a short time, and the whole then slowly heated to ebullition. After gently boiling for some minutes, the clear broth separates from the coagulated albumen, and from the muscular fibre, which has now assumed a sinewy appearance. After straining, it requires only to be seasoned, and slightly coloured with burnt onions, or with caramel. The colouring of the broth is nothing but a concession to the common prejudice, which cannot, however, be well dispensed with. By evaporation in a water-bath, or at a still lower temperature, the broth becomes spontaneously coloured, and leaves behind a brown extract, possessing a delicate odour of roasted meat; this extract, when dissolved in about thirty parts of water, and flavoured with salt, yields, at any moment, a most excellent broth. The advantage of extract of flesh for the nutrition of invalids, its use in hospitals, or in field service, as well as in domestic economy, is sufficiently obvious. We see, likewise, that bone-broth, broth-tablets, &c. being preparations essentially different from a true broth from flesh, cannot enter into competition with it as articles of food.

As an article of commerce, extract of flesh bears somewhat too high a price. It appears, however, to offer a new source of profit to the inhabitants of the different settlements in America and Australia, who might successfully prepare it from their cattle at a cheaper rate, and send it to the markets of our crowded populations.

As to the cooking of meat, it follows, that to prepare, by boiling, a rich broth, and, at the same time, a savoury *bouilli*, is perfectly impossible. After preparing broth to the above directions, the meat which remains is perfectly unpalatable, tasteless, and tough, and as dissimilar as possible to the boiled beef of our tables. If, on the other hand, it be desirable to leave in the boiled meat the greatest amount of nutrition and flavour, it must be at once plunged into boiling water. If the temperature, after some minutes, be reduced to about 158° Fahr. by the addition of cold water, and the water maintained at that temperature until the meat is thoroughly cooked, all the conditions necessary for this purpose will have been fulfilled. If it be perfectly established that pure fleshy fibre—viewed independently of the juice—instead of being softened by boiling, is converted into a horny or sinewy mass, it is evident that this change is prevented by two different means in the ordinary mode of cooking meat: in the first place, by the temperature in the interior of the piece of meat never reaching the boiling heat; and, in the second place, by its being, nevertheless, sufficiently high to coagulate the albumen which surrounds, and, to a certain extent, protects the fibre. The temperature in the interior of the meat is not only sufficient to coagulate the albumen (132° Fahr.), but must attain even the point necessary for the coagulation of the colouring matter of the blood (from 149° to 158° Fahr.)

The investigation of Liebig exhibits the process of salting meat under a perfectly new aspect. The “brine,” which meat and dry salt form when together, amounts to from one third to one half of the juice of the meat, and contains the chief constituents of concentrated broth. The brine presents an acid reaction, and, owing to the quantity of albumen present, coagulates when boiled; it contains, moreover, phosphoric acid, lactic acid, a large amount of potassa, kreatinine, and, doubtlessly, also

kreatine. There can be no doubt, therefore, that salting diminishes the nutritious properties of meat, by the amount of constituents which pass into the brine; hence the explanation of the well-known injurious effect on health produced by the continued consumption of salt meat.—*Liebig's Report*, vol. ii, p. 338.

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## PATHOLOGY AND PRACTICE OF MEDICINE.

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### *On Coup de Soleil.* By Drs. PEPPER and CONDIE.

DR. PEPPER believes that very erroneous notions respecting the nature of this affection prevail. Twenty patients have been admitted on account of it into the Philadelphia Hospital in seven years, of whom 7 recovered, 10 died, and 3 continued to suffer from chronic disease of the brain,—all these persons having been bled prior to admission. The prominent symptoms are violent convulsions, with muscular tremors in the intervals; a small, thready, irregular pulse, and dilated pupil. He has of late had the opportunity of witnessing four autopsies (from six to eight hours after death), at which there was found no congestion, or other unusual appearance of the brain; but, in all, the heart was pallid, flaccid, and softened, though the other muscles were florid and firm. Its lining membrane (and that of the blood-vessels) was of a dark purple colour; its cavities contained little blood, and no coagulum. Similar appearances are described by Louis as occurring in typhoid fever that proves rapidly fatal. Apoplexy may occasionally, as it is too generally supposed to do, result from insolation; but the cases usually met with in hot summers bear no resemblance to it, and require stimuli instead of depletion. In all the fatal cases Dr. Pepper has met with, death occurred in six hours, and in the others recovery was slow, the mind continuing long in a confused state, and the case sometimes passing into one of insanity. The accident usually occurs to persons laboriously employed, while exposed to the sun's rays. Muscular exertion, and excessive heat, combine to stimulate the heart to morbid activity; copious perspiration and fatigue ensue; the organ becomes exhausted, and a passive congestion of the capillaries of the body occurs. It is a disease of nervous exhaustion, to be carefully distinguished from the cerebral congestion occasionally arising from the same cause. Of 5 cases treated by stimuli, 3 recovered, and 2 died.

Dr. Condie observed, that the cases thus described are of common occurrence during hot summers, in persons engaged in laborious occupations, while exposed to the sun, and especially in those of intemperate habits. There is, however, another class of cases due to the same cause, in which the head, and often the entire surface, is intensely hot, the eyes are injected, the pupils contracted, the pulse small, quick, and corded, and the tongue red and dry. Such patients are delirious, morose, or constantly agitated; and if the acute meningitis, upon which these symptoms depend, is not treated by depletion, they die comatose.—*Amer. Journ. of Medical Science*, vol. xxi, p. 183.

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### *On the Diseases from Lead in Paris.* By M. CHEVALLIER.

M. CHEVALLIER, advocating the substitution of oxide of zinc for white lead in the manufacture of paint, states, that although Paris contains only two white lead manufactories, the number of persons admitted into the hospitals from 1838 to 1847, suffering from diseases caused by lead, amounted to 3142. Of these, 1898 were operatives engaged in its manufacture: 712 painters, 63 colour-grinders, and 10 porcelain card-makers. Of the 3142 there were 3030 discharged, relieved, or cured, and 112 died, some of them with frightful rapidity. Among the 112, there were 86 lead-workers, 13 painters, 1 colour-grinder, 1 printer, 1 workman in painted papers, 1 lead-flatter, 1 porcelain card-maker, and 1 potter. Six were so ill that no account of their occupations could be obtained.—*Gaz. des Hôp.*, 1850, No. 121.

*On the Animal Temperature in Intermittent Fever.* By M. GIRBAL.

ACCORDING to De Haen, the temperature of a healthy man being taken at 95° or 96°, F., this is found to be from 87° to 94° in the cold stage of an ague, and from 96° to 108°, or even sometimes several degrees higher, in the hot stage. Gavarret and Monneret, however, found the temperature of the cold stage to be higher than the normal, the difference being 1° or 2°, C. Bouillaud also found the temperature rise 1° or 2°, C., in the cold stage, and increase 3° or 4°, C., in the hot. M. Girbal, during the winter of 1849-50, instituted a great number of careful observations, and found that very different results were obtainable according to the part examined, the temperature of the sexual organs being always higher than that of the axillæ. The trials made in the axilla gave, as the mean temperature of all the patients, while in a state of apyrexia, 36·2, C. It varied in the cold stage from 37·2 to 38·7; in the transition from the cold to the hot stage, from 38·6 to 39·7. In the hot stage it varied from 38·6 to 41·2, giving a mean of 39·9. That of the sweating stage, which, however, was absent in many patients, was 38·5.—*Schmidt's Jahrbücher*, vol. lxxviii, p. 313.

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*On the Treatment of Lead Colic by the Internal and Topical Employment of Chloroform.* By M. ARAN.

ALTHOUGH M. Aran's paper is only based upon eight cases, the benefit he derived from chloroform in them was so marked, as to lead him to at once publish the results. He gave spoonful-doses of a mixture containing 40 parts of chloroform in 130 of water and syrup, and administered an enema containing a small portion. Upon a compress previously moistened with water and slightly squeezed, from 4 to 8 grammes of chloroform (according to the intensity and extent of surface occupied by the pain) were poured and kept applied for from 15 to 30 minutes by means of the hand to the abdomen. On the next day and day after, the application was renewed, but it was very rarely required the third time, and the other means were continued until spontaneous and natural stools became established. Alkaline and sulphureous baths were used every other day, in order to remove any lead that might adhere to the surface. In five of the eight cases, the pains at once disappeared, never to return, after the local application, the patient being quite cured of the disease in from two to six days. In the others the pain did return, in a mitigated form, requiring a new application. In only two cases relief from the internal use of the chloroform was obvious, while in some the clysters produced positive irritation. It is upon the local use of the substance, therefore, that the author lays the greatest stress. Of the three cases in which it was less successful than in the five, he gives particulars. In two of these the primary relief was just as rapid, but the patients leaving the hospital, relapses were at once induced; on returning and again being promptly relieved, however, and remaining a few days until the alvine evacuations and appetite returned, they too were effectually cured. In the third case, in which *liq. subac plumb.* had been drank in mistake, the pain too was promptly relieved, but castor-oil was required in addition to remove obstinate constipation.—*Bull. de Thérap.*, tom. xxxix, p. 296.

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*On Nervous Affections.* By M. CERISE.

WE know nothing of the intimate and essential point of departure of these, as they do not betray themselves to a material or anatomical appreciation. Their *diagnosis* is especially a *negative* one, by which we are able to assure the patient that no organic lesion is the cause of his suffering; and in presence of most serious and alarming symptoms, it is a great deal to be able to declare they are *nervous*. We may truly be reproached with the expression as a cloak for our ignorance; but yet it is fortunate for the practitioner, even when not aware of the real and positive value of the word, to know that it nevertheless signifies the indubitable absence of irreparable and fatal disease, and suggests, if not the choice of determinate thera-



peutical measures, at least the rejection of those of a mischievous and barbarous character.

The neuroses constitute a family or group of diseases having nothing in common with others, and characterised, therefore, by certain signs facilitating their diagnosis. The first of these is their aptitude of transforming themselves into each other, not only in the same individual during life, but hereditarily in the same family. This power of *individual and hereditary transformation* is the most important of all signs. Here we have a common type and numerous varieties, with a facility of perpetuation within this type of the varied forms of it. This power of transformation implies another character of the neuroses, viz., great *mobility*, and yet an intimate connection—forms constantly changing, symptoms substituted for each other, and the most variable disturbances, being yet all enchained together with remarkable uniformity. Hysteria, which is the type of nervous affections, offers frequent illustrations of this.

A second characteristic may be designated as *independence of the phenomena of nutrition*. The physiologist truly teaches us that the nervous system presides over all the operations of the organism. But in the neuroses the action of the nervous system on the operations of organic life sets at defiance all the most legitimate affirmations of science. Patients may be the objects of the most terrible pains, with convulsion, delirium, cramps, dyspnœa, and vomiting, and this for weeks, months, or years, without the circulation undergoing any modification, digestion becoming disturbed, or assimilation being interrupted. Nay, we find people keeping their beds for months, the prey to the most poignant moral and physical sufferings, unable to take or to support the slightest aliment, and yet preserving all through the cruel vicissitudes of their disease their bulk and even freshness of countenance.

After stating these two general characteristics by which neuroses may be distinguished from other pathological groups, a few words may be added as to the means of distinguishing them from each other. In nosological systems, from the time of Sauvages to Cullen, they possessed no collective name and determinate rank, but were disseminated under different names, some in one class of diseases, others in another, according to the symptoms most especially observed. From the time of Cullen to our own days, the neuroses have been distinguished as a group, constituting a class bearing a collective name. As they can only be studied and known by their symptomatic manifestations, they can only be classed according to the most constant and most evident of these; and the attempts of the anatomo-pathologists to arrange them according to the “seats” of the disease,—the same affection, as hysteria, epilepsy, tetanus, hypochondriasis, originating often in quite different apparatuses,—have always proved failures. By confining our attention for the purposes of classification to the predominant symptoms, observing and comparing these with care, we act far more wisely than by following a hypothetical localisation. We thus do not at every turn raise pathogenic questions, about which every one differs, or risk seeing our nosological classifications floating between the revolutions and counter-revolutions of theory, and dependent upon the changeable mind of opinion; but endeavour to bring prominently forward those differential characters, concerning which all observers of all schools and all times should be in accordance. It does not follow from this, that in the study of each disease the practitioner is to content himself with this symptomatic view. On the contrary, in referring the disease to the proper cause that has produced it, and to the organ in which it has originated, pathogenic theory will supply indications when empiricism is dumb.

To proceed with the classification of the neuroses, there may be, in the first place, a *general neuropathy, characterised by the absence of all predominant symptoms*; but in which there may successively appear, in the most sudden manner, and almost uninterruptedly, the most various and even the most opposite symptoms. This general neurosis or proteiform neuropathy varies in its degrees of intensity. In its moderate degree we have those occult disturbances of impressionability and innervation, necessarily attached to the nervous and melancholic temperaments. In a more marked form it constitutes that permanent, yet vague and indeterminate con-

dition, known as the hysteric condition, occurring in men as well as women. In its extreme form there is the scarcely interrupted series of nervous and painful disturbances, called by practitioners, from the difficulty of finding appropriate names, hysteria in the female, and hypochondriasis in the male.

The *neuroses with a predominant symptom* may be divided into three general groups. The *first* is characterised by special *disturbance of organic life*, in some appertaining especially to the *ganglionic* nervous system, and in others to the *pneumogastric*; and characterised by the predominance of some visceral disturbance in the chest, abdomen, or pelvis, as in pertussis, asthma, palpitation, syncope, vomiting, tympanitis, gastralgia, &c. The *second* group comprehends the neuroses characterised by *disturbances of sensation and locomotion*, especially developing themselves in various parts of the sensory and motor nervous system. The *third* group is characterised by *disturbances of the understanding, the psycho-cerebral neuroses*, or the different forms of insanity formerly termed *vesaniae*. — *L'Union Médicale*, 1850, Nos. 80, 83, and 87.

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*Case of Gangrene of the Vermiform Appendix.* By Dr. STORER.

THIS occurred in a boy æt. 12, who died of peritonitis after a week's illness. The vermiform appendix was found in a state of gangrene, its cavity containing an oval mass of hardened fæces, disposed in layers, through which ran several pieces of hair. No perforation had occurred, but a general state of peritonitis was present. — *Amer. Journ. Med. Sc.*, vol. xxi, p. 51.

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*Erysipelas after Vaccination.*

AMONG the "Extracts from the records of the Boston Society for Medical Improvement," is an account of several cases of erysipelas after vaccination, which some of the members have met with. Dr. Jackson relates two cases. One occurred in a child aged six months. The erysipelas appeared near the vesicle on the seventh day, and extended thence over the whole of the arm and part of the trunk, producing slight vesication, an abscess in the axilla, and severe constitutional irritation. It recurred, too, in parts whence it had subsided, and that even twice; but the child recovered in a month. The other was that of an old man of bad habits, who had been re-vaccinated, the erysipelas confining itself to the extremity. A case, related by Dr. Cabot, was an example of diffuse inflammation, and occurred in a gentleman, aged sixty-nine, who had been re-vaccinated. The inflammation occupied the whole limb and part of the trunk, and required deep incisions for the liberation of matter; a most extensive burrowing suppuration being set up, detaching the pectoral muscles from the chest, and the skin of the whole arm from the subjacent parts. The case lasted seven or eight weeks in its acute form, but without excessive prostration; and considerable benefit was obtained from the external application of *tr. iodin.*, and its injection, when diluted, into the suppurating cavities. Two cases are related by Dr. Bigelow: a gentleman, aged thirty, five days after revaccination had erysipelas of the entire arm, which extended over the trunk; he suffered for about a month from intense fever and delirium, until a slough, which had formed at the inner part of the arm, separated, leaving a deep ulcer, after which he became gradually convalescent. The virus with which he was vaccinated produced no ill effects upon others. In the other case, a child of five months was the subject; the erysipelas coming on on the ninth day spreading rapidly over the whole trunk, and causing death in a few days. All vaccinated from this child on the eighth day had normal vesicles. Dr. Homans relates the case of a child, aged three weeks, in whom the erysipelas appeared on the tenth day, and rapidly spread over the body, inducing deep sloughing of the scrotum, and abscesses in various parts of the integument. After an illness of three months, it was now convalescent. All who were vaccinated from it on the eighth day did well. Dr. Homans alludes also to the cases of two lads in whom the erysipelas was followed by extensive axillary abscess

one of them having been vaccinated, and the other revaccinated. Dr. Putnam relates a case in which the scalp became erysipelatous on the fourth day after vaccination, the infant recovering in a week, and no other part becoming affected. A fatal case occurred, after revaccination, in the person of a patient of the late Dr. Greene, aged sixty-six. Death occurred within eight days, erysipelas having extended to nearly the sternum. In the other cases, vaccinated with the same lymph, no unusual symptoms occurred.—*Ann. Jour. Med. So.*, N.S., No. 40, p. 318.

These cases are instructive, and should teach caution in vaccinating, and especially in revaccinating, during the presence of erysipelas; as at such a period it is no more surprising that the vaccine areola should prove the point of departure of an attack, than that cutaneous inflammation excited in any other way should be. The reporters are careful to note that other children, vaccinated with the same lymph, manifested no disease, and although this may seem superfluous in addressing the profession, it would be by no means so with regard to the public, amongst whom an opinion of the communicability of diseases, by means of vaccine lymph, is yet prevalent. Reputed cases, too, of this are from time to time recorded in the continental journals, and we may notice one we have recently met with. Dr. Wegeler of Koblenz, states that variola being extensively prevalent in a town in 1849, many revaccinations were performed. A surgeon revaccinated, with the same lymph, twenty-six individuals, and in nineteen of these (from eleven to forty years of age) vesicles appeared. But in three or four weeks they exhibited all the signs of *venereal ulcerations*, and were, in most of the individuals, sometime after followed by sore throat, eruptions, and pains in the head—no particulars being given,—so that by four physicians consulted, mercury was deemed necessary for a cure. The child whence the lymph had been obtained had been vaccinated Feb. 4., and as the vesicle was very slow in progress, the surgeon did not take lymph from it until the 14th and 15th, when it was, consequently, eleven or twelve days old; other children, however, were then vaccinated with it without ill effect. Of the twenty-six revaccinated, much the largest number fell ill in whom the lymph of the 15th had been used. Some time after the lymph had been taken from the child, it exhibited copper-coloured blotches, and at a later period died of supposed water on the brain. The vaccinator, for having employed lymph thus old, was sentenced to two months' imprisonment and a fine of fifty thalers,—leaving him still open to civil action on the part of the persons aggrieved!—*Med. Zeitung*, 1850, No. xvi.

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*On Progressive Muscular Atrophy.* By M. ARAN.

UNDER this title M. Aran believes he is depicting a form of fatty transformation of muscle not hitherto described. The peculiarity that distinguishes this form of atrophy is, that in place of affecting an entire limb or segment of limb, it affects certain muscles only, leaving others untouched; so that when it extends over a whole limb, or tends to become general by influencing other parts of the body, some muscles always remain exempt from its influence, even in those limbs most subjected to this. When it has once appeared in a portion of the muscular system of a limb, it has a constant tendency to extend to other portions, and to involve the muscles of the corresponding limb, or in some cases to invade, with the exception of the exempted muscles, the entire muscular system.

Sir C. Bell relates in his appendix two cases of *partial atrophy*, which he refers to a local affection of the nerves, and similar ones are related by Abercrombie and Cooke; but the most exact description of the affection he has in view that Dr. Aran has been able to meet with, is contained in a paper by Dr. Darwall, in the 7th vol. of the 'Medical Gazette,' in which it is designated as a partial and painless paralysis of the upper extremities, induced by raising heavy weights. In the present essay Dr. Aran is enabled to describe, which he does very minutely, from personal observation, the particulars of eleven cases, that are separable into two groups, the one consisting of examples of *partial atrophy*, in which it is limited to one or two limbs, or a portion of a limb, the other of examples of *general atrophy*,

in which a great portion of the muscular system is involved, though exactly speaking the disease is never general. From these the following description of the disease is derived :

*Symptoms.*—Several circumstances confer on this form of atrophy a special character. A debility is first observed in a single limb, or rather part of a limb, so as to confine it to the execution of certain movements, gradually, however, involving a larger portion of it, and generally extending to the opposite one after a time. Cold and fatigue augment it, while cramps and subsultus often accompany it. Emaciation of an irregular character follows this, affecting certain muscles only instead of the mass of the limb, and therefore giving rise to various deformities, and finishing with the destruction of the muscular structure and complete annihilation of function.

In nine out of the eleven cases it commenced in the *upper* extremity, the right side suffering first in seven, the left in two, and in two both being simultaneously affected. Sometimes the affection first appeared in the muscles of the shoulders or those of the upper part of the trunk, at others in the fleshy masses of the arm or fore-arm, but most commonly in those of the hand, or the little muscles of the thenar and hypothenar eminences, and the interosseous spaces. Side by side with the atrophied muscles others are found unaffected, even their congeners which to some degree supply their places. Moreover, in the case of some of the larger muscles, easily separable into bundles of fibres, some of these may remain unaffected. The muscles of the upper extremity, which the author has hitherto always found exempted, are the triceps in the upper arm, and some of those of the anterior part of the fore-arm, and especially the pronator teres and flexor carpi radialis. Intelligent patients have observed the wasting almost simultaneously with the weakness of the part; and if we find a slight wasting of the thenar or hypothenar eminences, or a too-marked depression between the interosseous spaces, combined with weakness of the hand, and unexplained by any other cause, we may suspect the existence of this disease, our convictions gaining in certainty as more muscles become after a time involved. Normally, the muscles offer a certain amount of *resistance* and *elasticity* on pressure, and in all *other* atrophies, from whatever cause, these qualities, though *diminished*, are not *lost*. Here they are absent, for not only is the muscle reduced in volume, but its tissue is probably replaced by cellular-adipose tissue, giving to the touch a sense of extreme softness (varying, however, with the degree of the disease, and the propinquity to osseous tissue), while the will cannot impress the slightest tension upon it.

*Fatty transformation* in all the cases is at present only highly probable, for its reality has as yet been shown in but one case, the only one in which an autopsy has taken place.

In an early stage of the disease obstinate cramps, and in a later one subsultus and *fibrillary contractions* may be present. These last may consist of the isolated and involuntary contraction of certain of the muscular fibres, being sometimes so numerous and continuous that the muscle seems in motion, while at others they are so rare as to require close observation for their detection. Voluntary contraction does not suspend them, and they may also appear in other parts not suffering from the disease, as the tongue.

The general economy remains unaffected amidst this slow and progressive muscular destruction; no general symptoms being present, however far the disease may be advanced, and every function being, to all appearance, perfectly performed, except that of voluntary motion.

*Duration and Progress.*—Although a very long interval may elapse between the first symptoms of weakness and the production of complete muscular degeneration, it is always an invading disease; and once having taken possession of the economy it never retrogrades; the utmost that can be done being to arrest its course, and that by no means with any certainty. The duration is indefinite, and most of the author's cases came under his notice months or years after their commencement.

*Prognosis.*—Even in its simplest forms, attacking, as it does, persons of laborious

occupations in the prime and vigour of life, it is a terrible disease; and where it involves muscles necessary for important functions, as respiration, it may cause death. Even as an infirmity, in its partial form, there is none more beyond our control.

*Diagnosis.*—This affection must be distinguished from the paralyses with which it has been confounded. In *paralysis*, if complete, motion is abolished; if incomplete, it is imperfect. In *atrophy*, so long as sufficient fibre remains to raise the levers, motion is performed, though feebly; abolition or incompleteness being thus the character of the one case, weakness of the other. Moreover there is no symptom of disease of the brain or spinal marrow present, and atrophy resulting from paralysis involves the entire limb. The investigations of M. Duchenne show that atrophy from paralysis very rarely gives rise to complete destruction of muscular power. Hysterical paralysis does not lead to atrophy, and the loss of motion is complete. Rheumatic paralysis is not always complete, but it affects the entire muscles of the region. Paralysis from lesion of a nerve is the form which most rapidly gives rise to atrophy; but this is exclusively confined to the muscles supplied by such nerve. M. Duchenne has shown that electrical irritability is intact in hysterical and rheumatic paralysis, even when atrophy is present. In saturnine paralysis, and paralysis from lesion of a nerve, it is entirely lost; while in progressive muscular atrophy, though enfeebled, it exists until the muscular fibre has entirely disappeared. The disease which offers most analogy to it is *progressive paralysis, independent of insanity*, in which electricity develops only very feeble contractions, even when the muscles retain their normal size and elasticity. The history of the two affections serves to distinguish them.

*Nature and Causes.*—It is primarily and exclusively a disease of the muscular system; but its etiology is completely obscure. Of the eleven cases, nine were men and two women—the mean age being 36. The occupations were various (as country labourers, stone-cutters, shoemakers), some requiring great muscular exertion, and most of the patients complaining of excess of work; so that the author is disposed to consider the too-continuous and excessive employment of the affected limb as a chief occasional cause; although when we consider the rarity of the disease amongst the hundreds of thousands so situated, this is very problematical.

*Treatment*, both general and local, has been most assiduously employed, never with the effect of causing the disease to retrograde, and seldom even of arresting it. Galvanism, localised in its action upon the muscles, seems to be the best palliative.—*Archives Générales*, tom. 24, pp. 1-24, and 172-214.

*Observations on Ozone.* By MM. POLLI, HEIDENREICH, and FABER.

DR. POLLI forms his ozonometer by dipping papers in the following solution: starch, 10 parts; iodide of potassium, 20; water, 400. Provided they are kept folded up, or in closed vessels, they preserve their power for months.

When one of these slips was suspended by a thread in the air, outside a window of the house, it became strongly coloured in a few hours, while a similar one, suspended within-doors, remained white for days, and only began to be coloured after several,—and this was the case, in whatever part of the house it was suspended, as in well-ventilated passages and corridors. The slip exposed out of doors became still more speedily and deeply coloured when freely exposed at a distance from the house. To ascertain how far a frequent renewal of the body of air might influence the appearance, one of two slips, placed out of doors, was fastened firmly at both ends, and the other allowed to fly about freely. Both became rapidly coloured, and with equal intensity. A portion of a slip was introduced within a phial, and a portion allowed to remain externally, the air, however, having access to the former. On exposure to the atmosphere, the portion external to the phial became intensely coloured, while that within remained unchanged, so that mere vicinity of another body prevents action. If ozone irritates the air-passages, we can see, from the above experiments, the importance of invalids suffering from a delicate state of



them, keeping the house, or protecting them when they quit it. So, too, some light is thrown upon the injurious operation of drafts and currents of air, by the fact that strips, suspended near cracks and fissures within-doors, only become coloured opposite these.

If slips of paper are covered before exposure, with layers of silk, wool, linen, and cotton, of the same size and thickness, those covered by the silk, wool, and linen, remain uncoloured, while those covered by the cotton become coloured. If a piece of linen and a piece of cotton are immersed in a solution of starch and iodide of potassium, and then exposed, the cotton becomes deeply coloured, while the linen becomes so, only feebly, after a long time. Will this aid in explaining the irritating effect of cotton handkerchiefs, compared to linen, when applied to the nose and eyes during catarrh? Humidity does not impede the appearance of ozone. The direct rays of the sun favour it, and it is less developed if these are shaded by dark-coloured glass, and during the night. When snow fell, Dr. Polli observed its action to be very powerful, as also during the prevalence of northern winds, and rainy and cloudy days. When fine weather had lasted many days, the air became loaded with ozone, but immediately after heavy showers it disappeared; slips which before the rain appeared, were coloured in a few hours, then remaining white for days. After the rains have ceased the ozone reappears, and continues increasing. That the rain-water, and especially the first drops, contain it abundantly, is seen by the deep colour of the slips exposed to its aspersion. In the stalls of stables, in which the air is moist, and ammoniacal, the slips do not change colour, or do so very slowly as compared with those placed in empty stables. Will this explain any benefit phthisical patients were once supposed to derive from breathing such air?

Dr. Heidenreich made a series of daily observations upon the relation of the amount of ozone to the nature of the prevailing diseases, from March 16 to May 22. He found that a strong ozonic reaction coincided with an exacerbation of the symptoms of catarrh, and the appearance of pulmonary phlegmasiæ, while a diminution of these took place when it was feeble. On the other hand, affections of serous membranes, as the arachnoid, and of the synovial, as also various cutaneous affections, as urticaria, varioloid, zoster, appeared during very feeble signs of the presence of ozone. Rheumatic affections seemed connected rather with a large than a small amount, while pleuritis was as often met with in the one case as the other.

Dr. Faber (in common with Wunderlich and other German observers) doubts the existence of any connection between the development of ozone and the prevalence of catarrhal affections. A year's observation, during 1848, failed to confirm the accuracy of Schönbein's conclusions upon this point. He found, too, the colour both strong and weak, whether the barometer was low or high, but perhaps oftener weak when it was high, and strong when it was low. During 1848, however, the barometer proved a very deceitful prognosticator of the weather.—*Omedei Annali*, vol. cxxxiv, p. 155; *Canstatt, Jahr*. 1850, vol. ii, p. 60; *Gaz. Médicale*, 1850, No. 51, p. 905.

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*On the Treatment of Sciatica by Galvanism and the Caутery.*

By MM. DUCHENNE and VALLEIX.

THE practice recently put into force by M. Malgaigne of cauterising the ear in sciatica, in imitation of the Corsican blacksmiths, has found little favour with the more scientific practitioners at Paris, and the two distinguished observers, whose names we have placed above, are loud in condemnation of it as a far less efficient means than we already possess. Each of them, however, advocates a different procedure.

M. Duchenne believes that the cures of sciatica which are effected by cauterising the helix, are so by reason of the perturbatory effect of this process; and that sudden and severe pain excited in any part of the economy, by means which do not disorganise the tissues and endanger erysipelas, is equally efficacious. In proof of this he cites the great success which has during five years attended his *localised gal-*

*vanization of the skin*, the sensation produced by which may be graduated from a mere tickling to the most intense pain. He is limiting his remarks to cases of purely dynamic disturbance of the nerve, excluding sciatica dependent upon inflammation or upon material lesion, as tumours, &c. There are few of these pure sciaticas which are not immediately influenced by the galvanization, upon whatever part of the body it is practised; but as the excitement of sudden and severe pain is necessary, in individuals of slight sensibility galvanization within the nares may be requisite. He effects galvanization of the skin by means of bundles of metallic wires brought into connection with an inductive apparatus, and applied transcurrently along the course of the nerve, or left for a while in contact with the skin, so as to produce a severe effect. The skin must be first dried by an absorbent powder, for if there is any moisture on the surface the stimulus reaches the affected nerve itself, which, according to M. Duchenne's observations, always exasperates the disease, whether thus effected, or by the agency of electro-puncture. The patient can rarely bear the galvanization longer than a few seconds, and on its cessation he is astounded to find his sciatica gone. It is very rare, however, for a permanent cure to be produced at a single sitting; and at different periods, from one to twelve hours, the pain returns, but mitigated and changed in situation; sleep, which had been so long absent, returning also, and progression being rendered easier; and if the application be renewed for six or eight times, at short intervals, as once or twice a day, the cure becomes definitive. These results have been obtained in cases that have long resisted all other means, and which are those in fact that have been usually selected; but it is not meant to be asserted that there are not some which resist even this procedure.

M. Valleix, while equally deprecating the practice of cauterizing the helix, which he says no one would dare propose to a patient in private practice with the knowledge that only a small proportion of patients are temporarily relieved by it, does not agree with M. Duchenne, that success arises in cases in which it occurs, from the perturbation that is produced by sudden and severe pain, whether this be caused by the rough process of cauterization of the ear, M. Duchenne's more elegant procedure, or his own plan of transcurrent cauterization.

If this production of sudden pain be necessary, how does it happen that a cure may so frequently be obtained by the application of small blisters along the track of the nerve, or when pain is entirely absent, as when the transcurrent cauterization is applied while the patient is under the influence of chloroform, as is the case with M. Valleix's patients, or when the parts are previously rendered insensible by the application of a mixture of ice and salt, as practised by M. Nelaton, in which case no pain whatever is produced, and the patient is almost immediately able to pursue his affairs as usual. When, too, we apply blisters, we destroy the neuralgia piecemeal. Applied to the thigh, the neuralgia is driven to the leg, followed here by a blister, it retreats to the ankle, and when this is also blistered it disappears entirely. So in the transcurrent cauterization, this partial disappearance of the pain from the cauterized spots, and its persistence in those which have not been cauterized, or only insufficiently so, is a thing of daily experience. This cannot arise from any perturbatory effect, like that induced by cauterizing distant parts, but from a modification of the sensibility of the affected parts, by which means they are restored to their normal condition. Thus very often the amelioration is not observable the day or the next day even after the cauterization, but two, three, or four days after, when the tissues have undergone all the influence of the blisters, or of the cautery, so much more efficient than blisters. Prior to the discovery of etherization, notwithstanding its great efficacy in the hands of M. Jobert, the author had rarely resorted to this means, in consequence of the terror it caused the patient; but since he has found that the insensibility of the patient is no obstacle to its success, he has resorted to it in all cases that have resisted other means, and has found that only the small proportion of cases dependent on organic lesion have resisted its influence, the patient in from six to eight days losing all traces of his suffering in a great number of cases. In others, amelioration to a certain degree only takes place, and

for it to become confirmed and extended, a reapplication of the means, at intervals of from five to ten days, may be required, even to the sixth or seventh time; but sooner or later they *all yield to it*. Still, although it causes no pain in its performance, and little inconvenience afterwards, both practitioners and patients seem averse to reap the benefit it holds out.—*L'Union Méd.*, 1850, Nos. 121, 128, 140, 141.

## SURGERY.

*On the Treatment of Ophthalmia in General.* By Professor LANGENBECK.

PROFESSOR LANGENBECK, of Erlangen, lays down some general principles for the treatment of inflammation of the eyes, which may be often usefully borne in mind.

1. A slight *revulsion on the intestinal canal or skin* suffices for the cure of mild inflammations of the eyelids and conjunctiva, especially in children, provided that neither the inflamed part nor the patient's constitution have undergone any material change. When the inflammation is consequent on the irritation of foreign or chemical bodies, or is sympathetic of a disordered state of the general economy, active exercise in the air for some hours, an antiphlogistic regimen, and care in using the organ, are required.

2. When the inflammation is more active, but the constitution still sound, a *more active revulsion* is required, and may be procured by strong purges and enemata, and the frequent application of large blisters or sinapisms to the calves, thighs, sacrum, or nucha. Friction of the feet with *Spt. sinapismi æthereus*, and then covering them up, soon induces revulsion. In blenorrhœal scrofulous ophthalmia, &c., these means are still of use, though not alone curative.

3. The *irritation of the nasal mucous membrane* is an excellent means when the disease is not removed in a few days, and is inclined to become chronic or relapsing, and especially if it assumes a distinct catarrhal character. A pinch of Spanish snuff may be taken every two hours, a little black pepper applied, or the infusion of chamomiles inhaled. This last, accompanied by a blister to the neck, soon disperses very obstinate cases, occurring in persons predisposed to angina.

(We may observe that M. Tavignot is a strong advocate for exciting revulsive action on the Schneiderian membrane, in the subacute stage of scrofulous, and some other forms of ophthalmia. To this end he either touches the mucous membrane daily with a pencil of nitrate of silver, or with a little ointment containing 1-10th of this substance. But to these means he prefers, when the patient is old enough to know how to use them, stimulating substances mixed up with iris powder, and taken as snuffs. Thus he uses a powder formed of 30 parts of iris powder, camphor 1 part, and sulph. zinc or copper from 2 to 8 parts. Or 2 parts of nitr. silver, or 1 of cantharides may be substituted for these salts, retaining the camphor. (See *L'Union Médicale*, No. lxxix.)

4. *Special excitement of the functions of the skin.*—Suppose the disease takes on the form of scleritis or rheumatic ophthalmia, with great irritability of the eye, and disposition to relapse on slight atmospheric changes. In such cases, besides the local treatment to be adverted to, great benefit accrues from exciting the functions of the skin by *tartar emetic vomitings*, repeated three or four times, continuing the medicine in smaller doses for a while afterwards, and keeping the patient in as warm a medium as possible. In other cases great benefit results from administering the *Sp. mindereri* in bland fluids, and keeping the body hot for twenty-four or forty-eight hours, frequently the while applying friction to it with a flesh-brush, or passing a hot domestic iron over it when covered with a blanket.

5. *Derivation to the glands.*—In scrofulous ophthalmia, which is very frequent in the author's neighbourhood, he finds great advantage from exciting inflammation or even suppuration by repeated blisters, or other revulsives in glands liable easily to become inflamed.

6. *Derivation by issues.*—This is especially indicated in those forms of ophthalmia, in which opacity of the vitreous humour is a common result, as hyaloiditis, keratitis, iritis, uveitis, and periphakitis. In such, into an issue opened in front of the ear or on the temple, a little powder of equal parts of salt and borax may be placed several times a day, and forms a powerful adjuvant to antiphlogistics.

7. *Revulsion on the joints* is a powerful adjuvant in persons who have already suffered from gout or rheumatism, or who by age or constitution seem especially predisposed to them, the joints being covered by flying blisters, or strong sinapisms. Dr. Langenbeck has frequently had recourse to this means prior to operations for cataract or artificial pupil, in order to prevent subsequent inflammations in such subjects.

8. *Bleeding*, whether local or general, is usually useless in superficial inflammations of the eye, unless they acquire great intensity, and threaten to implicate the deeper structures, when small general bleedings or leeches are necessary. In children even, bleeding from one to three oz. is usually better than leeches, which when used should not be applied to the cheeks, temples, or mastoid processes, where they may even do mischief, but at a distance, and especially along the course of the carotids.

9. *Local application of cold.*—Upon this subject Dr. Langenbeck offers some very minute directions, not only in respect to the cases to be chosen for its use, but its mode of application. He observes, that in many cases wherein cold is useful, wet is mischievous, and in others where this is not the case, it becomes so through faulty management. If merely permanent cold is required, he employs hollow horn rings, into which pieces of ice wrapped up in rag are introduced, which are replaced, when melted, by other pieces. When applied, the apparatus looks like a monster pair of spectacles, and is large enough to be supported on the orbit without compressing the globe of the eye at all. A piece of sponge is laid on the cheek to receive the fluid as it melts. Another plan of producing great cold, to which he gives a preference, is to place the patient on his back, with his eyes shut, and having deposited a small portion of a powder, formed of equal parts of nitre and sal ammoniac, in the inner angle of the eye, to allow water to fall on it *guttatim* until it is dissolved; the fluid so formed may be retained on the eye until heated, and then renewed. The author attributes the production of the good results which follow this plan, not only to the intense cold which is produced, but to the antiphlogistic effect of the salts which gain admission into the eye, on the partial opening of the eyelid. Repetition of this from four to eight times, at intervals of from ten to twenty minutes, is equivalent to glacial applications for twenty-four or forty-eight hours; and, indeed, the two, if necessary, may be combined. Persons who have no assistance, may manage the powder by inclosing a certain quantity in linen rags, and moistening it. So too glass globules may be filled with ice or this refrigerant.

As a general rule, *the local use of cold is proper in all cases which are not dependent upon evident constitutional cause or dyscrasia.* Certain exceptions to this must, however, be observed; as—1. When inflammation of the fibrous textures of the eye has extended to expansions of the motor apparatus of the globe. Recent *rheumatic* ophthalmia is, however, benefited by cold affusions, lasting from three to ten minutes, and chronic cases by dry cold. 2. In *erysipelatous* inflammations, cold affusion is interdicted. The temporary application of dry cold, and the covering up the forehead and cheek with taffetas, induces active transpiration. 3. *Blennorrhæal* inflammations are not dangerous to the internal structure of the eye in general, only inasmuch as the cornea becomes injured by the irritating discharges; and it is chiefly for the purpose of cleansing these from it, that washes are resorted to. In *acute ophthalmia of infants*, the author opens the eye every ten minutes and inserts a piece of ice within, or a few drops of a concentrated solution of alum and tannin; and however long the disease may last, no injury will accrue to the cornea if the discharge is removed as fast as formed by cold water. 4. Chronic ophthalmias, and especially in the aged, are far more favourably influenced by the use of cold from time to time than by its continuous use, the dilated vessels and exudations being thus more

advantageously modified; and exudations are sometimes much influenced by sudden douches propelled with force, even if composed of warm water. These form, too, one of the best stimulants in nervous affections of the eye.

A second general rule is, that in the treatment of all ophthalmias, dependent on a constitutional cause, but not on a dyscrasia or cachæmia (as hæmorrhoidal, menstrual, abdominal ophthalmias), the local use of cold, applied as energetically as possible, is the best means. If severe the *douche* may be continued for six or eight hours. It is especially when there is a varicose state of the vessels of the eye left, or that *engorgement* of the choroid accompanies conjunctivitis, that these douches are so useful. So in atonic injection of the conjunctiva, especially the palpebral, the *douche*, repeated several times daily, is far more useful than any astringent injection.

A third rule is that in ophthalmias, seated on a tissue in a state of dyscrasia from evident alteration of the blood, whether it is a mere local symptom of such altered condition, or whether it has been induced by external agency, dry permanent cold, not moist cold, which would favour softening of the cornea, is indicated.—*Annales d'Oculistique*, xxiv, 100-17.

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*On Subconjunctival Dislocation of the Crystalline Lens.* By M. BARRIER.

M. BARRIER relates two cases of this singular occurrence, both produced by the same description of violence—the thrust of the horn of an ox. In the first case, (a man, æt. 58,) application was made a fortnight after the accident, on account of the continuance of the blindness, which had suddenly supervened. The cornea was found normal, but the iris had disappeared, and the transparent media were obscured by the effusion of blood. Several ecchymosed spots appeared under the conjunctiva, and on cutting into a small tumefaction, situated between the cornea and caruncle, the crystalline of normal consistence, but opaque, escaped.—The other case occurred to a woman, æt. 47, who applied eight months after the occurrence, having, as in the other case, suddenly lost her sight, and having, as in that instance, suffered little from inflammatory action. The cornea was transparent, but all behind it was confused. A little tumour presenting itself under the conjunctiva at the outer angle of the eye, quite unsurrounded by signs of inflammation, it was cut down upon. The conjunctiva was thick and dense, forming a cyst for the lens, which did not turn out when this was opened, but required the assistance of a cataract needle. It might have been expected, after so long a sojourn, that the lens would have excited eliminatory action. It had undergone some softening, and probably would have ultimately become absorbed.—*Annales d'Oculistique*, t. xxiv, p. 84.

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*On Acupuncture in Varix.* By Professor RIZZOLI.

THE author believing that the occasional violent inflammation produced by Frick's operation on varix, which consists in passing in a thread and leaving it in the vein, arises from the irritation produced by the tissue of the thread itself, and judging that a smooth metallic body would cause much less, seeing how easily such sometimes traverses the organism, resolved to employ the acupuncture needles alone. He passed these through the principal veins leading to the varix, and left them *in situ* for two or three days, covering the projecting parts with cork; and on removing them found the veins to have become indurated and obstructed. Besides cases of varicocele, he has operated also for the relief of old varicose ulcers. The thigh was bandaged so as to render the trunk of the saphena turgid, and one needle was passed through the vein near the knee, and another at an inch distant. They excited but little inflammation, and were not removed until the sixth day, when considerable occlusion of the vein had taken place; and it was interesting to observe the ulcer which had resisted all treatment, now rapidly yield. As in some instances, in consequence of the venous anastomoses, the varices do not become obstructed, though the principal vein may be so, this suggested to the author not only to act



on the principal vein and its main divisions, but also on the varix itself. In this way a mild degree of inflammation is set up in the varices, inducing their obstruction. Some time after the relief of the varix, the fibrin which had been deposited during the induced inflammation is absorbed, the hard and obturated varicose tumours are dispersed, and the circulation is restored, without, except in very rare cases, the varix being reproduced. *Post-mortem* examination of the condition of veins thus treated in three persons who had died (one three, another six months, and the other five years) after, but not in consequence of, the operation, showed that they had assumed the thickness of arterial tunics, and thus acquired strength enough to enable them to bear the weight of the column of blood without yielding.—*Annali Omedei*, vol. cxxxii, p. 645.

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### MIDWIFERY, &c.

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*On the Induction of Premature Labour.* By Dr. LEHMANN, of Amsterdam.

AFTER passing in review the various means of effecting this, Dr. Lehmann gives the preference to that practised by the Dutch accoucheur, Zuydhoek, viz., detachment of the membranes by means of a wax bougie. A bougie, nine inches long, and two or three lines in diameter, is passed within the uterus, carried for six or eight inches along its anterior wall, and then at once withdrawn. This detaches the membranes, and *directly* excites the motor nerves of the uterus; while the prepared sponge, usually employed for this purpose, merely acts upon the sensitive nerves of the cervix, and affects the uterus itself only by a reflex action. By remaining within the organ for so long a period, too, the sponge may induce inflammation, which the bougie does not; and from this cause Dr. Lehmann lost two patients in whom he resorted to the sponge.—*Rev. Méd. Chir.*, tom. viii, p. 366.

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*On Oophoritis.* By Dr. PISTOCCHI.

AFTER narrating several cases of inflammation of the ovary, Dr. Pistocchi states:

1st. In reference to *Semeiotics*, that, although in the opinion of many, no pathognomic and differential signs are presented by this affection, he believes the following may be considered as such: 1. Single or double lateral pain, accordingly as the disease complicates one or both ovaries, spreads along the hips and side, especially on movement. 2. Metrorrhœa is frequent without a proportionate uterine idiopathic affection. 3. The ovarian functions, as regards concupiscence and fecundity, undergo disturbance. 4. There is a lateral *consensus* of parts more immediately brought into connection with the ovary, as the breasts, and of the hypochondrial viscera, the kidneys, and possibly of all parts of the same side. Three cases are referred to, in one of which amblyopia, and in the other two sciatica, occurred on the same side. 5. The especial liability of the left ovary. 6. The patient, while suffering from disease, is liable to a variety of anomalous and violent anæsthetic and convulsive affections of the nervous system. General febrile action too, is active, and frequently intermittent.

2d. With regard to *etiology*, the author believes that the affection is very dependent upon innate original peculiarities,—the nervous temperament remarkably prevailing in the subjects of it. Concupiscence prevails in some such individuals to a degree leading to onanism, and even in the absence of actual disease, they are usually sterile. In such individuals any exciting cause which would be inoperative in other persons, may induce oophoritis. He considers onanism, venereal disease, and re-percussed gonorrhœa as especially likely to induce the affection, though in the predisposed it may occur independently of any of these.

3d. *Therapeutics.* Treatment of a depletory and contra-stimulant character is required to be energetically put in force. Few acute diseases tolerate and require

such active depletion; pound after pound of blood being abstracted with nothing but advantage. As soon as the more acute symptoms are thus got under, nothing so much aids the cure and prevents relapse as the employment of *cicuta*. The disease being, however, more frequently chronic than acute in its character, may proceed even to the organic destruction of the ovary, without its presence being detected; the most extensive changes in an organ not necessary to life occurring, without inducing general reaction. Dr. Pistocchi believes that several of the convulsive affections of women, treated as idiopathic nervous affections, are really dependent upon, or at all events intimately connected with, the dynamo-organic affections of the ovary.—*Bulletino delle Sc. Med.*, vol. xvii, pp. 1—81.

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*On the Application of Collodion to Inflamed Breasts.* By Professor EVANS.

DISHEARTENED by want of success in the employment of the means usually recommended for the prevention of suppuration of the breast during lactation, and believing equable compression to be the procedure rationally indicated, Professor Evans has, in several cases, had recourse to the practice of completely coating the organ with a layer of collodion; and in this paper he reports that the relief from pain is prompt and striking, while the formation of matter, which seemed imminent, is prevented.—*New York Journ. of Med.*, N. S., vol. v, p. 431.

[Upon the same principle we have for some time past employed, in cases where the "dispersion" of the milk from the breasts has been desirable, thick pledgets of cotton-wool, applied with moderate compression.]

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*Case of Early Viability of an Infant.*

M. BIERBAUM, in an article upon this subject, relates the following case that occurred to his preceptor, Outrepont. A newly married woman bore a child twenty-seven weeks after her last menstruation, and five weeks after perception of foetal movements, *i. e.*, between the 175th and 189th day, it was  $13\frac{1}{2}$  inches long, and  $1\frac{1}{2}$  lb. in weight; covered with woolly hair; slept almost uninterruptedly; seldom opened its eyes; did not cry, but moaned when its position was changed. The pupillary membrane was present, the knees were pressed close against the chest, and the thighs drawn against the belly. In place of nails there were firm white folds of skin. Fifteen weeks after birth it had scarcely grown  $1\frac{1}{4}$  inch, and in its eleventh year was not larger than a child of seven or eight; but in other respects, both mentally and bodily, was as it then should be.—*Med. Zeitung*, 1850, No. 44.

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*Case of Quintuple Birth of Living Children.* By Dr. SERLO.

DR. SERLO of Krossen relates the following remarkable case. The mother, æt. 34, had had five favorable labours, and was now pregnant for the sixth time. During the last few weeks, she had become so large and cumbersome as to be obliged to keep her bed. Dr. Serlo saw her the day before delivery, and found her abdomen enormously distended in every direction, and hard, and projecting much towards the right. The foetal movements were feeble. She was weak, and had a small, rapid pulse, with œdema of the thighs and legs. On examination the os was found partly open, and the membranes flaccid; but no part of the child could be felt. As the pains proved very inefficient, Dr. Serlo next day delivered her by the forceps of a small living child, and soon after of another, which presented by the feet. In like manner three others were successively delivered by the feet, the accoucheur breaking the bag of waters in each which presented while he was in search for the placenta. Contraction of the uterus was produced after some minutes.

All the children were alive and crying, but the 2d died in three hours, the 4th in twelve, the 3d in seventeen, the 5th in twenty-five hours; and the 1st, which had been delivered by the forceps, in nine days. The author supplies the weights and

admeasurements of the children and the funes ; but we are not aware of the exact relation which those of that part of Germany bear to our own.

	Length.		Weight.	
	Child.	Funis.	Child.	Placenta.
1st child.....	15 inches.....	24 inches.....	3½ civil pounds.....	28 oz.
2d „ .....	12½ „ .....	11 „ .....	2¼ „ .....	14 „
3d „ .....	13 „ .....	15 „ .....	3½ „ .....	25 „
4th „ .....	14 „ .....	14 „ .....	3 „ .....	20 „
5th „ .....	14 „ .....	11 „ .....	3 „ .....	20 „

Med. Zeit., 1850, No. 50.

Abnormal Relation of the Clitoris and Urethra.

A WOMAN, æt. 45, mother of several children, presented the following appearances. A fold of mucous membrane afforded the only sign of the existence of the nymphæ. Immediately beneath the commissure of the vulva was the smooth triangular space of the *vestibulum*, with the *meatus urinarius* situated two lines above its lower margin, and ⅔-inch above the projecting clitoris, which dipped into the superior angle of the vagina. A catheter could be passed vertically for about an inch, when a slight incurvation led directly into the bladder. The clitoris was reverted so as completely to conceal the anterior edge of the upper wall of the vagina, and its glans was grooved antero-posteriorly by a deep sulcus.—*Amer. Journ. Med. Sc.*, vol. xxi, p. 261.

MATERIA MEDICA AND THERAPEUTICS.

Volatile Oils. Quantities yielded by Plants.

VAN HEES has determined, by experiment, that the following quantities of oil were yielded by the undermentioned plants. The light oils were obtained by passing steam through the plants ; and the heavy ones by surrounding the still with high-pressure steam.

			sp. gr.
<i>Oleum anisi</i> , 20lbs.	gave	5½ ozs. of	0·977
<i>Ol. anisi stellati</i> , 20lbs.	„	8 „	0 976
<i>Ol. calam. arom.</i> (old oil)	„	0 „	0·984
„ 55lbs., calamus of previous year	„	12 „	0·956
„ 85lbs., new calamus, somewhat dried	„	10 „	0·950
<i>Ol. carui</i> . 12½lbs. of the last year's seeds	„	8 „	0·923
„ 25lbs. fresh seeds from the Eifel	„	17 „	0·913
„ 14lbs. Saxony seeds	„	6 „	0·926
<i>Ol. caryophyll.</i> 10lbs. Amboyna cloves, by 6 distillations	„	31 „	1·040
„ 8lbs. Bourbon	„	21 „	1·035
„ 25lbs. Dutch cloves, by 8 distillations	„	74 „	1·033
„ 25lbs. clove stems	„	16 „	1·049
<i>Ol. cass. cinnam.</i> 14lbs. bruised cinnamon	„	2¼ „	1·035
<i>Ol. cass. flor.</i> 12½lbs.	„	3½ „	1·023
<i>Ol. fœni</i> . 10lbs.	„	5 „	0·968
<i>Ol. junip. bacc.</i> 44lbs. dried ripe berries	„	2¼ „	0·870
„ 96lbs. fresh „	„	7½ „	0·862
„ 53lbs. unripe „	„	3 „	0·864
<i>Ol. lavendulæ</i> , 14lbs. dried flowers	„	2 „	0·892

Liebig and Kopp's Report, vol. ii, p. 33.

*On the Destruction of Leeches by the Oniscus aquaticus.* By M. LEON SOUBERAIN.

IN this paper M. L. Souberain relates the history of a small leech-pond which his father had constructed in April, near the Central Hospital Pharmacy; in order to observe the conditions most favorable to the reproduction of the animal. The bottom of the pond was covered with clay, in which were planted abundance of aquatic herbs, and, at one part, an isle of soft earth and turf was placed, in which the leeches could bury themselves. Three hundred fine Hungarian leeches were put in and left there till the end of September, being fed in the interval only thrice, twice with blood, and once with frogs. In giving them the blood, a small quantity was put in the water, and the clot then laid upon a small floating plank. The leeches attracted by the smell, swam to the plank from all parts, and never left the coagulum until all was devoured, save a small piece of colourless fibrin. The frogs thrown to them succumbed in a very short time, exhausted by the suction, the leeches adhering to their eyelids, sometimes to the number of five or six, whence no struggles of the animals could detach them. A toad thrown in by chance, was pursued with equal avidity, but his thick skin and disagreeable smell always succeeded in saving him from his enemies. Although during the whole period the water was not changed, it remained, thanks to the plants, quite clear and limpid.

On removing the leeches, the adults and some young ones were found in fine condition, but with whatever care the search was made, not a cocoon could be found. Yet the leeches were fitted for reproduction, for among the leaves of the typha and iris, about 100 young ones, a centimetre in length, were found.

But, together with the old leeches, were found in the pond great numbers of a small crustaceous animal, having its extremities armed with hooks (*Ascellus vulgaris*, Geoff., *Oniscus aquaticus* L.), and which was found walking at the bottom of the pond and among the stalks of the plants, and especially within the folds of the leaves where the young leeches were concealed. Placed in a bottle with some young leeches, they attacked these ferociously, fixing upon them with such tenacity, that the leeches could never rid themselves of them, and eventually died. They fixed just as pertinaciously on old leeches also, but after vainly struggling against them these seemed to bear their attacks with impunity.

It is by the abundant presence of this animal in the Seine waters and the marshes of the environs, that the author believes the constant failures of the Paris leech-merchants to reproduce this valuable annelide, may very probably be explained.—*L'Union Médicale*, 1850, No. 137.

*Iodine in Hospital Gangrene.*

M. NELATON has of late employed the tincture of iodine with excellent results in hospital gangrene. The effect is very prompt, the granulations putting on a good appearance by the third day, when they may be dressed with chloride of sodium. It causes less pain than lemon-juice, which is the usual application in the French hospitals.—*Gaz. des Hôp.* 1850, No. 86.

*Professor Mettauer's Aperient Solution.*

Professor METTAUER, in an article on constipation, speaks in the highest terms of praise of the following "Aperient Solution:"—*R Aloes Soc. ʒijss; Sod. Supercarb. ʒvj; Aquæ Oiv: Sp. Lavand. C. ʒij.* After digesting for fourteen days, the clear liquor may be either decanted or allowed to remain. Age improves both the power and taste of the solution. "I claim for myself," the professor says, "the sole credit of originality in the invention of this compound,"—a modest claim enough too, which we hope posterity will ratify. The aperient acts both on the liver and the muciparous glands; corrects and prevents acidity; and probably aids assimilation when oily articles are used as food. It should be given about half an hour after dinner and supper, the common dose being ʒj, though in some cases this may be increased even to ʒj. A single dose is sometimes sufficient, and then

should be given at bed-time, diluting the solution with water, if desired. It is suitable to almost every example of constipation complicated with defective biliary secretion, except when the state of the stomach is alkalescent; and may be used also in the constipation of pregnancy. It has been found useful in constipation with hæmorrhoidal tumours about the verge of the anus, and especially so in that which accompanies all varieties of perverted menstruation; so much so, that its inventor regards it as a valuable emmenagogue wherever constipation is present, while fever and inflammation are absent. In constipation connected with long-continued derangement of the biliary system, or what is sometimes called bilious dyspepsia, it is the most useful of aperients, with which nitro-muriatic acid in doses of a few drops before meals (the aperient being given after meals) may often usefully be associated. It has often been employed with great benefit in the constipation of hysteria and hypochondriasis, a "nervine incitant," as castor or valerian, being added to it in such cases. In the constipation of the convalescent stage of fever, when the patients have begun to take solid food, it is of great value in preventing relapse, by regulating the functions of the bowels and promoting digestion. It requires in these cases to be cautiously given in small doses. For students of sedentary habits, "it is the very best remedy in the world for obviating the constipation so troublesome and health-destroying with them at that important and interesting period of their lives. If used regularly, their bowels will seldom or never become constipated; and the use of the remedy is not attended with inconvenience nor trouble, and never interferes in the smallest degree with study or diet." (In spite of the author's somewhat extravagant laudation, we do not doubt this is an excellent aperient, and have therefore transcribed the formula.)—*Amer. Journal Med. Sc.*, N. S., No. 40, p. 380.

#### Substitutes for Quinine.

THE present high price of quinine, and the threatened extinction of the supply of cinchona, have led to the publication in the French Journals of various propositions for substitutes. Among these *arsenic* deservedly enjoys most favour, especially since the publication of M. Boudin's papers upon its employment, his favorable report being confirmed by many practitioners, both civil and military, though demurred to by others. Valuable a medicine as it is, however, we fear that as a general rule it is very inferior to quinine as a febrifuge in certainty and rapidity of action, and the less liability to relapse said to be consequent upon its employment is anything but proved. A medicine recently introduced by Dr. Baud, under the name of *hydroferrocyanate of potassa and urea*, has excited considerable attention. The Academy appointed a commission to investigate its claims, and 30 cases of ague were treated by it, most of them being old Algerian colonists, who had had recourse already to various other means without success. Of these 26 were cured, confirming M. Baud's favorable statement, founded on 200 cases that had been treated by himself and others. The salt being very soluble is given in the form of pills encased in sugar, its bitter taste being thus better concealed, its dose more easily adjusted, and its alteration by light and moisture prevented. It is innocuous even in very large doses, merely producing a little temporary narcotism. When called to an ordinary case during the paroxysm, M. Baud prescribes from 10 to 20 three-grain pills, five to be taken each half hour; and if he sees the patient during the apyrexia, he orders from five to ten an hour before the paroxysm, and the same during its first hour, not giving any on the intercalary days unless the fever is pernicious. The entire treatment of a case requires upon the average three or four repetitions of the ten pills; but much larger quantities may be required in very severe and urgent cases. M. Ossian Henry, Director of the Laboratory at the Academy, has assisted M. Baud in the production of this substance on a large scale, but its exact composition has not been made known. From a complex organic product like this, the transition is great to so simple a one as *common salt*; and yet, according to M. Piorry, given in doses of from four to eight drachms *per*



*diem*, it effects very rapidly what no other *succedaneum* of quinine that he has tried does, a diminution of the size of the spleen. Recommended to the Academy by a provincial practitioner, M. Piorry promptly cured six out of eight cases in which he employed it. Another provincial practitioner of high repute, M. Gendron, has published an account of the great efficacy of an indigenous solanaceous plant, the *alkekenge* (*Physalis alkekenge*), found among the vines and shady places in France, Spain, and Italy. Of forty cases, it failed only in five or six. The dried ripe capsule and seeds seem especially efficacious, containing an intense bitter. As much as four drachms twice a day may be prescribed, but in other cases one drachm four times a day suffices. Dr. Chevreuse also states he has derived great benefit from the juice of the leaves of the *Plantago major*. It seems almost trenching on the ludicrous to repeat that two practitioners, residing one at Naples and the other in Sardinia, are quoted in the 'Révue Médicale' as recommending, as based upon sufficient experience, *spider's web*, forty grains being given in divided doses. We quoted in a recent number (No. xi, p. 276) some observations upon the febrifuge power of *chloroform*.

Other practitioners recommend a more economical employment of quinine. Dr. Pfeufer (see *Periscope*, No. X, p. 534) speaks highly of the efficacy of a single ten-grain dose; and we find Dr. Trask, of the United States, where quinine is often administered in quantities that seem to us enormous, and with excellent effects too, in the pernicious forms of the southern latitudes, relating of the intermittent fever he meets with in Westchester county, New York, that he almost always treats it by giving ten grains of the powder, four or five hours before the expected paroxysm;—"One dose has almost uniformly effected a cure." He has also often employed *ferroprussiate of iron* with advantage, though it is not comparable to quinine.

Dr. Ruspini states also that great economy would result from the substitution of a neutral sulphate for the present bibasic salt; for he and other practitioners have found such neutral sulphate or persulphate as useful as the bibasic in a half or a quarter the dose—a fact easily understood in consequence of its great solubility. Computing with Chevallier the annual consumption of yellow cinchona in France at 140,000 kilogrammes, valued at 3,360,000 francs, the substitution of the neutral salt would reduce the quantity to 55,580 k., and the price to 1,333,920 francs.

Although quinine still holds its vantage ground, the importance of these investigations as to the discovery of possible substitutes, and the greater economising of present supplies, are impressed upon us by the unfavorable report of M. Weddel, after five years' investigation of the sources of supply, the high price which places the article already beyond the reach of the poorer classes, and the rapidly increasing adulteration it is subjected to.—See *Gaz. Méd.*, 1850, Nos. 38, 42, and 45; *L'Union Médicale*, 1850, Nos. 81, 89, 90, 110; *Amer. Journal*, No. 40, pp. 346; *Annali Omedei*, vol. 131, p. 267; *Bulletin de Thérapeutique*, t. 39, p. 374.—*Rév. Méd. Chir.* vol. viii, p. 257.

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## FORENSIC MEDICINE AND TOXICOLOGY.

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### *Fatal Case of Poisoning by Bromine.* By Dr. SNELL.

HALF an hour before the author was called to this man, he had taken, fasting, an ounce bottle of bromine. He was complaining now of incessant burning pain, his pulse being somewhat frequent and small, and his respiration hastened and thoracic. In two hours and a half after, prostration commenced, and copious secretion flowed from the nose and mouth. No thirst or vomiting, but increase of pain. Deglutition more and more difficult, and presently impossible. The prostration after awhile became excessive, and in four hours the pulse could hardly be perceived. He died seven hours and a half after taking the bromine. There were no symptoms referable to the head throughout. Emetics were given, and the stomach-pump attempted to be used, but the pain and inflammation of the fauces and

œsophagus prevented it. Magnesia, white of egg, and *aqua ammonia* were also administered.

At the *post-mortem* examination, sixteen hours after death, the peritoneum in the vicinity of the stomach and liver and omentum were found tinged of a reddish yellow, and injected. On the exterior of the stomach were several ecchymosed spots, one of which, two inches in diameter, was softened in the centre. The whole internal surface was covered with a thick black layer, resembling tanned leather, which readily peeled off. "The mucous membrane was softened with submucous injection; the lower portion of the stomach was smooth, hard, and tanned," as were also the *valvulae conniventes* of the duodenum, the mucous membrane between them being softened and injected.—*New York Journal*, N.S. vol. v. p. 182.

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*On a Case of Fatal Hæmorrhage from the Female External Genital Organs.*  
By Dr. EVRARD.

CASES are not unfrequently brought before the courts of justice in this country, in which a fatal hæmorrhage has resulted from the female external organs being wounded by cutting instruments; but such hæmorrhage has less commonly occurred from a contused wound. Indeed even this instance can hardly be called one; for although the wound resulted from the kick with the boot of a brutal husband, yet this was armed externally with enormous projecting nails. The woman, previously in good health, walked a short distance after receiving the injury, bleeding all the way, and was dead when assistance arrived. The body was found quite pale and exsanguineous, the heart and large vessels being very empty, and no effusion into any internal organ discoverable. A large lacerated wound in the labium was discovered, the finger freely passing to the horizontal ramus of the pubes in one direction, and towards the cavity of the pelvis externally to the vagina in another.—*Annales d'Hygiène*, tom. xliv, p. 425.

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*On the Duration of Life among the Clergy.* By Dr. SCHNEIDER.

IN this paper Dr. Schneider furnishes an account of the ages of 794 of the clergy who have died within the bishoprick of Würzburg since 1824. They belonged to the Catholic religion, and may be compared with the 637, almost all Protestants, recorded by Casper in his work. In the diocese of Würzburg, including priests, professors, and students, the mean annual number of this profession amounts to 1050; and between 1824-47, there have died 800. The ages of 794 persons are indicated, and are thus compared with Casper's numbers:

Ages.	Schneider.	Casper.
From 21 to 30 . . .	45 . . .	21
„ 30 „ 40 . . .	66 . . .	33
„ 40 „ 50 . . .	57 . . .	39
„ 50 „ 60 . . .	102 . . .	95
„ 60 „ 70 . . .	188 . . .	191
„ 70 „ 80 . . .	217 . . .	188
„ 80 „ 90 . . .	111 . . .	62
„ 90 „ 97 . . .	8 . . .	8
	<hr/> 794	<hr/> 637

Casper, speaking of the high age which the priestly order attains, refers in explanation to the regularity and sobriety of their lives, the absence of excess of mental or bodily stimulus and efforts, freedom from anxiety, and the wholesome alternation of moderate mental activity with corporeal exertion in the open air. Most of these conditions apply likewise to the Catholic priesthood, and their

*celibacy* constitutes their only peculiarity; but this, contrary to what is observed in other positions of life, does not shorten their lives, inasmuch as so large a proportion as 217 out of 794 lived to between 70 and 80. On the contrary, they live longer than the Protestant clergy; for while, according to Casper, the mean duration of life with them is 65, and therefore higher than any other class, that of the Catholics of Würzburg is 70½. The Protestant clergy have, owing to their families, more care and anxieties, without better incomes in proportion.—*Casper's Wochenschrift*, 1850, Nos. 8 and 9.

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#### *Poisoning by Peach Blossoms.*

M. BARIOD relates the case of an insane patient at the Maréville Asylum, æt. thirty-nine, who, having a voracious appetite, while working in the garden, consumed a large quantity of peach blossoms, and in about twenty minutes was seized with convulsions. The body became cold, the face pale, the features deformed, the pupils large, and the pulse irregular and hardly perceptible. The patient expired under an attack of tetanic convulsions in three or four hours.

At the *post-mortem* examination there was found abundant injection of the scalp, membranes, and brain; the lungs were gorged, and the auricles filled with blood. Red patches were also observed on the mucous membrane of the stomach and intestines.—*Annal. Med. Psych.*, N.S., vol. ii, p. 720.

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#### *On Poisoning by Decoction of Rose-bay (Nerium oleander).* By Dr. LANGLOIS.

THE experiments of Orfila have shown that an aqueous extract of this substance, introduced into the cellular tissue or stomach of dogs, proves a very active poison. In more southern countries mere emanations of the plant have induced indisposition. In 1810 several soldiers died near Madrid, after eating of meat which had been cooked on stalks of the plant—a common and innocuous practice, however, among the French soldiers in Algeria; and Orfila relates that some sheep died who had drank of the water in which some of the leaves had been macerated.

Dr. Langlois, of Metz, has recently met with a case of fatal poisoning in the human subject, occurring in the person of a prisoner for debt, æt. thirty-six, of weakly frame. A few hours before his death he had drank a glass of the decoction of the leaves, and expired in great agony. The quantity of watery extract this contained was supposed, from calculation, to have been about five or six grammes. The only remarkable *post-mortem* appearances were a vivid redness of the mucous membrane of the stomach, and some dilatation of the pupil.—*Gaz. des Hôpitaux*, 1850, No. 103.

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#### *Cases of Poisoning by Corrosive Sublimate.*

Two or three interesting cases of this are recorded in the 'American Journal of Med. Sc.' One, related by Dr. Coale, occurred in a man who took gr. x instead of calomel, much of which was at once rejected by vomiting. White of eggs, and afterwards anodynes, were administered. After the cessation of vomiting, violent and persistent hiccough occurred for two or three days, and an eruption of variola appeared. The vomiting also returned, and continued, with little intermission, until death, which occurred on the eleventh day after taking the poison, and the ninth after the first appearance of the variola. In this case there was entire suppression of urine; the tongue was clean, the skin cool; no fever was present, and the *post-mortem* examination revealed no appreciable lesion.

In a case reported by Dr. Chaplin, in which a teaspoonful of sublimate with laudanum is represented to have been taken by a woman aged 25, considerable

vomiting, purging, and epigastric pain resulted. The urgency of the symptoms, however, gradually abated, and hopes of saving her were entertained, when she became restless on the eleventh day, passed pus by stool, and died, with symptoms of prostration, on the thirteenth. No autopsy.

Dr. Ware also referred to a case in which dysentery came on after the primary symptoms had disappeared, and the patient died about the fifteenth day.

In a case related by Dr. Williams, a man had taken, about half an hour before being seen, a solution, containing about xxx grs. of the sublimate; free vomiting and severe abdominal pain ensuing, accompanied by chilliness, prostration, and the passage of abundant dark fluid stools. Delirium tremens, from which he, being of intemperate habits, had once before suffered, soon complicated the case, and he died on the second day. At the autopsy the morbid appearances, except subarachnoid effusion, and a shrivelled state of the spleen, were slight. The stomach was contracted for about two inches at its middle, giving it a dumb-bell appearance; but although the gastro-intestinal mucous membrane presented some crimson-dotted injection and arborescent vascular patches, there was no ulceration. The bladder was contracted.—*Amer. Journ. Med. Sc.*, vol. xxi, pp. 47, 79.

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*On the Solidification of Foot-prints.*    By M. HUGOULIN.

IN cases in which it may be deemed necessary to preserve foot-prints in a dry state for judicial examination, M. Hugoulin recommends the following procedure: an iron plate, supported by bars, is so placed as to be about three or four *centimetres* above the mark to be solidified, and upon it live coals are placed so that heat may radiate to the subjacent soil. When this has become heated to about 100°C, *stearic acid*, reduced to impalpable powder, (by previously dissolving it in its weight of alcohol, and then, having added abundance of water, evaporating,) is to be dusted over it through a fine hair sieve, so as to form an uniform layer. This, falling as a snowy dust, cannot by its weight injure the impression, and is indeed dissolved and absorbed as soon as it touches the soil. It is continued until the earth has become too cold to dissolve any more. After a sufficient time has been allowed for it to have become completely cold, the earth is mined around at some distance so as to raise the entire mass in a single piece, cutting afterwards carefully away the superfluous soil. After reversing it on several folds of linen, and surrounding it with a case, plaster is allowed to run in between the case and the reverse of the impression, so as to ensure complete solidity to it. If the soil is muddy or marshy, before commencing these operations, a trench should be dug around, into which plaster is to be introduced, which, on solidifying, will absorb much of the moisture, after which the whole mass may be exposed for some days to the sun and air before commencing to act upon it. M. Hugoulin says this plan is applicable to the most shifting soils, as sand, &c., and indeed to all except snow.—*Annales d'Hygiène*, t. 44, p. 429.

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INTELLIGENCE.

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ON THE LAST YELLOW FEVER EPIDEMIC OF BRITISH GUIANA.

IN our review of Dr. Blair's work on this subject, contained in our last volume (p. 418), we pointed out the insufficiency of Dr. Blair's disproof of the statements of Dr. Fraser in regard to its contagious origin, and called upon Dr. Blair to make public the additional evidence of which he has stated himself to be in possession. To this call he has responded, in the following letter to Dr. Davy, which that gentleman has forwarded to us, with a request for its publication. We give insertion to it, not because we feel that we have done any injustice to Dr. Blair or to his editor; but because we think, that everything which has reference to the origin of

epidemics has peculiar interest at the present time, and deserves the most careful examination.

(To Dr. JOHN DAVY, F.R.S., Inspector General of Army Hospitals, &c.)

George Town, Demerara; Dec. 11, 1850.

"MY DEAR SIR,—I have not seen Dr. M'William's notes on the Boa Vista fever, but I have read the article in the 'British and Foreign Medico-Chirurgical Review.' I find by it, as well as by a notice of the monograph in 'The Lancet,' that the subject of contagion, as regards our late epidemic, was not handled with sufficient detail. The fact is, that the idea of contagion in yellow fever had become here so totally obsolete, that I felt reluctant to do more on that subject than to make a declaratory statement of the universal opinion. When through you I was informed that a commission of military medical officers was sitting in London to investigate and decide on the subject of the alleged contagion of yellow fever, I did not suppose it possible that there would be the slightest difficulty in coming to a speedy conclusion in the negative. I find that I have mistaken the professional opinion at home, and that it will be necessary to supply some additional matter on the subject. For this purpose I enclose copies of some papers, which, with the contents of this letter, be pleased to use in any way you think best; at the same time I must observe, that the fact of the opinion of contagion in yellow fever here being abandoned by the unanimous consent of the entire community, professional and non-professional, who had countless opportunities of observing the disease spread over an eight years' epidemic, in all its phases, and from every point of view, seems to me testimony and proof as strong as the subject is susceptible of. There is certainly a moral certainty of the correctness of the conclusion so formed. This community is fully alive to the doctrine of contagion in disease. In fact, many diseases are reported "catching" here, which are not considered so elsewhere. A short time since I incurred considerable obloquy by recommending the governor to remove the Lazaretto for Lepers from the interior of Essequibo to the grounds of the Colonial Hospital, for the sake of superior superintendence. So they have not formed the opinion of the non-contagion of our yellow fever from any ignorance of the existence of such origin and mode of propagation of disease. And I submit, that a universal conviction, founded on extensive experience, is very superior proof to that of any collection of facts or process of reasoning made by any individual after the lapse of some time from the date of the events, and without a searching cross-examination on the spot, and in which so much of the aspect of the whole question depends on the ingenuity of the advocate, and the omission, or suppression, or transposition, of some facts. It might have occurred to the reviewer, in his notice of the opinion of the Medical Society here, that the capability of arriving at a true and correct conclusion does not imply the power of communicating the mode by which the conclusion was obtained. Dr. Abercrombie, in his 'Treatise on the Intellectual Powers,' refers to a judge who was advised never to state his reasons, for they would be sure to be wrong, while in all probability his decision would be right. That rapid reasoning called *tact*, which the reviewer praises and recommends, is very frequently inexplicable to its most gifted possessor. And I imagine that a lover of truth in science would have been more impressed with the fact of the unanimous and perfectly disinterested conviction of our Medical Society, than by any deficiency in their manner of communicating it. The reviewer requires to know the number of practitioners in George Town, or in the colony, who were exposed to the *quasi* contagion of the epidemic, and who escaped. It is difficult to give the entire number, but the 'Local Guide' for 1843 contains the names of sixty-five, only two of whom suffered. The number of course fluctuated during the eight years of the epidemic, some dying or leaving the colony, and being replaced by others; and sixty-five, therefore, represent the number of only one year who were exposed, while three represent the number who suffered during the whole period. In the same authority, the 'Local Guide' for 1843, the names of the directors of the two hospitals are given: they number twenty-three with the secretary. The duty of the directors was chiefly to



check and pay the accounts, and visit the wards, and see that the rules and regulations were duly observed. None of these named suffered; nor do I recollect of any others who suffered from an attack of the disease, who were near the sick; in fact, it seemed as if those who were closest to the diseased, enjoyed the most perfect immunity. The number of resident surgeons of the hospitals during the course of the epidemic, whose term of service varied from six months to two years and upwards, was *six*,—they also escaped. Dr. Smith, my predecessor as chief medical officer, also escaped. All the dispensers (three in number, who did duty as such,) escaped. He who died of the yellow fever never did duty: he died the same evening he was gazetted to his office. All the stewards, eight in number, escaped. It is difficult even to guess at the number of nurses exposed, for they were frequently changed; they must have been very numerous: by a memorandum of Mr. W. B. Pollard, the financial accountant (and I prefer official and public documents to any others as vouchers) in 1842, the year of the second maximum of intensity of the epidemic, the average number of nurses, exclusive of other servants in daily attendance in the Seaman's Hospital, was seven, and in the Colonial Hospital fifteen. Washing is, and always has been done for the Colonial Hospital by contract: the contractor is Mr. Friday Hopkinson. I do not insert any communication from him in the 'Appendix,' as, although a man of very considerable property in houses and land, he can neither read nor write. Before 1840, Mr. Hopkinson contracted for the clothes-washing of both hospitals; and, besides the assistance of his own family, required eight people constantly at work. After 1840, the women were employed separately for the Seaman's Hospital, and then only six assistants beside his own family were required by him, and the same numbers respectively continue till now. The individual washers were often changed during the period of the epidemic, but none of them were ever affected by the prevalent disease. I hope I have now stated enough to rectify any looseness of manner with which my facts on the contagion-question has been charged by the reviewer.

"The reviewer calls on me to substantiate the character which I gave to the letter of Dr. W. Fraser to Sir Andrew Halliday, which appeared in the 'London Medical Gazette,' and was replied to by the late Dr. Ferguson in the same periodical on the 24th of March, 1838. It is easily done. I denounced his material statements in support of the importation and contagiousness of yellow fever as *defective, hypothetical, or fictitious*. His first material statement is, that the disease commenced in Surinam, was transported thence to Barbadoes, and from Barbadoes imported into Demerara. With Surinam it little concerns us, although I have written to Baron Van Rader, the governor, to obtain some information, in consequence of being informed by Mr. Van Ononeller, of the B. G. Bank, that he was in Surinam in 1836 (the date given by Dr. Fraser), and yellow fever did not exist there then. Dr. Fraser does not state the exact date of its introduction from Barbadoes. He does not state in what person or bale of goods, or by what vessel it came. Where is the *corpus delicti*? Until the public and the profession here were startled by almost accidentally discovering Dr. Fraser's letter twelve months after it was written, and twenty months after the epidemic had existed, no one had ever heard of *importation* as an imputed cause. Dr. Fraser had never trusted the secret to any one here, neither the public, his professional friends, nor the government, whose health-officer he was. About the time of the outbreak of the new pestilence, we find, in the government secretary's office, numerous quarantine communications regarding smallpox, but not one word about yellow fever, the danger of its being imported from Barbadoes, or that it already existed from neglect of quarantine regulations. Now, these are fatal defects unless they can be supplied or rectified by Dr. Fraser. Full explanations are the more necessary, as it happens, that while our earliest cases in the epidemic could not in any likelihood have been earlier than the 1st of April, 1837, no yellow fever existed in Barbadoes during any part of that year. It is worthy of remark, in connection with this subject, that although an immense intercourse exists between this colony and Barbadoes, and was not for a single day interrupted during the last lamentable devastation in

1847-8, of which you have had experience, we have been perfectly free from that scourge since 1845.

"The second material statement of Dr. Fraser is, that the disease was propagated by contagion; and he illustrates this by the cases of Messrs. Wray and Howes, the first who died of the disease in Berbice, who evidently contracted it in Demerara, but who had no known communication with the person or apparel of the sick; the family of whom did not become affected, according to the testimony of the medical attendants, and whose cases, according to the same authority, had no relation with any of the subsequent cases in Berbice, except that of priority. In further illustration of its propagation by contagion, he asserts, that Mr. (now Sir Robert Schomburgk), the traveller, caught the disease by lying in a bed lately occupied by a young man who died of black vomit; that the Indian attendant caught the disease of Schomburgk, and his retinue carried the epidemic into the "sylvan villages" of the interior. Now, this is pure unmitigated fiction. Robert Schomburgk never had yellow fever, although his brother Richard had twelve or eighteen months afterwards. He never slept in the bed or in the same room in which the young man Cherrington had died. His Indian attendant never had the disease; and the disease never extended amongst his retinue, nor to the other Indians of the interior, although a few Indians of the sandhills, who contracted it in town, died of it there, and the disease died with them. This exquisite fiction, about "poor Schomburgk," was retracted and apologised for by Dr. Fraser to Mr. Stutchley, in presence of Schomburgk, when he next returned from the interior. What was Sir Robert Schomburgk's own opinion on the subject? In his 'Description of British Guiana,' published in 1840, he states that 'immediate contact with the sick did not propagate the disease (yellow fever), nor did seclusion diminish it; and there is no instance known in which it extended to the more elevated places of the interior.' I cannot make out who the lady was, and what the mercantile firm which Dr. Fraser refers to, which was shut up for a couple of weeks in consequence of the whole establishment being laid up by the prevailing malady. The case is very defective in being anonymous. It is singular that, in the *beginning* of the epidemic, they should all recover in that house.

"We come now to the *hypothetical*. 'A ship,' says Dr. Fraser, 'arrives, and the captain uses every precaution to avoid having any communication with the shipping or shore. This precautionary sort of measure does very well for a week or a fortnight; but at the expiration of that period, the rigorous restrictions he has placed himself and crew under become relaxed, and they steal a visit to see some of their old messmates, who may perhaps be at the time very ill with fever. The visitor sits, has a long yarn with the visited, he returns to his ship,' &c. &c. Now, this is not only hypothesis, but impossible hypothesis, as any one who knows anything of this harbour could at once detect. The ships swing out in the river at cable length,—the stream runs at the rate of three or four miles an hour; the river abounds with sharks; no approach to either ship or shore can be effected except by the boats, which are entirely under the power of the officers of the ships. In fact, each ship is perfectly isolated, and remains so during the voyage; and whatever yarns the messmates may have with each other, they can have none with their neighbours except through a speaking trumpet. Again, Dr. Fraser says: 'An overseer of an estate comes to town with produce to be shipped. He visits some friend's house where the fever is raging. He returns to the estate, sickens and dies, and his fellow overseers go through a similar ordeal, having caught the infection from him. From this point the fever spreads itself to every family in the district.' This is a mere fancy sketch, no real instance of which I am sure could be adduced. It is not only not true as a generalization, but the very reverse of truth. Dr. Fraser had no country practice during the time of the epidemic, and as I am sure no medical practitioner could have furnished him with materials for the description, he must have drawn entirely on his imagination. Many overseers and others who came to Water street became affected on their return home; but the character of the cases was to have no issue, on lateral offshoots. The epidemic seemed, in all cases, to have, as it were, dropped down into a family; and having struck its victim, darted off to pounce

down in the neighbourhood, or at a great distance. If Dr. Fraser, instead of speculating on fanciful sketches of the progress of the epidemic in the rural districts, where he had no personal experience, and I feel confident could have had no trustworthy correspondents, had observed the habits of the disease from his own house, he might, in a great measure, have steered clear of his gross errors. On the right-hand house there was a field for observation, on the left another, under his feet another, and before his eyes another. I, who was his left-hand neighbour, took the disease: besides having a nurse and my own servants constantly in the house, I had relays of batchelor friends, who every night watched by me and tended me. But no extension of the disease occurred in consequence. The next victim in the range was in the house to his right. Mr. M'Dougal, a young Scotch barrister, came up from Leguan, where he had been on a visit, caught the disease, and died. Nobody else in that house (Sir Michael M'Turks') became affected. Next, his (Dr. Fraser's) turn came in the centre: he had had no intercourse with either parties, and was to windward of me: his wife was not aware of his doctrine of contagion, or did not regard it, and was unremittingly attentive; his son was there; his servants did not fly away; he recovered, and nobody else in his house sickened of the disease afterwards: and his neighbour opposite, Dr. Smith, who attended all the three cases, never was attacked. Dr. Fraser goes on to state something more tangible than his hypothetical cases: he states that 'every member of the medical profession, who from their avocations were necessarily exposed to the infection, have been attacked, and undergone a most severe fit of sickness; none of them, however, have died.' This is fiction except the last portion of the sentence. The medical men were exceedingly harassed, but there was not much actual sickness of any kind amongst them: indeed, if we except Dr. M'Nish, who, I believe, was laid up for three months from dry bellyache during the period referred to by Dr. Fraser, I question much if the medical men generally were ever freer from sickness. The two cases of yellow fever in town were Dr. Fraser and myself. I can account for my own without the aid of contagion. The case in the country, Dr. Reid's, has been already referred to in the monograph. Dr. Fraser goes on to remark: 'The mortality has been great amongst the clergy.' This is quite true. But the inference he would draw is not. Here, again, those *most* exposed, suffered *least* from the malady, and *vice versa*. Those, the metropolitan clergy, whose avocation was amongst the stricken and the susceptible, passed through the ordeal least harmed; and those whose congregations were chiefly of the negro race, such as London missionaries and those of the rural parishes of the Scotch church, suffered severely. This will be seen by an enumeration of the deaths within the period referred to in Dr. Fraser's and Sir A. Halliday's letters. They were:

Messrs. Wray and Howes . . .	died	} London Missionaries.
Mrs. Rattray . . .	"	
Mr. Markland . . .	recovered	
" Williams . . .	died	} Plymouth Brother?
" Bryan . . .	"	
Dr. Hardy . . .	"	} 1st Minister of Christ Church.
Mr. Stewart . . .	"	
" Haig . . .	"	} Scotch Church.
" M'Intosh . . .	"	
" Mr. Forbes . . .	recovered	
" Stevenson . . .	"	
" Anderson . . .	"	
" Menzies . . .	"	

"Now, of all these cases, let Dr. Fraser point out one that had its origin in contagion; or, with a single exception, one that was talked of, as being suspected in the remotest degree of being concerned in the propagation of the disease. But turn we to those whose duties called them most to the bedside of the sick and the funerals of the dead, to the clergyman of St. George's church (now the cathedral), to the clergyman of St. Andrew's church in George Town, to him of Christ church, also in

George Town (whose predecessor lived only a few weeks after his arrival here); these clergymen minister to almost all the white inhabitants of the city. Mr. Fox, besides his other duties, had, during the whole epidemic, since the foundation of the Seaman's Hospital, solemnized the service of the church of England every Sunday to the convalescents of that hospital. Mr. Sugur's labours had been unremitting; and he has had his nephew, Mr. Austin, (then lately arrived,) ill of the graver form of the disease, in his house. Dr. Struther was peculiarly exposed by his ministerial duties among the multitude of Scotch Presbyterians who inhabit the mercantile part of the city; in addition to this exposure, he had at various times yellow fever cases introduced into his house; the Rev. Mr. Hay's case was one; the Rev. Mr. Stevenson's another; in a third instance the captain of the 'Duke of Gordon,' Mr. Longman, was brought to his house, and there remained throughout a most severe illness and protracted convalescence. Dr. Struther's family at that time consisted of thirteen persons. Now neither of these clergymen, nor any of the inmates of their house, ever contracted the disease.

"The Bishop of Guiana, not now in the colony, had a large family of children, and a large retinue of servants; two of the latter (whites recently arrived) died of yellow fever; but the disease never spread in the house. During the presence of the disease in the colony, the bringing of the sick into the midst of families was of usual occurrence; no fear was felt; there was no difficulty in obtaining nurses; no danger was incurred. Several years after my own illness, I brought a favorite white servant, who was very ill, to the room next my own bedroom, that I might see him two or three times during the night. My brother-in-law, Dr. Manget, did the same with one of his servants. There was nothing remarkable or heroic in this; it was a mere matter of course, and practised by dozens without remark or laudation. I have not communicated, you will observe, in the appendix, either with the Roman Catholic pastors, or those of the Wesleyan persuasion. The reason is, that none of either denomination who saw the epidemic, are now in the colony. But as far as I recollect, the Wesleyan clergymen had no deaths amongst them, and only one priest died of yellow fever, although the service of this church brought them frequently among the Portuguese immigrants in the Colonial Hospital. The priest who died was considered at the time as having inhaled the malaria in crossing the Demerara river in an open boat after night.

"I have now done with Dr. Fraser's most extraordinary letter. It may be thought strange, and it is so, that no one save an anonymous correspondent of the 'Guiana Chronicle,' took the trouble to expose and refute a production so unfounded at the time when it was discovered. That *exposé*, too, never saw the light; no doubt for some personal reason of the editor, either of ill-will to the assailant or friendship for the assailed. Colonial apathy, which in many things disregards the opinion of 'home,' operated likely as a sedative. When all disbelieved it here, where was the use of trouble merely to set the profession in England right! Compassion for the old gentleman who had so terribly committed himself, I know, moved one individual to forbearance. Some despised the production, although endorsed by Sir Andrew Halliday; for there is no use in concealing the circumstance, that, although Dr. Fraser was an old practitioner in the colony, he never stood high either with the public or the profession. Neither did Sir Andrew Halliday's character stand high in this community. Here he was celebrated chiefly for his credulity, on which some of the wags of the colony cruelly played, and for his patronage of quack medicines. However the neglect of the profession may be accounted for, Dr. Fraser's narrative remained without public contradiction from the faculty. None of the chief practitioners seemed to regard the effect of Dr. Fraser's letter abroad, where the true circumstances were unknown, nor to care for it, except staff-surgeon Hackett; and his hands soon became too full of his own special duties to allow time for extraneous work.

"I think the reviewer is hardly fair or correct in his estimate of the treatment described in the monograph. He says there is nothing new in it, and that it has been tried five hundred times, and that it is founded on a crazy hypothesis long since exploded. I suppose he refers to Dr. Chisholm's mercurial treatment, which

also was extensively tried and given up as useless here, when he inveighs against calomel. But the treatment recommended, I submit, is a very different affair. It is founded on no hypothesis whatever. It is purely empirical, and became established from the surprisingly beneficial effects found to follow its adoption. I should be sorry, indeed, if the remarks of the reviewer could deter any medical man who has had to encounter such fevers as are epidemic or endemic in this colony from trying the plan recommended. The making of our medical men acquainted with the power of the combination of calomel and quinine in *large doses* in febrile diseases, I look upon as a considerable compensation for the ravages of our late epidemic. The physician here now wields a power over the most formidable disease such as he never before possessed, and with results as striking as the feats of the surgeon.

"Calomel is very much disliked in this colony. This repugnance to the mineral was inherited from our predecessors the Dutch, who considered mercury should never be given except in venereal disease, and existed long before the modern and improved views of the use of that medicine obtained in England. Influenced by this prevalent dislike to mercurials here, and from the ill effects that no doubt frequently follow the excessive use and abuse of them, I have sometimes been tempted to try a small dose of the compound or a reduced proportion of the calomel, but I have seldom failed to regret the experiment. The formula became completely established for the treatment of yellow fever; and since the departure of that disease from amongst us, it has been most successfully extended to all the intensified forms of our endemic intermittent, except the congestive.

"I quite agree with the reviewer as to that oversight of not giving the number of observations, and that it materially diminishes the value of the table which proposes to show the influence of *age* on the mortality of yellow fever. I admit also that considerable improvement might be effected in the mode of grouping the *post-mortem* appearances by the numerical method. But I do not think that the candid inquirer can be at any loss in knowing how many livers were examined when told that ninety-seven post-mortem examinations were made. Louis' anatomical character of yellow fever was looked for on every occasion. I imagine that the discrepancy which the reviewer affirms to exist between your description of the spleen and mine in the fatal cases of yellow fever is no discrepancy, but a significant and harmonious coincidence, and helps to explain the difference of the results of treatment by some difference in the constitution of the disease.

"In regard to the severe remarks which the reviewer makes on the military authorities about the site of the military hospital, it is possible they are undeserved. Might not the shifting of the fore shores have materially altered the sanitary condition of the site. It is long since I read Sir Andrew Halliday's 'Notes on the West Indies;' but if my memory serves me right, he then describes (1833 or '34) that the lately built hospital was within *fifty yards of the sea*. If my inference is correct, then it is highly probable that its site was determined on, when no such marsh as that described in 1840 existed; and that the authorities are not open to the measure of reprobation heaped on them by the reviewer.

"Excuse the length to which this letter has extended,—called for by the challenge of the reviewer. If any explanations or amplifications are required on any point, I shall be ready to afford them. Believe me to be, my dear Sir,

"Ever yours, faithfully,

"D. BLAIR."

The documents alluded to in this letter, and which we have not room to insert, are the following:

1. An unpublished letter (dated Nov., 1838,) from Mr. Sturschburg to the editor of the 'Medical Gazette,' contradicting the account given by Dr. Fraser of the illness of Mr. Schomburgk, and of the conveyance of the disease to the Indian villages.
2. A letter from Mr. Sturschburg (dated November 15, 1850,) to Dr. Blair, explaining why the above letter had never been sent.
3. A letter from Mr. Rattray to Dr. Blair, stating that Mrs. Rattray died of yellow fever, but that nothing like contagion was traceable in her case.



4. A letter from the Rev. Archdeacon James Sugar to Dr. Blair, stating that he had never known any instance of contagion.

5. An examination of the newspaper of 1847, from which it appears that abundant remonstrances were made on the sanitary condition of George-town, but that no suspicion had ever been entertained that the disease was imported.

In conclusion, we have no hesitation in saying that Dr. Fraser's communication to the 'Medical Gazette,' to which we referred in our article on Dr. Blair's book, is worthless as evidence on the question of contagion, and should not be used by any one. The statements contained in Dr. Blair's letter may probably be considered by us at some future opportunity.

#### HOSPITAL FOR SICK CHILDREN.

WE are happy to see that a vigorous effort is now being made for the establishment of a Hospital for Sick Children,—a measure on which we have several times insisted as of fundamental importance to the scientific prosecution of Infantile Pathology and Therapeutics; and we trust that this effort may be crowned with the success it deserves. Although the immediate advantages of the Institution must be confined to the Metropolis, yet it cannot but exert, in our estimation, a most favorable influence on the entire profession, in affording opportunities for Medical Observation which can be obtained in no other way; and we venture to claim for it, therefore, the sympathy and co-operation of our readers generally. There are some points in the proposed Constitution, on which we may take a future opportunity of remarking. It reached us, however, at too late a period to admit of our doing so at present.

#### ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

THE events of the late Election of Officers have fully justified the remarks we have felt ourselves called on to make in the present number of our Review (pp. 362-4), in regard to the management of this Society, and especially in reference to the impropriety of making a plurality of appointments from the staff of any one hospital. Three of the principal offices in the Society are now filled by gentlemen selected from a single institution; and a fourth member of the Council is closely connected with the same establishment. There was much opposition to this nomination, out of doors; and although the house-list was carried by a large majority, the Council may feel assured that many of the most distinguished among the Fellows voted for it, simply because they feared to originate disunion in the Society, and not because such wholesale methods of appointment meet with their approval. It is not easy, perhaps not possible, to please everybody; but *it is easy* to avoid such a glaring mistake as this, and henceforth we hope that the Council will give no occasion for such remarks as it has now been our painful duty to make.

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The Anatomist's Vade-Mecum. A System of Human Anatomy. By Erasmus Wilson, F.R.S. Fifth Edition. London, 1851. Fcap. 8vo, pp. 656.

Remarks on Insanity: its Nature and Treatment. By Henry Monro, M.B. London, 1851. 8vo, pp. 158.

The Philosophy of Vital Motion. By Charles B. Radcliffe, M.B. London, 1851. 8vo, pp. 158.

Physiology of Human Nature; being an investigation into the Physical and Moral Condition of Man, in his relations to the Inspired

Word of God. By Robert Cross, M.D. London, 1851. Post 8vo, pp. 367.

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The Pharmacopœia of the King and Queen's College of Physicians in Ireland. Dublin, 1850. 8vo, pp. 190.

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**Phthisis and the Stethoscope. A concise Practical Guide to the Physical Diagnosis of Consumption.** By Richard Payne Cotton, M.D. London, 1851. Fcap. 8vo, pp. 97.

**An Essay explanatory of the Tempest Prognosticator.** By George Merryweather, M.D. London, 1851. 8vo, pp. 63.

**A Practical Treatise on Diseases of the Urinary and Generative Organs.** By William Acton. Second Edition. London, 1851. 8vo, pp. 693.

**On the Causes, Symptoms, and Treatment of Spermatorrhœa.** By M. Lallemand. Translated and Edited by Henry J. McDougal. Second Edition. London, 1851. 8vo, pp. 411.

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**A Letter to the Right Hon. Sir G. Grey, on some of the Social Relations of the Medical Profession.** By George Robinson, M.D. Newcastle-on-Tyne. 1850. 8vo, pp. 28.

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**and recommended by the Commissioners appointed under a Resolve of the Legislature of Massachusetts, relating to a Sanitary Survey of the State.** Boston (N.E.), 1850. 8vo, pp. 544.

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# INDEX TO VOL. VII

OF THE

## BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.

	PAGE		PAGE
Absorption by chyloferous vessels, M.		Bubo, fatal peritonitis from . . .	268
Bernard on . . .	527	Burnett's fluid, poisoning by . . .	384
of poisons by plants . . .	532	Bushnan, Dr., his address to students	231
Acupuncture in varix . . .	545	on cholera, and its cures . . .	1
Albuminous urine, Dr. Bence Jones on	388	on the New Cattle	
Dr. G. Johnson on . . .	375	Market . . .	526
in scarlatina . . .	498	Cadge, Mr., his surgical anatomy . . .	518
Alcoholismus chronicus, Dr. Huss on	41	Cæcum, inflammation of . . .	345
Alcoholic liquors, use and abuse of . .	46	Calcia, excision of, Mr. Page on . . .	382
America, North, diseases of . . .	302	Cancer of intestinal canal . . .	354
Anderson, Mr., on eccentric nervous		Canstatt, Dr., his practice of medicine	333
affections . . .	189	Carnochan, Dr., on dislocation of head	
Anus, artificial, case of . . .	382	of femur . . .	358
Aperient solution, Prof. Mettauer's . .	549	Carpenter, Dr., on alcoholic liquors . .	46
Apoplexy, Dr. Copland on . . .	189	Cattle-market, new, Dr. Bushnan on	526
Aran, M., on muscular atrophy . . .	538	Cautery, use of, in sciatica . . .	541
on chloroform in lead colic	535	Cazenave, M., on lupus . . .	258
Arsenic, elimination of . . .	281	Cerebral substance, M. Orfila on . . .	280
Atrophy of spinal muscles . . .	259	Cerise, M., on nervous affections . . .	535
muscular, M. Aran on . . .	538	Chambers, Dr. T. C., on corpulence . .	524
Bath waters, Dr. Tunstall on . . .	429	Chatin, M., on iodine in fresh-water	
Baths, Dr. Bell, &c. on . . .	429	plants . . .	530
Basham, Dr., his case of stricture of		Chemistry, animal, Dr. Bence Jones on	460
the œsophagus . . .	374	Prof. Graham's elements of	240
Baudens, M., on ingrowing toe-nail . .	267	Liebig and Kopp's report	
Bazin, M., on the treatment of itch . .	258	on . . .	525
Beaumont, Mr., his case of disarticu-		Children, diseases of . . .	107
lation of the lower jaw . . .	381	hospital for . . .	561
Bednar, Dr., on the diseases of chil-		Cholera, epidemic, in relation to sani-	
dren . . .	107	itary measures . . .	1
Bell, Dr., on Baths . . .	429	Dr. Thompson's chemical re-	
Bernard, M., on absorption by vena		searches on . . .	372
portæ . . .	527	Cholesterine in synchysis . . .	267
on Woorara poison . . .	531	Chorda tympani, functions of . . .	528
on the functions of the		Chorea and heart-disease, relation of	258
liver . . .	251	Chyle, nature of . . .	527
Bladder, hernia of . . .	267	Chylous urine, composition of . . .	529
gunshot wound of . . .	378	Clarkson, Mr., his case of intestinal	
Blair, Dr., on yellow fever . . .	555	obstruction . . .	370
Blasius, Prof., on operative surgery . .	285	Clergy, duration of life amongst . . .	552
Bowman, Mr., his case of hæmatocele	379	Clitoris, abnormal conformation of . .	548
Brachet, M., on lead colic . . .	41	Colic, lead, M. Brachet on . . .	41
Breasts, inflamed, application of collo-		of prostitutes . . .	245
dion to . . .	547	Collodion, abortive power in smallpox	279
Bright, Dr. James, on diseases of the		application of, to inflamed	
chest . . .	167	breasts . . .	547
Bright's disease, pathology of . . .	375	Colon, diseases of . . .	347
Brodie, Sir B., on diseases of joints . .	177	Coma, Dr. Todd on . . .	189
Bronchus, case of foreign body in . . .	364	Consumption, nature and cure of . . .	167

	PAGE		PAGE
Cooking and preserving of meat, Prof.		Forbes, Dr., his introductory lecture	231
Liebig on . . . . .	532	Forbes, Mr., on foreign body in bron-	
Copland, Dr., on palsy and apoplexy .	189	chus . . . . .	364
Copper, health of workmen employed		Froriep, Dr., on electro-magnetism .	455
in . . . . .	282	Gangrene of Intestine . . . . .	356
Corpulence, Dr. Chambers on . . . .	524	Gastritis . . . . .	336
Coup de soleil . . . . .	534	Genitals, female, malformation of .	548
Crystalline lens, dislocation of . . .	545	fatal hæmorrhage from . . . . .	552
Curling, Mr., his case of hæmatocele	379	Glass, fragments of, in orbit . . . .	271
his cases of absence of		Gout, Dr. Toulmin on . . . . .	249
thyroid body . . . . .	386	Graham, Prof., his Elements of Che-	
Curare-poison, experiments on . . . .	531	mistry . . . . .	240
Cyanosis, case of . . . . .	260	Gregory, Dr., his Conspectus . . . .	246
Dalrymple, Mr., his pathology of the		Hæmatemesis . . . . .	337
human eye . . . . .	242	Hæmatocele, cases of . . . . .	379
Dampness, influence of, on spread of		Hæmorrhage, fatal, from female geni-	
cholera . . . . .	27	tals . . . . .	552
Deafness, Mr. Harvey on . . . . .	517	Hannon, M., on elimination of arsenic	281
Mr. Yearsley on . . . . .	520	Happiness, Dr. Forbes on . . . . .	231
Delirium, Dr. Todd on . . . . .	189	Hassing, Dr., on the colic of prostitutes	245
Delusions, Medical, Dr. Hooker on . .	519	Heart, disease of, and chorea, rela-	
De Morgan, Mr., on section of tendo		tion of . . . . .	258
achillis . . . . .	366	wound of . . . . .	269
Depilatory effect of sulphuret of soda	279	fatty diseases of . . . . .	376
Digestive system, diseases of . . . .	333	Hemiplegia, Mr. Dunn's case of . . .	390
Dislocation of vertebra . . . . .	269	Henry, Mr. Mitchell, his case of ab-	
of head of femur . . . . .	358	scess of vesicula seminalis . . . .	387
of crystalline lens . . . . .	545	Hernia of bladder . . . . .	267
Dissection, Mr. Holden's Manual of .	241	Higginbottom, Mr., on nitrate of silver	521
Dorvault, M., on iodine compounds .	91	Holden, Mr., his Manual of Dissection	241
Drake, Dr., on diseases of North		Holland, Dr. Calvert, on consumption	167
America . . . . .	302	Hooker, Dr., his Physician and Patient	519
Druitt, Mr., his Surgeon's Vade-Mecum	250	on medical delusions . . . . .	ib.
Dunn, Mr., his case of hemiplegia . .	390	Hospital gangrene, iodine in . . . .	549
Duodenum, inflammation of . . . . .	344	for children . . . . .	561
Duration of life among clergy . . . .	552	Howard, Mr., on the eye . . . . .	115
Dysentery . . . . .	351	Huss, Dr., on alcoholismus . . . .	46
Eggs, Mr. Paget on freezing of . . . .	256	Hydrocele of the neck . . . . .	266
Electro-physiological researches, Prof.		Ileus, case of complicated . . . . .	386
Matteucci's . . . . .	254	Infants, weight of new-born . . . .	276
Electro-magnetism, therapeutic uses		Infantile pneumonia, &c., M. Trou-	
of . . . . .	455	seau on . . . . .	277
Erysipelas after vaccination . . . .	537	Inflammation, Paget, &c. on . . . .	127, 472
Estlin, Mr., his Eye Dispensary . . .	520	Insanity, M. Morel on . . . . .	265
Exostosis, subungual . . . . .	270	Intermittent fever, temperature in .	535
External iliac, case of ligature of . .	268	Intestine, cancer of . . . . .	354
Extra-uterine pregnancy, case of . . .	273	gangrene of . . . . .	356
Eye, Mr. Howard on . . . . .	115	Intestinal obstruction, cases of . . .	370
Mr. Dalrymple's pathology of . . . .	242	Introductory lectures . . . . .	231
Eye Dispensary, Bristol, report of . .	520	Inunction, Mr. Taylor's treatment by	513
Færoe Islands, sanitary state of . . .	419	Iodine in hospital gangrene . . . .	549
Fat, excess of, Dr. Chambers on . . . .	524	Iodine in fresh-water plants . . . .	530
Fatty diseases of heart . . . . .	376	Iodognosy, M. Dorvault on . . . .	91
urine, Dr. Bence Jones on . . . . .	388	Itch, treatment of . . . . .	258
Femur, dislocation of head of . . . .	358	Jaw, disarticulation of . . . . .	381
Field, Mr., his case of intestinal ob-		Jenner, Dr., on typhoid and typhus	
struction . . . . .	370	fever . . . . .	367
Flandin, M., on poisoning by oxide of		Johnson, Prof., his case of cyanosis .	260
zinc . . . . .	282	Dr. G., on pathology of kid-	
Foot-prints, solidification of . . . .	554	ney . . . . .	375

	PAGE		PAGE
Johnson, Dr. Howard, on baths . . . . .	429	Mollities ossium, case of . . . . .	378
Joints, diseases of, Sir B. Brodie on . . . . .	177	Morel, M., on insanity . . . . .	265
Jones, Mr. Wharton, on Inflammation . . . . .	127	Morton, M., his surgical anatomy . . . . .	518
Dr. Bence, on albuminous and		Mütter, Prof., on hydrocele of neck . . . . .	266
fatty urine . . . . .	388	Navy, issue of spirits in . . . . .	74
on chylous urine . . . . .	529	Neill, Dr., on the shape of the thyroid	
on animal chemistry . . . . .	460	foramen . . . . .	254
Kidney, pathology of . . . . .	375, 498	Nervous affections, M. Cerise on . . . . .	535
Kilian, Prof., on degeneration of the		Dr. Todd, &c. on . . . . .	189
placenta . . . . .	272	Nitrate of silver, uses of . . . . .	521
Labour, mechanism of . . . . .	211, 393	Œsophagus, case of stricture of . . . . .	374
Lactation, M. Trousseau on . . . . .	274	inflammation of . . . . .	333
Langenbeck, Prof., on ophthalmia . . . . .	543	Oils, volatile, quantities of, yielded by	
Lankester, Dr., his introductory lec-		plants . . . . .	548
ture . . . . .	231	Olfactory nerve, case of paralysis of . . . . .	528
Lassaigne, M., on cerebral substance . . . . .	280	Oophoritis, Dr. Pistocchi on . . . . .	546
Lawrance, Dr., his translation of Fro-		Ophthalmia, Professor Langenbeck on . . . . .	543
riep on electro-magnetism . . . . .	455	Orbit, fragments of glass in . . . . .	271
Lead colic, M. Brachet on . . . . .	41	Orfila, M., on cerebral substance . . . . .	280
treatment of by chloroform . . . . .	535	Ovarian inflammation, Dr. Pistocchi on . . . . .	546
Lead, diseases from, in Paris . . . . .	534	Overcrowding, influence of on spread	
Lectures, introductory . . . . .	230	of cholera . . . . .	7
Lee, Dr. R., on the use of the speculum . . . . .	382	Ozone, observations on . . . . .	540
on tumours of the uterus . . . . .	383	Page, Mr., on excision of os calcis . . . . .	382
Lee, Mr., on the baths of Germany . . . . .	429	Paget, Mr., his case of open urachus . . . . .	385
Letheby, Dr., his case of poisoning by		on inflammation . . . . .	127, 472
Sir W. Burnett's fluid . . . . .	384	the freezing of eggs . . . . .	256
Liebig, Prof., his report on chemistry . . . . .	525	Palsy, Dr. Copland on . . . . .	189
on the cooking and pre-		Panum, Dr., on the Færoe Islands . . . . .	419
serving of meat . . . . .	532	Papillæ of tongue, morbid changes in . . . . .	261
Liver, M. Bernard on the functions of . . . . .	251	Paralytic affections, application of	
Lupus, M. Cazenave on . . . . .	250	electro-magnetism in . . . . .	455
Macintyre, Dr., his case of mollities		Parturition, Dr. Millar on . . . . .	211, 393
ossium . . . . .	378	Penis, luxation of . . . . .	269
Maclachlan, Dr., on abscess of medias-		Peritonitis, fatal, from bubo . . . . .	268
tinum . . . . .	ib.	Petit, Dr., on Vichy waters . . . . .	415
Maclise, Mr., his surgical anatomy, 242, 523		Phthisis, incipient signs of . . . . .	389
Macpherson, Mr., on gun-shot wound		curability and treatment of . . . . .	167
of the bladder . . . . .	378	Placenta, degeneration of . . . . .	272
Matteucci, Prof., his researches . . . . .	254	Plants, absorption of poisons by . . . . .	532
Measles, epidemic of, in Færoe islands . . . . .	419	Pneumonia of infants . . . . .	277
Meat, Prof. Liebig on preparation of . . . . .	532	Poisoning by chloride of zinc . . . . .	384
Mediastinum, abscess of . . . . .	378	bromine . . . . .	551
Medical and Chirurgical Society, elec-		corrosive sublimate . . . . .	553
tion at . . . . .	561	peach-blossoms . . . . .	ib.
strictures on . . . . .	362	rose-bay . . . . .	ib.
Medicine, French and German practice		Porta, Professor, on the thyroid gland . . . . .	96
of . . . . .	333	Pregnancy, extra-uterine . . . . .	273
Medico-Chirurgical Transactions . . . . .	364	Premature labour, induction of . . . . .	546
Medicines, scale of, for merchant ves-		Prostitutes, colic of . . . . .	245
sels . . . . .	522	Quain, Dr. R., on fatty diseases of	
Meissner, Dr., on the literature of		the heart . . . . .	376
pædiatrics . . . . .	107	Quinine, substitutes for . . . . .	550
Mettauer, Prof., his aperient solution . . . . .	549	Quintuple birth, case of . . . . .	547
Mialhe, M., on the tartrate of potass		Rainey, Mr., on the round ligament of	
and iron . . . . .	278	the uterus . . . . .	256
Millar, Dr., on parturition . . . . .	211, 393	Rectum, structure of, case of . . . . .	282
Miller, Prof., his principles of surgery . . . . .	84	Revaccination, in the Prussian army,	
on inflammation . . . . .	127	results of . . . . .	262
Dr., on scarlatina . . . . .	498	M. Craninx on . . . . .	263



	PAGE		PAGE
Rheumatic affections, application of electro-magnetism in . . . . .	455	Thyroid gland, Professor Porta on . . .	96
Rheumatism, acute, termination of in suppuration . . . . .	264	Todd, Dr. on delirium and coma . . .	189
Round ligament of the uterus, Mr. Rainey on . . . . .	256	Toe-nail, ingrowing, operation for . .	267
Sandras, M., on nervous diseases . . .	189	Tongue, papillæ of, morbid changes in	261
Sanitary Reform, in relation to cholera	1	Tonsil, excision of, Mr. Harvey on . .	517
Scarlatina, Dra. Millar and Tripe on . .	498	Mr. Yearsley on . .	520
Schroeder, Van der Kolk, on alcoholic liquors . . . . .	46	Toulmin, Dr., on gout . . . . .	249
on the sputa of phthisis	257	Transposition of arterial trunks . . .	260
Sciatica, use of cautery in . . . . .	541	Tripe, Dr., on Scarlatina . . . . .	498
Simon, Mr., on inflammation . . . . .	127, 472	Trousseau, M., on Lactation . . . . .	274
Skey, Mr., his operative surgery . . . .	285	Tunstall, Dr., on Bath waters . . . .	429
Smell, sense of, case of loss of . . . .	528	Turnbull, Dr., on consumption . . . .	167
Smith, Dr., on terebinthinate medi- cines . . . . .	248	Typhoid and typhus fever, Dr. Jenner on . . . . .	367
Soap-lees, case of poisoning by . . . .	374	Urachus, persistent opening of . . . .	385
Solidification of foot-prints . . . . .	554	Urine, albuminous, Dr. Bence Jones on . . . . .	388, 529
Solly, Mr., his case of ileus . . . . .	386	Dr. G. Johnson on . . . .	375
Speculum, use of, Dr. K. Lee on . . . .	382	Dr. Miller on . . . . .	498
Spina bifida, spontaneous cure of . . .	268	Uterine disease, on the use of the speculum in . . . . .	382
Spinal marrow, atrophy of . . . . .	259	Uterus, round ligament of, Mr. Rainey on . . . . .	256
Spirits, issue of, in the Navy . . . . .	74	Vaccination, M. Craninx on . . . . .	263
Sputa of phthisis, elastic fibres in . .	257	erysipelas after . . . . .	537
Stomach, diseases of . . . . .	336	Vade-Mecum of Surgery, Mr. Druitt's	250
Subungual exostosis, case of . . . . .	270	Valleix, Dr., his practice of medicine	333
Sulphuret of soda as depilatory . . . .	279	Venables, Dr., on urinary analysis . .	460
Surgery, operative, Mr. Skey on . . . .	285	Ventilation, insufficient, influence of on spread of cholera . . . . .	7
Prof. Blasius on . . . .	ib.	Varix, treatment of by acupuncture . .	545
principles of, Prof. Miller's . .	84	Vermiform appendix, gangrene of . . .	537
Surgical anatomy, Mr. Morton's . . . .	518	Vertebrae, dislocation of . . . . .	269
Suppuration in acute rheumatism . . . .	264	Vesicula seminalis, abscess of . . . .	387
Symonds, Dr., his introductory lec- ture . . . . .	231	Viability, case of early . . . . .	547
Tartrate of potass and iron . . . . .	278	Vichy waters, therapeutic action of . .	415
Taste, sense of, dependent on smell . .	528	Volatile oils, quantities of yielded by plants . . . . .	548
Taylor, Mr., on inunction . . . . .	513	Ward, Mr., his Introductory Lecture	231
Temperance and total abstinence . . .	46	Water, supply of to the metropolis . .	247
Temperature in intermittent fever . . .	535	cold, external use of . . . . .	429
Tendo-Achillis, section of, in fracture of leg . . . . .	366	Waters, Vichy, action of . . . . .	415
Terebinthinate Medicines, Dr. Smith on . . . . .	248	Bath, action of . . . . .	429
Thomson, Dr. Spencer, on alcoholic liquors . . . . .	46	Weight of new-born infants . . . . .	276
Dr. R. D., his chemical re- searches on cholera . . . . .	372	Wells, Mr. Spencer, on scale of me- dicines . . . . .	522
Dr. Theophilus, on expi- ratory murmur . . . . .	389	West, Dr., his Introductory Lecture	230
Thyroid foramen, shape of . . . . .	254	Woorara-poison, experiments on . . .	531
gland, cases of absence of . .	386	Wunderlich, Dr., his Practice of Me- dicine . . . . .	333
		Yearsley, Mr., on Deafness . . . . .	520
		Yellow fever, Dr. Blair on . . . . .	555
		Zinc, oxide of, poisoning by . . . . .	282

END OF VOL. VII.





